If you refer to this thesis, please cite the published article versions of the chapters:
Chapter 2 was published in Social Studies of Science, vol 28, 2, April 1998, p. 291-323; chapter 3 in Environmental Management, vol. 22, 4, 1998, p.597-609; chapter 4 in American Journal of Science, 296 (8), 1996, p.932-953. If you would like to receive copies of the published papers please write me at j.p.vandersluijs@uu.nl and check my websites for recent publications: www.jvds.nl and www.nusap.net

Anchoring amid uncertainty

**On the management of uncertainties
in risk assessment of anthropogenic climate change**

Houvast zoeken in onzekerheid

Over het omgaan met onzekerheden
in risicoanalyse van klimaatverandering door menselijk handelen

(met een samenvatting in het Nederlands)

Proefschrift

Ter verkrijging van de graad van doctor
aan de universiteit utrecht,
op gezag van de rector magnificus Prof. dr. J.A. Van Ginkel,
ingevolge het besluit van het college van Decanen
in het openbaar te verdedigen op
maandag 21 april 1997 om 16.15 uur.

door

Jeroen Pieter van der Sluijs

geboren te Herwijnen in 1965

Promotoren: prof.dr. Josee C.M. van Eijndhoven
prof.dr. Wim C. Turkenburg

Beoordelingscommissie: prof.dr.ir. P.J.H. Builtjes (Utrecht University)
prof.dr. P. Vellinga (Free University of Amsterdam)
prof.dr. W.C. Clark (Harvard University)
prof.dr. J. Goudriaan (Agricultural University Wageningen)
dr. J. Ravetz (The Research Methods Consultancy Ltd., London)

Wij zijn maar als de blaren in den wind
ritselend langs de zoom van oude wouden,
en alles is onzeker, en hoe zouden
wij weten wat alleen de wind weet, kind -

(Adriaan Roland Holst, Zwerversliefde,
Groot Nederland II, **9**, 1913, p. 580)

Aan Gemma

Contents:

List of acronyms, abbreviations, and chemical symbols

1. **General Introduction**
    - 1.1 Introduction
    - 1.2 Anthropogenic climate change
        - 1.2.1 Temperature and the radiation balance
        - 1.2.2 The natural greenhouse effect
        - 1.2.3 The enhanced greenhouse effect
        - 1.2.4 Climate change
    - 1.3 Short history of the climate change issue
        - 1.3.1 Research on anthropogenic climate change
        - 1.3.2 Climate risk assessment
        - 1.3.3. Climate change policy development
        - 1.3.4 Controversies on global warming
    - 1.4 Basic notions
        - 1.4.1 Climate risk assessment and post normal science
        - 1.4.2 Uncertainty as a social construct
    - 1.5 Objective, research questions and outline of this thesis
    - 1.6 References

2. **Anchoring Devices in Science For Policy: The Case of Consensus Around Climate Sensitivity**
    - 2.1 Introduction
    - 2.2 Science for policy
    - 2.3 The CO_2 doubling temperature: a history of sticking to the same numbers
        - 2.3.1 The concept of the 'climate sensitivity'
        - 2.3.2 The construction of the estimate for climate sensitivity
        - 2.3.3 The 'best guess' and the uncertainty of the climate sensitivity range
        - 2.3.4 Different meanings and functions of climate sensitivity
        - 2.3.5 Multiple functions and uses of the climate sensitivity
        - 2.3.6 The 1.5°C to 4.5°C temperature range as *anchoring device* in the climate debate
    - 2.4 Conclusion and discussion
    - 2.5 Acknowledgements
    - 2.6 Notes

3 **Closure of Disputes in the Assessments of Climate Change in the Netherlands Arena**
    - 3.1 Introduction
    - 3.2 The closure time lines
    - 3.3 The construction of the Gezondheidsraad assessments
        - 3.3.1 The Construction of the Gezondheidsraad estimate for climate sensitivity
        - 3.3.2 The CO_2 focus of the Gezondheidsraad
    - 3.4 Discussion and Conclusion
    - 3.5 Acknowledgements
    - 3.6 References

4 **Biogenic Feedbacks in the Carbonate-Silicate Geochemical Cycle and the Global Climate**
    - 4.1 Introduction
    - 4.2 The carbonate-silicate geochemical cycle

4.3 The model
4.4 Results
4.5 Conclusions and discussion
4.6 Acknowledgements
4.7 References
appendix A The 0-dimensional temperature model

5 **Possibilities and Limitations of Integrated Assessment Models for the Climate Issue**
5.1 Introduction
5.2 The emergence of IAMs as a science-policy interface
5.3 What are IAMs?
5.3.1 Definitions of IAMs
5.3.2 Variability in IAM-modelling practice
5.4 Key uncertainties and limitations faced by IAMs of the climate issue
5.4.1 Culture and Values
5.4.2 Demands for goods and services
5.4.3 Choice of technologies and practices
5.4.4 Fluxes of material in the environment
5.4.5 Valued Environmental Components
5.4.6 Exposure
5.4.7 Consequences
5.5 The usefulness and use of IAMs for the climate issue
5.5.1 The policy-usefulness of climate IAMs
5.5.2 The context of use of IAMs
5.6 Conclusions
5.7 Acknowledgments
5.8 References

6 **Integrated Assessment Models of Climate Change and the Management of Uncertainties**
6.1 Introduction
6.2 Classifications of uncertainty
6.3 Addressing uncertainty due to inexactness
6.3.1 Uncertainties in input data and model parameters
6.3.2 Uncertainties regarding conceptual model structure and technical model structure
6.3.3 Uncertainties regarding model completeness
6.4 Addressing unreliability: quality control in IAM practice
6.5 Addressing ignorance
6.5.1 Reducing ignorance through research, a paradox
6.5.2 The modelling of surprise
6.6 Areas for improvement in uncertainty management
6.7 Disentangling the uncertainty problem: adding the quality dimension
6.8 Conclusions
6.9 Acknowledgements
6.10 References

7 **Major conclusions of this thesis**

Summary

Samenvatting

Curriculum Vitae

Research publications

Nawoord

List of acronyms, abriviations, and chemical symbols

AAAS	American Association for the Advancement of Science
AGGG	Advisory Group on Greenhouse Gases
AIM	Asian-Pacific Integrated Model
ASF	Atmospheric Stabilization Framework
ASTM	American Society for Testing and Materials
BLAG	Long-term carbon cycle model by Berner LAsaga and Garrels
CCC	Canadian Climate Center
CCN	Cloud Condensation Nuclei
CCOL	Coordination Committee on the Ozone Layer
CETA	Carbon Emission Trajectory Assessment
CFCs	Chlorofluorocarbons
CH_3CCl_3	Methychloroform
CH_4	Methane
CH_2Cl_2	Dichloromethane
$CHCl_3$	Chloroform
CIM	Coordination Committee Concerning International Environmental Affairs (The Netherlands)
CLW	Cloud Liquid Water
CO	Carbon Monoxide
CO_2	Carbon Dioxide
CoP	Conference of Parties (to the Climate Convention)
CRMH	Central Council for Environmental Hygiene (The Netherlands)
CSERGE	Center for Social and Economic Research into the Global Environment
DGM	Directorate General for Environmental Protection (VROM, NL)
DICE	Dynamic Integrated Climate Economy
DMS	Di Methyl Sulphide
EHEP	Experimental High Energy Physics
ENSO	El Niño Southern Oscillation
ESCAPE	Evaluation of Strategies to address Climate change by Adapting to and Preventing Emissions
EPA	Environmental Protection Agency (USA)
ESM	Earth System Model
FCCC	see UNFCCC
FUND	The Climate Framework for Uncertainty, Negotiation, and Distribution
GCAM	Global Change Assessment Model
GCM	General Circulation Model (also: Global Circulation Model)
GCTE	Global Change and Terrestrial Ecosystems
GDP	Gross Domestic Product
GEOCARB	GEOchemical CARBoncycle model by Robert Berner
GFDL	Geophysics Fluid Dynamics Laboratory
GHGs	GreenHouse Gases
GISS	NASA Goddard Institute of Space Studies
GNP	Gross National Product
CSIRO	Commonwealth Scientific and Industrial Research Organization (Australia)
CSGC	Carbonate-Silicate Geochemical Cycle
HDGEG	Human Dimensions of Global Environmental Change
IAM	Integrated Assessment Model
ICAM	Integrated Climate Assessment Model
ICMH	Interdepartmental Committee Environmental Hygiene (NL)
ICSU	International Council of Scientific Unions
IEA	Integrated Environmental Assessment

IGBP	International Geosphere Biosphere Programme
IGY	International Geophysical Year
IIASA	International Institute for Applied Systems Analysis
IMAGE	Integrated Model to Assess the Greenhouse Effect
IMP	Indicative Multi-year Program on Air (NL)
INC	International Negotiating Committee (on the FCCC)
IPCC	Intergovernmental Panel on Climate Change
IPCC WGI	Intergovernmental Panel on Climate Change, Working Group I
IS92a-f	IPCC Scenario *a* up to and including scenario *f*, as defined in the 1992 IPCC report
ISSC	International Social Science Council
ISM	Integrated Science Model for assessment of climate change
KNMI	Koninklijk Nederlands Meteorologisch Instituut (Royal Netherlands Meteorological Institute) (NL)
LASOM	National Steering Group Environmental Research (NL)
LMD	Laboratoire de Météorologie Dynamique (France)
LOS	Norwegian Research Centre in Organisation and Management
MAB	Man and the Biosphere program
MAGICC	Model for the Assessment of Greenhouse gas Induced Climate Change
MARIA	Multiregional Approach for Resource and Industry Allocation
MARKAL	Market Allocation
MBIS	MacKenzie Basin Impact Study
MCS	Monte Carlo Simulation
MCW	Model of Global Warming Commitment
MERGE	Model for Evaluating Regional and Global Effects of GHG Reductions Policies
MIT	Massachusetts Institute of Technology
MiniCAM	Mini Climate Assessment Model
MPI	Max Planck Institute for Meteorology
N_2O	Nitrous Oxide
NAPAP	National Acid Precipitation Assessment Program (USA)
NAS	National Academy of Sciences (USA)
NCAR	National Center for Atmospheric Research (USA)
NGO	Non-Governmental Organization
NH_3	Ammonia
NO_x	Nitric Oxide (NO) and/or Nitrogen Dioxide (NO_2)
NRP	Netherlands National Research Programme on Global Air Pollution and Climate Change
NUSAP	Numeral Unit Spread Assessment Pedigree notational system
NWO	Nederlandse Organisatie voor Wetenschappelijk Onderzoek (Netherlands Organization for Scientific Research) (NL)
O_3	Ozone
OECD-GREEN	Organization for Economic Co-operation and Development GREEN model
PAGE	Policy Analysis of the Greenhouse Effect
PAGES	PAst Global changES (part of IGBP)
PC	Personal Computer
PEF	Policy Evaluation Framework
ppmv	Parts per million by volume (a measure of concentration)
ppbv	Parts per billion by volume (a measure of concentration)
ProCAM	Process Oriented Global Change Assessment Model
RAINS	Regional Acidification INformation and Simulation
RH	Relative Humidity
RICE	Regional Integrated Climate Economy (regionalized version of the Nordhaus' DICE model)
RIVM	Rijksinstituut voor Volksgezondheid en Milieuhygiene (National Institute of Public

	Health and Environmental Protection) (NL)
RMNO	Raad voor het Milieu- en Natuuronderzoek (Advisory Council for Research on Nature and Environment) (NL)
SAR	IPCC's Second Assessment Report
SBSTA	Subsidiary Body for Scientific and Technological Advice
SCOPE	Scientific Committee on the Problems of the Environment
SF	Stability Factor
SF_6	Sulfur Hexafluoride
SMIC	Study of Man's Impact on Climate
SO_2	Sulfur dioxide
SSK	Sociology of Scientific Knowledge
TARGETS	Tool to Assess Regional and Global Environmental and Health Targets for Sustainability
UK	United Kingdom
UKMO	United Kingdom Meteorological Office
ULYSSES	Urban LifestYles, SuStainability, and Environmental aSsessment
UN	United Nations
UNEP	United Nations Environmental Programme
UNESCO	United Nations Educational, Scientific, and Cultural Organisation
UNFCCC	United Nations Framework Convention on Climate Change
US	United States of America
VEC	Valued Environmental Component
VOC	Volatile Organic Compound
VROM	Ministerie van Volkshuisvesting, Ruimtelijke Ordening en Milieuhygiene (Netherlands Ministry of Housing, Physical Planning and the Environment) (NL)
WAIS	West Antarctic Ice Sheet
WCRP	World Climate Research Programme
WMO	World Meteorological Organization
WRI	World Resources Institute
WRR	Wetenschappelijke Raad voor het Regerings Beleid (Scientific Council for Government Policy (NL)
YSSP	IIASA Young Scientist Summer Program

1. General Introduction

1.1 Introduction
1.2 Anthropogenic climate change
1.2.1 Temperature and the radiation balance
1.2.2 The natural greenhouse effect
1.2.3 The enhanced greenhouse effect
1.2.4 Climate change
1.3 Short history of the climate change issue
1.3.1 Research on anthropogenic climate change
1.3.2 Climate risk assessment
1.3.3 Climate change policy development
1.3.4 Controversies on global warming
1.4 Basic notions
1.4.1 Climate risk assessment and post-normal science
1.4.2 Uncertainty as a social construct
1.5 Objective, research questions and thesis outline
1.6 References

1.1 Introduction

Over the past few decades, several attempts have been made to assess to what extent man-made emissions of greenhouse gases may change the Earth's climate. This thesis consists mainly of a critical analysis of the processes by which the climate issue has been assessed over that period. We focus particularly on the question of uncertainty management in climate risk assessment. Assessments serve as a scientific basis for the climate policy debate. The overall scientific objective of this dissertation is to gain insight into the processes by which assessments of the risks of anthropogenic climate change are constructed and more specifically into the way in which uncertainty management is conducted within these processes. This insight can help us to identify ways of managing uncertainties in the assessments better and ways of strengthening the role of assessment as a frame of reference acceptable to actors in the policy process.

Assessment is the analysis and review of information derived from research for the purpose of helping someone in a position of responsibility to evaluate possible actions or think about a problem. Assessment usually does not mean doing new research. Assessment means assembling, summarizing, organizing, interpreting, and possibly reconciling pieces of existing knowledge, and communicating them so that they are relevant and helpful for the deliberations of an intelligent but inexpert policy-maker (Parson, 1995).

Experts started drafting assessment reports on climate change for policy-makers when research into anthropogenic climate change, and especially climate modelling, was still in an early stage of development. The first notable assessments of the climate problem date from the seventies. Climate research, and especially climate modelling, has expanded enormously during the last few decades. Consequently, successive assessments have had to deal with new insights, theories and data. This history, and the prevalence of large scientific and epistemologic uncertainties regarding future climate, mean that anthropogenic climate change is an issue which can be used effectively for investigating the science-policy interface and the processes that have helped to construct a shared body of scientific knowledge that acts as scientific basis for the climate policy debate.

Assessment draws upon information from research. Climate research programmes were developped in parallel with the emergence of assessment. Initially, climate research programmes aimed at the reduction of the uncertainties in climate forecasting (WCRP, 1979, IGBP, 1992). The belief in the feasibility of this objective was so strong that the Intergovernmental Panel on Climate Change (IPCC) stated in their 1990 report that they *"are confident that the uncertainties can be reduced by further research*[1]", they were referring to the uncertainties about sources and sinks of greenhouse gases, cloud formation, oceans and ice sheets. IPCC’s 1995 Second Assessment Report is still dominated by the belief in the reducibility of uncertainties and the ultimate ’do-ability’ of long-term climate prediction, in spite of a growing awareness among the research communities involved that further research will not necessarily reduce the overall uncertainties regarding future climate. There are some uncertainties about particular aspects of the climate system and its dynamics which have been reduced. However, ongoing research is also revealing unforeseen complexities in the climate system and novel uncertainties, which increase the uncertainty of which we are already aware.

For that reason, the IGBP (International Geosphere Biosphere Programme), one of the largest international research programmes on global change, concluded during their third Scientific Advisory Council Meeting in January 1993, that it might not be feasible to reduce uncertainties (Williamson, 1994). Williamson also notes that the increasing complexity of global models inevitably decreases the precision of their products and *"full predictability of the earth system is almost certainly unattainable."* He makes a case for the replacement of the research objective to reduce uncertainties by a pragmatic research goal to *"provide reliable estimates of probability within defined limits, so that risks can be assessed and appropriate actions taken, rather than single value ’predictions’ with spurious exactitude."* On the subject of the research objective of reducing uncertainties Bolin (1994) says: *"We cannot be certain that this can be achieved easily and we do know it will take time. Since a fundamentally chaotic climate system is predictable only to a certain degree, our research achievements will always remain uncertain. Exploring the significance and characteristics of this uncertainty is a fundamental challenge to the scientific community."*

Abarbanel *et al.* (1991) and Tennekes (1994) also stress that the predictability of the long-term climate is limited because of the (partly) chaotic nature of the climate system. Further, there are unresolvable limits to the reduction of scientific uncertainties,

---

1 They added *"However, the complexity of the system means that we cannot rule out surprises."*

e.g. the epistemological limits of science, our limited capacity to know and understand, limits on our capacity to handle complexity, and computer limitations (these issues will be discussed in detail in chapter 5). These circumstances make the assessment of future anthropogenic climate change and its impacts an extremely difficult and partly impossible task.

The scientific basis for the climate policy debate has elements with a scientific status that varies from well-established knowledge to judgements and educated guesses. This is reflected in the formulations used by the Intergovernmental Panel on Climate Change (IPCC) to express different levels of certainty in their claims (IPCC, 1990):

"We are certain of the following . . . "
"We calculate with confidence that . . . "
"Based on current model results, we predict . . . "
"Our judgement is that . . . "

Another aspect of the science-policy interface is that the experts who carry out the assessments address questions that can be stated in scientific terms, but are either in principle or in practice beyond the proficiency of science to answer. An example of such a question is the one raised by the ultimate objective of the United Nations Framework Convention on Climate Change (Article 2): *"stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system."* The question of what stabilization level would prevent dangerous interference cannot be answered by science alone. What happens then is that the experts often express their own political judgement and beliefs.

Consequently, we have to be aware of the possibility that claims based on conceptual computer models, expert-judgements, and beliefs expressed by experts go beyond the competence of present-day science. Often, such claims are at best educated tentative speculation.

The climate policy debate, including the negotiations on the United Nations Framework Convention on Climate Change, builds upon the scientific basis provided by the assessment community. In that context, it is important to understand the limits of science for policy advice and to gain insight into the scientific status of the knowledge-claims produced by the assessment community. To enhance this understanding, we need better insight into the assessment practice, uncertainties in the assessments and the management of these uncertainties. To investigate these phenomena, this thesis makes use of insights from domains in which questions were studied concerning the relations of science and policy, the phenomenon of scientific expertise, and the construction processes of knowledge claims. These domains belong to the field of science and technology studies (Jasanoff *et al.*, 1995), philosophy of science (Funtowicz and Ravetz, 1990) and global environmental risk research (The Social Learning Group, forthcoming).

In section 1.2 of this introduction chapter we will briefly discuss the state of our knowledge about the risks of anthropogenic climate change. In section 1.3 we sketch the history of how the issue of anthropogenic climate change emerged. After that, in section 1.4 we discuss basic notions that served as a starting-point for our analysis. In section 1.5

we discuss the objective and the research questions of this thesis and present the further outline of this thesis.

1.2 Anthropogenic climate change

The natural heat-trapping effect of greenhouse gases in the atmosphere forms the core of the theory of anthropogenic climate change. To explain this phenomenon, we will first briefly describe the energy radiation balance which to a large extent determines the climate on earth. Then we will discuss the natural and the enhanced greenhouse effect.

1.2.1 Temperature and the radiation balance

The ultimate source of energy which drives the Earth's climate is the absorption of solar radiation[1]. Any object with a temperature above 0 K emits energy by electromagnetic radiation. According to Stefan-Boltzmann's law, the energy emitted (Q, in Watt m^{-2}) by an object is proportional to the fourth power of its temperature (T, in K):

$$Q = \sigma T^4$$

where σ is Stefan-Boltzmann's constant. Its experimental value is 5.66961 x 10^{-8} W m^{-2} K^{-4}. The distribution of the emitted energy over the spectrum of electromagnetic radiation is described by the Planck distribution. The normalized Planck-distributions or 'black-body curves' for the sun and for the Earth are presented in Figure 1.1.
The wavelength at maximum emission (λ_{max}) decreases with increasing temperature following Wien's displacement law:

$$T \lambda_{\mathrm{max}} = k_W$$

where k_W is Wien's constant. Its experimental value is 2.8978 x 10^{-3} m K.

The sun has an effective temperature of about 6000 K. The resulting λ_{max} of the sun is 480 nm, which is in the blue-green part of the visible light. This means that most of the energy emitted by the sun is short-wave radiation. The Earth's atmosphere is transparent for short wave radiation, so the part that is not reflected by clouds reaches the

1 There are a few other sources of energy, such as the heat flux from the inner earth, and energy from cosmic radiation. However, they can be neglected because they contribute less than 1 % of the total energy input to the earth system (Sagan and Mullen, 1972). Thermal pollution by human energy consumption is another source. Given that the world energy use is about 400 EJ yr^{-1} and that the radius of the Earth is 6400 km, the globally averaged direct energy flux from human energy consumption amounts to 0.025 W m^{-2}, which means that it is negligible on the global scale. Thermal pollution can however have local climate effects in industrial and urban areas.

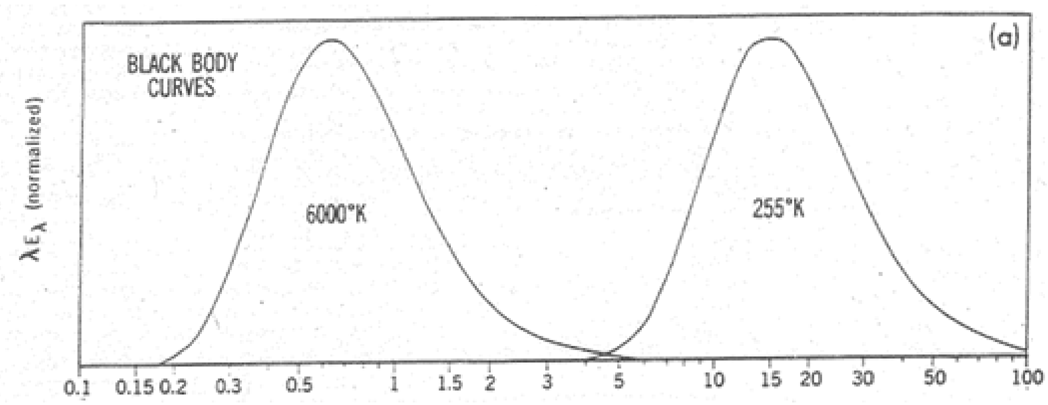

Figure 1.1 Normalized Planck distributions for the solar radiation (assumed to have a temperature of 6000 K) and the terrestrial radiation (assumed to have a temperature of 255 K) (Peixoto and Oort, 1995).

Earth's surface almost unhindered. The Earth's surface reflects part of the incoming solar radiation. The rest is absorbed and therefore warms the surface. According to Stefan-Boltzmann's law, a warm Earth surface emits energy by electromagnetic radiation. If absorption of radiation by the surface and emission of radiation by the surface were the only energy exchanges that take place, it can simply be calculated from the albedo (indicating the fraction of the incoming radiation that is reflected) of the earth (0.3) and the solar luminosity (342 W m^{-2} averaged over the earth surface) that thermodynamic equilibrium between energy absorption and energy emission would correspond to a surface temperature of 255 K (-18°C). According to Wien's displacement law, λ_{max} for an object with a temperature of 255 K is 11,400 nm, showing that most of the energy emitted by the Earth's surface is long-wave radiation in the infrared part of the spectrum.

1.2.2 The natural greenhouse effect

The atmosphere is an important component of the climate system. Without the atmosphere, the global mean temperature at the Earth's surface would be about - 18°C, whereas it is about + 15°C today. The 33°C difference is caused by the radiation from the atmosphere back to the earth's surface. The main source of energy for the atmospheric back-radiation is absorption of long-wave radiation (emitted by the earth's surface) by the natural atmospheric greenhouse gases such as H_2O, CO_2, N_2O, CH_4 and O_3. Greenhouse gases are gases that absorb infrared radiation. For instance, CO_2 has strong absorption bands in the spectral interval from 12,500 to 17,500 nm. About 20% of the energy emitted by the Earth's surface is emitted in this interval. H_2O is an even stronger greenhouse gas and clouds also strongly absorb infrared radiation. Consequently the atmosphere warms up and, following Stefan-Boltzmann's law, re-emits energy in all directions. Part of the energy is emitted to the Earth's surface, causing additional warming.

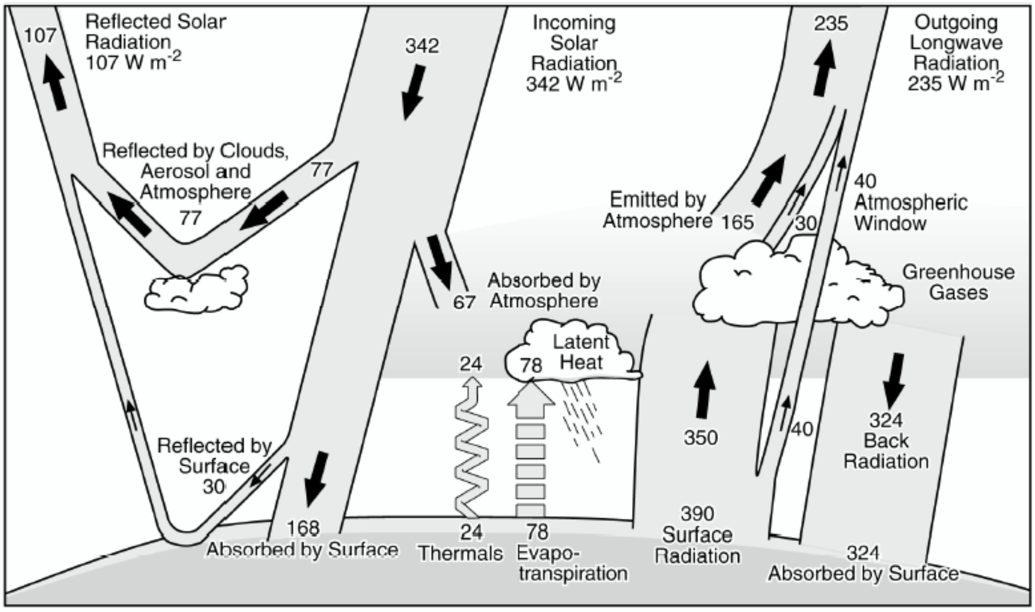

Figure 1.2 Schematic representation of the globally averaged Earth's radiation and energy balance. All fluxes are in W m^{-2} (Houghton *et al.*, 1996).

Other heat exchange processes between the earth surface and the various layers of the atmosphere are convection and latent heat transport. Their contribution to the globally averaged energy balance is shown in Figure 1.2.

In summary, the greenhouse effect results from the radiative properties of greenhouse gases; they are transparent for the short wave radiation of the sun, but have strong absorption bands for long-wave infrared radiation emitted by the Earth's surface and the atmosphere. The absorbed energy is re-emitted, partly back to the earth surface. This mechanism forms a heat-trap in the lower atmosphere.

1.2.3 The enhanced greenhouse effect

Since pre-industrial times (which is defined as *the several centuries preceding 1750*, Houghton *et al.*, 1996), the CO_2 concentration has increased from 280 ppmv to 358 ppmv in 1994, CH_4 has increased from 700 ppbv to 1721 ppbv, N_2O has increased from 275 ppbv to 311 ppbv, whereas halocarbons have been added to the atmosphere as a new component. These increases have caused a perturbation of the energy balance at the earth's surface, relative to the energy balance for a pre-industrial atmospheric composition. This perturbation in the energy balance is called the *radiative forcing*, and is expressed in Watts per square metre. The radiative forcing of the climate system caused by anthropogenic increase in greenhouse gas concentrations since 1880 constitutes an *enhanced greenhouse effect*. At present, the radiative forcing caused by the increased concentrations of anthropogenic greenhouse gases amounts to 2.45 W m^{-2}, with an estimated uncertainty of 15% (all figures from Houghton *et al.*, 1996).

In addition to the anthropogenic greenhouse effect, there are also other factors that could cause a radiative forcing of the climate system. Examples are the solar cycles, volcanic dust emissions, and the emissions of aerosols, soot and aerosol-precursors such as SO_2. Aerosols influence the radiation balance by reflecting solar radiation back to space and by absorbing and emitting radiation. Further, they have an indirect radiative effect in that they influence the optical properties and lifetime of clouds. Aerosols act as Cloud Condensation Nuclei (CCN). Given a same amount of water vapour available for cloud formation, increases in CCN would create clouds with more (but smaller) droplets, and therefore a larger total reflection surface (Charlson *et al.*, 1987, Taylor and Penner, 1994). This indirect effect of aerosols on the radiation balance has been poorly quantified. The atmospheric lifetime of aerosols is relatively short. Therefore, their concentration is not well mixed over the globe. Its effect is mainly regional in the vicinity of aerosol emission sources. Locally, the negative forcing by anthropogenic aerosols can be larger than the positive forcing by the enhanced greenhouse effect.[1]

1.2.4 Climate change

The forcing due to anthropogenic increases in greenhouse gas concentrations is - together with the forcing from other factors - imposed upon the very complicated coupled geosphere-biosphere-climate system. This system contains many interlinked non-linear feedback loops, acting on different temporal and spatial scales. There are both positive and negative feedbacks. Positive feedbacks amplify the initial warming, negative feedbacks reduce it. An example of a positive feedback is the water vapour feedback. The amount of water vapour in the atmosphere increases if the Earth warms. Because water vapour is a strong greenhouse gas, it will amplify the warming. However, an increase in

---

[1] On a global scale, the enhanced greenhouse effect is believed to dominate (Houghton *et al.*, 1996).

the amount of clouds will both amplify the warming by the greenhouse effect and reduce the warming by the increase in albedo. Which of the effects dominates in the cloud feedback is still subject to scientific debate. The ice-albedo feedback is another example of an important mechanism in the climate system which may amplify warming in the high latitudes because decreases in snow and ice coverage decrease the albedo. However, where sea-ice melts, the increased open water may increase fog and low-cloud amount, offsetting the change in albedo. Apart from these geophysical feedbacks, there is a range of biospheric feedbacks via the carbon cycle and other biogeochemical cycles which are closely coupled to climate variables. Our understanding of these feedbacks is insufficient to quantify the effects of many of them. Even the sign of some of the proposed feedbacks is unknown (Houghton *et al.*, 1996). Therefore there are many uncertainties in the assessments of the effect of greenhouse gas emissions on the climate.

Further, the climate system exhibits natural variability on virtually all spatial and temporal scales. Our scientific understanding of the phenomenon of natural variability is also incomplete and is characterized by huge uncertainties and unresolved scientific puzzles.

At the same time, it is obvious that climate change caused by the enhanced greenhouse effect can have a severe impact on societies and ecosystems. Examples of adverse effects are flooding, shifting climate zones, changes in agricultural production, extinction of species, changes in ecosystems, loss of biodiversity, changes in human migration patterns, changes in geographical distribution of diseases (such as malaria and schistosomiasis), storm damage, and effects on water supply.

In conclusion, the greenhouse theory of climate change is plausible and its core is formed by well established physics. The inherent uncertainties constrain our competence to establish reliable estimates of the size and timing of the risks involved in climate change caused by human behaviour. The awareness that anthropogenic climate change is plausible and can have severe effects on societies and ecosystems has given rise to an urgent demand for assessment and quantification of the risks associated with anthropogenic greenhouse gas emissions.

1.3 Short history of the climate change issue

It is only fairly recently that anthropogenic climate change has been perceived as a problem for society. For a good understanding of the current relation between climate science, policy and society, we need to consider how this relationship developed. In the following, we therefore briefly explore the history of how assessment of anthropogenic climate change emerged and how climate research and assessment became embedded in international institutions.

1.3.1 Research on anthropogenic climate change

The heat-trapping effect of natural greenhouse gases in the atmosphere has been understood since the work of, *inter alia*, Joseph Fourrier (1768-1830), John Tyndall (1820-1893), and Heinrich Gustav Magnus (1802-1870) (Grinevald, 1995, 1996). The possibility of *anthropogenic* climate change as a result of an enhanced greenhouse effect was (probably) first recognized in 1895. In that year the Swedish chemist Svante Arrhenius presented a paper *"On the influence of Carbonic Acid in the Air upon the Temperature of the Ground"* to the Royal Swedish Academy of Sciences. In 1896 an extract from this paper was published (Arrhenius, 1896). Arrhenius calculated the temperature effect of a change in the atmospheric CO_2 concentration by a factor K, varying the value of K from 0.67 to 3.0. Without a computer, he performed calculations of the mean temperature change for latitudes 70°N to 60°S in steps of 10°, for each season. According to his calculations a doubling of CO_2 (K=2) would produce a warming of the Earth's surface (annual mean) temperature of about 4.95° at the equator rising to 6.05°C at 70°N and 5.95°C at 60°S. Arrhenius also recognized that coal-burning in modern industry could affect the atmospheric CO_2 concentration, although this was not the primary theme of his 1896 paper.

In the 1920s, A.J. Lotka was one of the first to frame the large-scale burning of fossil fuels as a disturbance of the global carbon cycle with the potential for far-reaching impacts. On the basis of the rate of coal use in 1920, he calculated that the atmospheric CO_2 concentration would double in a period of 500 years (Kowaloc, 1993). In 1938, the British chemist G.S. Callender stated that the large-scale burning of fossil fuels would lead to an increase in the atmospheric CO_2 concentration and an associated increase in the temperature. Callender's work had an international impact and triggered a scientific debate on anthropogenic climate change (Victor and Clark, 1991; Van der Sluijs, 1992). In October 1949 Callender asserted in the journal *Weather* that the increment in atmospheric carbon dioxide in the period 1900 until 1949 coincided with a general temperature increase on earth. In the same year the Swedish Geophysical Society founded the journal *Tellus*, which was to become an important forum for the reporting of research on anthropogenic (and other) climate change. Bert Bolin[1] was Executive Editor of *Tellus* from 1952-1957, and has been its Editor since 1958 (Grinevald, 1995, 1996).

Starting in 1954 there were annual informal conferences on atmospheric chemistry at the Meteorological Institute in Stockholm (Eriksson 1954, 1955; Mordy, 1957; Neumann, 1958). At these meetings, information on the carbon dioxide issue was exchanged. This included the work by Callender, Bolin, Kaplan and Plass. Plass made laboratory measurements of the absorption in the CO_2 band, and calculated the radiation flux in the atmosphere. His calculations suggested that a doubling of the CO_2

[1] Later, Bert Bolin became a key-figure in the establishment of the IPCC and its precursors. He was chairman of the IPCC from its establishment in 1988 until 1997. He will be succeeded by Robert Watson, who is currently chairman-elect.

concentration would lead to a temperature rise of 3.6 °C (Plass, 1956). Within the framework of these informal conferences it was decided to extend the global CO_2 measurement network.

In 1957 several important seeds were sown which led to further developments and present-day global change research. First, in that year the international research programme "International Geophysical Year" (IGY) started. This can be seen as the first international research programme that considered the earth-system on a global scale. Second, Keeling and others began to make continuous measurements of atmospheric CO_2 variations at Mauna Loa Observatory, Hawaii, which were to provide uncontested evidence that the CO_2 concentration in the earth's atmosphere is continuing to increase. Third, it was the year in which the first satellites were launched and as such the year that marked the beginning of remote sensing of the climate system. Finally, it was the year in which ^{14}C measurements in the deep ocean showed that the vertical transport of CO_2 from the upper ocean to the deep ocean is a very slow process. This implied that the ocean could not be the major sink of CO_2 which some had thought it to be, implying that more anthropogenic CO_2 would remain in the atmosphere than was assumed hitherto. On the basis of this new insight, Roger Revelle and Hans E. Suess of the "Scripps Institute of Oceanography" were the first to calculate that half of the emitted anthropogenic CO_2 remains in the atmosphere. This estimate has not changed significantly since then.

In 1963, F. Möller calculated with a surface energy balance model that a doubling of CO_2 would lead to a temperature increase on earth of 9.6 °C. Möller's model (unrealistically) assumed that relative humidity remains constant upon CO_2-doubling, which added a strong positive feedback to the calculations.

Using a one-dimensional radiative-convective equilibrium model, Manabe and Wetherald from Princeton University's Geophysical Fluid Dynamics Laboratory calculated the change in the vertical temperature profile of the atmosphere for CO_2-doubling. These calculations, published in 1967, suggested that whereas the troposphere would warm, the stratosphere would cool. They calculated a CO_2-doubling temperature of 2.4 °C at the surface. From about this time onwards, it was commonplace to illustrate the sensitivity of climate models in terms of their response to a doubling of the atmospheric CO_2-concentration, not because it was feared that carbon dioxide would double, but because this was a convenient benchmark (e.g. Lanchbery and Victor, 1995).

The measurement programmes which started in the International Geophysical Year were followed by the development of 3-dimensional geographically-explicit physics-based models of atmospheric circulation in the late fifties and of ocean circulation in the sixties. The first coupled Ocean Atmosphere General Circulation Model (GCM) was constructed by Manabe (1969) and Bryan (1969) (Peixoto and Oort, 1995). The GCMs were developed primarily for numerical weather forecasting. The extension of weather-prediction-GCMs to climate-prediction-GCMs closely followed the growing interest in climatology in the late 1960s and 1970s (Lanchbery and Victor, 1995). This growing interest was driven by the issue of deliberate climate modification (Jäger *et al.*, forthcoming). For instance, at the UK Meteorological Office climate-modelling with GCMs began in the late sixties in response to a Ministry of Defence request for predictions of the impact of deliberate modification of climate by an adversary (Shackley *et al.*, 1995). In 1975, Manabe and Holloway were the first to present CO_2-doubling calculations based on a GCM that included a representation of the hydrological cycle. The resulting model was a precursor of the present-day GCMs. It was the first time a model indicated that an intensification of the hydrological cycle would result from a doubled CO_2 concentration.

In the second half of the seventies it was recognized that CO_2 was not the only anthropogenic greenhouse gas. In 1975 V. Ramanathan discovered the greenhouse effect of CFCs (Victor and Clark, 1991). The first statement by the World Meteorological Organization (WMO) on the greenhouse effect of CFCs dates from 26 November, 1975 (WMO, 1975). The significance of the greenhouse effect of anthropogenic CH_4 and N_2O was recognized in 1976 (Jäger and others, forthcoming). It was also known that human activities influenced the atmospheric concentrations of these gases. It took more than ten years for the non-CO_2 greenhouse gases to be included in the assessments (see chapter 3 of this thesis).

In 1978, the glaciologist J. Mercer asserted in *Nature* that CO_2-doubling would occur in 50 years and that the associated climate change at latitude 80°S would be enough to trigger rapid deglaciation of the West Antarctic Ice Sheet (WAIS). Such a deglaciation would lead to a world-wide average sea level rise of 5 metres. This was the first time that anthropogenic climate change was associated with a plausible cataclysmic risk[1]. Mercer's theory gave rise to public concern and to a debate in science on the stability of the WAIS. In the mid-eighties closure was reached in the disputes on the stability of the WAIS: its disintegration was not likely to occur in the 21st century.

In 1986, the ICSU started the International Geosphere Biosphere Programme (IGBP) to investigate global change. This programme's overall objective is: *"to describe and understand the interactive physical, chemical, and biological processes that regulate the total Earth system, the unique environment that it provides for life, the changes that are occurring in this system, and the manner in which they are influenced by human actions."* (ICSU, 1986). A good understanding of the feedbacks between climate, geosphere and biosphere is a prerequisite for climate forecasting.

Several scientific developments in the late eighties and in the nineties have further improved our understanding of the geosphere-biosphere-climate system. The most important developments in this period were the identification of the large-scale thermohaline circulation in the world ocean systems (the so called 'Conveyer Belt') in the late eighties and the notion that anthropogenic climate change could eventually cause this Conveyer Belt to switch to another regime, leading to substantial changes in regional climate on time scales of only a few years (Rahmstorf, 1995).

---

1 A cataclysmic event is an event that totally and irreversibly transforms a system. The issue of cataclysmic risks of climate change very recently became a central issue on the research agenda. In July 1996, the International Institute for Applied Systems Analysis (IIASA) and the Norwegian Research Centre in Organisation and Management (LOS) organized in Laxenburg (Austria) a meeting on "Climate change, cataclysmic risk and fairness".

Major innovations in climate system modelling in the past decade have been the shift from equilibrium modelling towards transient modelling of future change, the inclusion of the aerosol effect in GCMs (Tailor and Penner, 1994), and the development of the 'finger-print method' to detect anthropogenic climate change (Schneider, 1994; Houghton *et al.*, 1996; Santer *et al.*, 1996). The inclusion of aerosols in the models and the development of the finger-print method made it possible to distinguish between anthropogenic and natural climate change. This enabled the IPCC to conclude in the Second Assessment Report that *"The balance of evidence suggests a discernible human influence on global climate"* (Houghton *et al.*, 1996).

1.3.2 Climate risk assessment

Up to and including the sixties, the motivation for anthropogenic climate change research was rooted primarily in scientific curiosity. The research was not driven by a widely-shared concern about adverse impacts of anthropogenic global climate change. We even found a publication in that period which referred to a warming-trend as 'climate betterment'[1] (Labrijn, 1950).

In 1969, the International Council of Scientific Unions (ICSU) established the Scientific Committee on the Problems of the Environment (SCOPE). The mandate of SCOPE is *to assemble, review and assess the information available on man-made environmental changes and the effects of these changes on man (...) to establish itself as a corpus of informed advice for the benefit of centres of fundamental research and of organizations and agencies operationally engaged in studies of the environment* (Bolin *et al.*, 1986). The establishment of SCOPE points to a growing awareness of environmental problems and marks the beginning of the assessment of, and issue-driven (in contrast with scientific-curiosity driven) research on, global environmental problems. This development was further enhanced by the Man and the Biosphere (MAB) programme, established by UNESCO in 1971 (Price, 1992). The emergence of internationally coordinated research programmes, initiated by bodies of the UN and by the ICSU is also the start of the institutionalization of global change research.

The MIT (Massachusetts Institute of Technology) Study of Man's Impact on Climate (SMIC), published in 1971, was the first notable assessment of the climate problem (MIT, 1971). This study was part of the preparations for the 1972 United Nations Conference on the Human Environment, held in Stockholm.

In 1979, the World Meteorological Organisation (WMO) and the United Nations Environmental Programme (UNEP) organized the First World Climate Conference in Geneva. The conference concluded that there was a clear possibility that increases in CO_2 would result in significant long-term changes in climate. The conference was attended by 350 scientists. The conference resulted in the establishment of a World Climate Research

[1] Original language (Dutch:) "klimaatverbetering".

Programme (WCRP), coordinated by the UNEP, the WMO and the ICSU. The purposes of the WCRP were (Kates *et al.*, 1986):
- to improve our understanding of the physical climate system;
- to improve the accuracy and availability of climate data;
- to expand the application of current climate knowledge to human betterment;
- to advance our understanding of the relation between climate and human activities.

In 1979 the US National Academy of Sciences (NAS) 'Ad Hoc Study Group on Carbon Dioxide and Climate', chaired by Jules Charney (from MIT), carried out a comprehensive assessment of the climate problem. The tasks of this group were *"(1) to identify the principal premises on which our current understanding of the question* [of possible future climate changes resulting from man-made CO_2 emissions, JvdS] *is based; (2) to assess quantitatively the adequacy and uncertainty of our knowledge of these factors and processes and (3) to summarize in concise and objective terms our best present understanding of the carbon dioxide/climate issue for the benefit of policy-makers."* (U.S. National Academy of Sciences, 1979). The main result was a range of 1.5°C - 4.5 °C for climate sensitivity[1], based on a critical evaluation of CO_2-doubling calculations with the GCMs then available. As is shown in chapter 2 of this thesis, this temperature range has remained unchanged since then in successive climate risk assessments.

In 1980 The US Energy Security Act mandated the NAS to carry out a comprehensive study. This study resulted in the influential NAS'83 report "Changing Climate" (U.S. National Academy of Sciences, 1983). Both NAS reports had an international impact. They have been quoted by advisory bodies in many countries. They are also quoted in assessment reports by the Intergovernmental Panel on Climate Change (IPCC).

The 1979 World Climate Conference was followed by a series of other meetings sponsored by WMO, UNEP and ICSU, held in Villach in 1980, 1983 and 1985. The 1985 "Conference on the Assessment of the role of Carbon Dioxide and of Other Greenhouse Gases in Climate Variations and Associated Impacts" (Bolin *et al.*, 1986) was in many respects a milestone in the climate debate. It succeeded in bringing together scientists from all over the world to form a panel and was a major step in interfacing international science with national and international policy (Jäger *et al.*, forthcoming).

Although the community of "climate change scientists" was seen mainly as a natural sciences domain, the competence of the social sciences to deal with many important links in the causal chain of climate change began to be recognized in the late eighties. In 1988, the International Social Science Council (ISSC) established a Standing Committee on the Human Dimensions of Global Environmental Change (HDGEC) (Price,

[1] Climate sensitivity can be defined as the equilibrium global mean surface temperature change following a doubling of the atmospheric CO_2 concentration relative to the pre-industrial concentration. For a comprehensive discussion of this concept we refer to chapter 2 of this thesis.

1992). It recognized three fundamental factors driving the interaction between human activities and the global environment: population size and distribution; human needs and desires, as conditioned by psychological, cultural, economic and historical motivations to act; and the cultural, social economic, and political structures and institutions and the norms and laws that shape and mediate human behaviour. The aim of the Human Dimensions programme is to obtain a better understanding of the role of these factors. The increased interest in the human dimensions led in 1990 to the establishment of a new journal: *Global Environmental Change: Human and Policy Dimensions*.

In the second half of the eighties, the Netherlands National Institute for Public Health and the Environment (RIVM) developed the IMAGE model (Integrated Model to Assess the Greenhouse Effect) (De Boois and Rotmans, 1986; Rotmans, 1990) as a new approach for interfacing climate science with policy. IMAGE combines knowledge from a large number of disciplines in one integrated framework and is designed to analyse policy scenarios. Nowadays, Integrated Assessment Models (IAMs) have become a major tool in the assessment of the climate issue. According to Science and Policy Associates Inc. (1995), the IMAGE model has established a niche as a world-leader in integrated systems modelling of climate change. The emergence of IAMs for interfacing science with policy and the ins and outs of their use for this purpose will be discussed in chapters 5 and 6 of this thesis.

The awareness of the risk of severe effects has given rise to an increasingly urgent demand for assessment concerning the climate issue. Policy-makers need to be informed adequately and in good time about the risks involved. At present the Intergovernmental Panel on Climate Change (IPCC, established in 1988 by WMO and ICSU, see section 1.3.3) is attempting to fulfil the task of climate risk assessment[1]. Decision-makers want to know how much climate change we can expect and how this varies with different policy scenarios. Therefore, in 1992 the IPCC designed six different policy scenarios based on different assumptions for population growth, economic growth and emission reduction policy (Houghton *et al.*, 1992). These scenarios are known under the names

---

[1] Lanchbery and Victor (1995) observed that the IPCC fulfils its role as a provider of balanced scientific judgements, but is much less comfortable in its role as an informer of the treaty negotiating process: *"Indeed this is a role that it has never quite accepted. The IPCC is, therefore, always likely to fail to provide timely information for the treaty negotiating process."* For that reason, the Convention established the Subsidiary Body for Scientific and Technological Advice (SBSTA). Since 1995, the SBSTA has formed the link between the policy-oriented needs of the Parties on the one hand, and the scientific, technical and technological assessments and information that various external groups provide on the other. The Intergovernmental Panel on Climate Change (IPCC) continues to function as a prime source of such information (Cutajar, 1995a, 1995b).

IS92a to IS92f[1]. On the basis of a comprehensive analysis, the IPCC 1995 Second Assessment Report projects an accumulated CO_2 emission of 770 GtC (scenario IS92c) to 2190 GtC (Scenario IS92e) for the period 1991 to 2100. The resulting CO_2-concentrations in 2100 are projected to be 490 ppmv (IS92c) to 950 ppmv (IS92e). The increasing CO_2-concentration corresponds to a radiative forcing of 4 W m^{-2} (IS92c) to 8 W m^{-2} (IS92e) in 2100. Taking into account the aerosol effect, the IPCC projects for the year 2100 a range of realized anthropogenic increase of 1°C (scenario IS92c combined with the low estimate of climate sensitivity[2] namely 1.5°C) to 3.5°C (scenario IS92e, combined with the high estimate of climate sensitivity namely 4.5°C) in global mean temperature relative to 1990, and a corresponding sea level rise of 15 cm and 95 cm respectively. According to IPCC, the "best estimates" for the year 2100 are a 2°C temperature increase and a 50 cm sea level rise.

1.3.3. Climate change policy development

In response to the recommendations of the 1985 Villach conference, a small task-force was established by WMO, UNEP and ICSU to ensure an appropriate follow-up: the Advisory Group on Greenhouse Gases (AGGG). The AGGG served until 1988 when it was superseded by the IPCC. The AGGG initiated several follow-up studies and conferences, all aimed at the development of climate policy. Examples are the Villach and Bellagio workshops of 1987. At these workshops, attempts were made to draft ecological standards and policy targets for the climate issue. Another important follow-up of the Villach conference was the 1987 conference in Noordwijkerhout, where scientists and government representatives discussed the consequences of the climate problem for Europe. At the same time, a West European Ministers' conference on climate change was held in Noordwijk, showing that the climate issue had reached the European policy agendas.

In 1988 a conference in Toronto marked the beginning of high-level political debate on the risks of anthropogenic climate change.[3] It recommended a world-wide CO_2 emission reduction of 20% in the year 2005, relative to 1988.

Independent of the Toronto Conference, UNEP and WMO established in 1988 the Intergovernmental Panel on Climate Change (IPCC), chaired by Bert Bolin. According to

---

[1] In the 1995 Second Assessment Report, the IS92 scenarios were updated by taking into account the phasing out of chlorine and bromine containing halocarbons, according to the Adjustments and Amendments to the Montreal Protocol.

[2] The climate sensitivity is the increase in global averaged equilibrium surface temperature for a doubling of the atmospheric CO_2 concentration relative to the pre-industrial concentration. IPCC's low, best, and high estimates for climate sensitivity are 1.5°C, 2.5°C and 4.5°C.

[3] The World Conference on the Changing Atmosphere: Implications for Global Security.

Hecht and Tirpak (1995), the establishment of the IPCC had its roots in a 1985 UNEP long-range planning document that called for a convention on climate change, which was followed by the Villach'85 conference report. Hecht and Tirpak identified the following course of events which resulted in the establishment of IPCC: In response to the Villach'85 recommendations, UNEP's executive director, M. Tolba, sent a letter to the US Secretary of State, G. Schultz, urging the US to take appropriate policy actions. In the policy debate that followed, the mood of senior officials then in Washington was that the underlying scientific evidence for global warming was inconsistent, contradictory and incomplete, and did not justify possibly expensive policy actions. Further, it was felt that the Villach'85 report was inadequate because it had not been prepared by government officials. The idea of a convention was supported, but in the light of the conflicting scientific evidence, it was considered desirable to prepare an intergovernmental assessment. When later presented to WMO and UNEP in the form of intergovernmental resolutions, this assessment resulted in the establishment of the IPCC in 1988.

The IPCC panel consisted of three working groups: Working Group I (WGI) to assess the science of climate change, working group II (WGII) to assess the impacts and working group III (WGIII) to formulate response strategies. In 1990 IPCC WGI issued its first report (hereafter called the IPCC'90 report), a comprehensive state-of-the-art report, with an executive summary for policy-makers. Also, WGII and WGIII issued a report. Hundreds of scientists from all over the world contributed to this report.

At the Second World Climate Conference in Geneva in 1990, the IPCC assessment was accepted by over 137 attendant countries as a vital scientific basis for international negotiations on a climate convention (Jäger and Ferguson, 1991). In December 1990 the UN General Assembly established the International Negotiating Committee (INC) to draw up a framework convention on climate change. The INC was charged with drawing up a convention to be signed by world leaders at the Earth Summit in Rio de Janeiro. The INC drew up this document, but in an incomplete manner, leaving large sections of the United Nations Framework Convention on Climate Change (FCCC) open to different interpretations (Lanchbery and Victor, 1995). Consequently, the INC continued to meet after the Rio conference to further clarify what the Parties should do and how this should be done. The first Conference of Parties (CoP-1) to the Convention was held in March 1995 in Berlin. CoP-2 was held in July 1996 in Geneva. CoP-3 is scheduled to be held in December 1997 in Japan.

The ultimate objective of the FCCC, as formulated in Article 2 of that convention, defined the foci of the assessment community. Article 2 reads as follows:

> *"The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner."*

The FCCC took effect on 21 March, 1994. It has been ratified by 159 countries (figure as of June 1996). The challenge to the climate risk assessment community now is to provide adequate information so that the policy-process can establish what level of stabilization is safe.

Supplements of the IPCC'90 report with updates were issued in 1992 and 1994, and a completely updated state-of-the-art report, the Second Assessment Report (SAR) was completed in 1995, and accepted by CoP-2 in July 1996.

1.3.4 Controversies on global warming

In reaction to the IPCC process, the institutionalization of climate change research, and the development of climate policies, there emerged the 'backlash phenomenon'. It was particularly prominent in the US. One of the most outstanding scientific critics is Richard Lindzen, a well known meteorologist from MIT. Lindzen (1990) has questioned the representation of science in the IPCC reports. Also, he questioned the process by which scientific advice is formulated and the oversimplified public representation of complex and uncertain insights from climate science (see for a more detailed discussion Shackley and Wynne, 1993).

In the US and elsewhere fossil fuel companies are funding research to question the scientific credibility of the IPCC. They have organized themselves in a fossil fuel pressure group called the Global Climate Coalition (Masood, 1996).

A group of scientists who claim to have been excluded from the IPCC process organized themselves in the European Science and Environment Forum (ESEF) that *"will seek to provide a platform for scientists whose views are not being heard, but who have a contribution to make"* (Emsley, 1996). They strongly criticize the 'science by consensus' approach of the IPCC. However, they are mistaken in criticizing IPCC because what IPCC does is not 'science by consensus' but 'assessment'. It can be argued that consensus building is a legitimate approach for doing assessment, although it might not be the best way to cope with pluralism in climate risk assessment.

Apart from criticism on the IPCC assessment process and on the representativeness of the IPCC, there is also scientific criticism. For instance, Lindzen (1990) stressed the possibility of a negative feedback via water vapour[1] not accounted for in the IPCC assessment, which would lead to a an estimate below 1°C for the CO_2 doubling temperature. However, according to Schneider (1990), this negative water vapour feedback is disproved satisfactory[2]. Another example is the criticism by John Emsley, a

---

[1] The negative feedback is: warming leads to deeper cumulus convection, which would dry out the troposphere in the tropics and reduce the infrared heat-trapping capacity of the atmosphere.

[2] The effect is cancelled by a positive feedback of incremental cloud-top height increase, which increases heat trapping.

chemist and member of the ESEF, who claims that the main conclusion of IPCC's 1995 Second Assessment Report, namely that *"the balance of evidence suggests a discernible human influence on the climate"*, is not supported by scientific papers in the report (Masood, 1996). The ESEF issued a book in which almost every link in the IPCC chain of arguments is challenged (Emsley, 1996).

The scientific controversies about global warming make the process of science for policy more complex. However, they also can help to clarify where uncertainties are. One needs insight in the backgrounds to controversies in order to handle the conflicting claims and uncertainties better. Unfortunately, the debate is troubled by the fact that some critics keep putting forward arguments that have been proved to be scientifically untenable in earlier discussions between critics and IPCC contributors. This makes it difficult to distinguish between sense and nonsense in the criticism. Also, the IPCC is subject to thorough peer review procedures, whereas many of the critics pursue the debate in popular news media rather than in peer reviewed scientific journals. By doing so, they shirk the standard quality control procedures in scientific work. The contributions they made and intend to make will be of limited scientific value, unless they are subject to standard peer review. On the other hand, by stimulating a public debate rather than a debate in the scientific arena only, the critics indirectly induce enhancement of the quality control of climate risk assessment by charging the arena in such a way that the actors involved give priority to quality control in order to maintain the legitimacy and authority of their assessments.

1.4 Basic notions

Important trends in the above-sketched history of climate change analysis were a shift from scientific-curiosity-driven towards issue-driven research, an increased demand for assessment of the risks of climate change, and an increased demand for analysis of the policy-meaning of the knowledge and theories about the human influence on climate. Parallel to this trend, we observed the emergent embedding of the research in international programmes and institutions (WMO, UNEP, ICSU) and embedding of the assessment process in global institutional (IPCC) and policy contexts (FCCC and CoP) of the United Nations[1].

The current relation between climate science, policy and society is complex. Scientists have found that anthropogenic emissions of greenhouse gases may have severe effects. This has caused an urgent societal demand for assessment and quantification of the risk of anthropogenic climate change. Unfortunately the many uncertainties about the

1 Sonja Boehmer-Christiansen (1994a; 1994b) claims that these institutional settings have been crucial in the construction process of the assessments. She also claims that these international scientific institutions of concerned scientists acted primarily as a lobby for their own research agendas, dedicated to the modelling of planet Earth and the development of alternative energy sources.

dynamics of the coupled geosphere-biosphere-climate system make science less competent to reliably establish quantitative estimates of the risks. In the following we analyse the ins and outs of this situation, using recent insights and concepts from philosophy of science and from social studies of science.

1.4.1 Climate risk assessment and post-normal science

The science involved in issue-driven risk assessments is in some ways radically different from the science of curiosity-driven classic laboratory practice (Funtowicz and Ravetz, 1992). Assessment of the risks of anthropogenic climate change involves uncertainties of many sorts, not all of which can be effectively controlled in practice. As already noticed by Weinberg (1972), in such a situation the classic mode of analysis in the form of puzzle-solving is unfeasible. To give an example from the climate issue, we cannot perform a statistically satisfying series of experiments to test the effect of higher atmospheric greenhouse gas concentrations, because there is only one Earth available, and even the one available is poorly monitored, and other factors that influence climate are - in contrast to the situation in a laboratory - largely beyond our control. The problem is further compounded by the value-laden context of risk assessment combined with the presence of ineradicable uncertainties and indeterminacies.

Funtowicz and Ravetz (1993) introduced the term *post-normal science* for issue-driven research in a context of hard political pressure, values in dispute, high decision stakes and high epistemological and ethical systems uncertainties. They use the term *post-normal* to indicate that the puzzle-solving exercises of normal science (that is: science in the Kuhnian sense) are no longer appropriate when society is confronted with the need to resolve policy issues regarding trans-national and trans-generational environmental risks. An inventory of typical symptoms of post-normal science as described in the literature is given in Table 1.1.

Climate risk assessment exhibits many symptoms of post-normality. The problem is generally perceived to be urgent and the cost of measures to mitigate global warming could be high. The uncertainties are huge at every step of the causal chain. Usually, the available scientific evidence allows for more than one interpretation. Consequently, science cannot provide ultimate answers. For instance, the observed global warming can be interpreted as an indication of the anthropogenic greenhouse effect, but it can, at least partly, also be interpreted as a consequence of changes in solar output combined with decreased volcanic dust emissions. It can also be regarded as a natural fluctuation of the climate system or even as an artefact of the way the warming is measured and the way globally averaged figures are constructed from an imperfect climate monitoring network, or it can be interpreted as an error in the estimate of the pre-industrial climate. Consequently, one needs to make climate risk assessment transparent in order to understand the background of conflicting interpretations provided by various experts. Such coexisting different interpretations are often perceived as *conflicting certainties* (Stijkel, 1995). In such a situation, values can no longer be taken for granted, but need to

Table 1.1 Symptoms of post-normal science (based upon Funtowicz and Ravetz, 1992, 1993; Nolin, 1995; Stijkel, 1995).

* Research is issue-driven;
* External pressure is exercised on the research groups involved, because associated policy decisions are urgent, decision stakes are high and values are in dispute;
* No single paradigm dominates;
* Complications within scientific venture are confronted and not skirted;
* Research is focused on a whole web of extensive problems;
* Research concerns many large (partly irreducible) uncertainties;
* Conflicting certainties co-exist;
* Scientists are confronted with incomplete control and unpredictability of the analysed system;
* A multitude of legitimate scientific and ethical perspectives co-exist;
* Basic research is transplanted into strategic research, with a view to long-term application;
* Established boundaries between the political and the scientific arena are subject to continual renegotiation;

be made explicit. Because scientific consensus about the truth of the risk of climate change is unlikely to be achieved given the *post-normal* situation, we will have to drop our demand for a single certain truth and strive instead for transparency of the various positions and learn to live with pluralism in risk assessment.

Further, it has been stressed by several authors that in a post-normal situation quality control of the assessment practice and the interpretation of the policy-meaning of outcomes of assessments cannot be left to the experts themselves. As Funtowicz and Ravetz (1992) state: *"in the face of such uncertainties, they* [the experts] *too are amateurs."* To handle issues in a post-normal situation, and to extend quality control, new trans-disciplinary contacts and integration (internal extension of the peer community) on the one hand, and new contacts with policy-makers, Non-Governmental Organizations (NGOs), industry, media and the public (external extension of the peer community) on the other hand, are necessary (Funtowicz and Ravetz, 1992, 1993; Nolin, 1995).

In mediating science and policy, experts act in a complicated arena that overlaps with scientific arenas and with political arenas and is embedded in social arenas. Each of these types of arenas imposes different demands and constraints on the experts. Merton (1973) formulated the demands in the scientific arena as *universalism* (knowledge claims should be evaluated using pre-established impersonal criteria), *communality* (scientists should share their findings), *organized scepticism* (instead of dogmatic acceptance of claims, scientists should suspend judgement until sufficient evidence and argument are available) and *disinterestedness* (scientists should avoid self-interested behaviour that conflicts with the institutional goal of science to extend certified knowledge). In the political arena the experts are required to transform the complexity into simplified unambiguous quantitative information. For policies to be legitimated, robustness and consensus regarding scientific claims about the problem at hand are desirable. The policy process further requires that the problem be reduced to a selection of various policy options, and that attempts be made to balance the pros and cons of different strategies for managing the risks of anthropogenic climate change. For this purpose, the policy process also needs instruments, even if the science is incomplete. At the same time, the experts need to negotiate credibility not only with scientific peer groups and policy-makers but also with other actors. Within the social arena of competing interest groups, the experts need to legitimate their scientific claims, especially those claims which, according to some actors, justify far-reaching measures.

The experts are well aware of the complexity of the arena within which they operate, as can be illustrated by a statement made by the chairman of the IPCC, Bert Bolin (1994): *"Scientists as well as politicians need to recognize their different roles. The former must protect their scientific integrity, but also respect the role of politicians. Scientists must also be viewed as honest representatives of their scientific colleagues, to ensure that the assessment process will maintain its credibility."* And: *"Scientists need to inform politicians in a simple manner that can be readily understood, but the message must always be scientifically exact. In reality, little of what we know as scientists is politically interesting or even understandable. Politicians are seldom scientists. It is difficult to sift objectively all the available scientific information and extract what is politically relevant."*

1.4.2 Uncertainty as a social construct

A notion relevant to the understanding of the science-policy interface, taken from the field of Sociology of Scientific Knowledge (SSK) is the relation between social distance (from the knowledge producers) and the perceived level of uncertainty in knowledge claims. MacKenzie (1990) developed the idea of the ’certainty trough’ (Figure 1.3): The perceived uncertainty of knowledge claims is smallest a little bit away from the actual site of knowledge production. Scientists from one discipline may attribute less uncertainty to knowledge from another discipline than the practitioners of that other discipline would themselves attribute to it. If applied to the climate change case: policy-makers may attribute less uncertainty to IPCC’s claims than do the IPCC experts themselves. Those who feel alienated from the IPCC process will attribute the highest uncertainty to the IPCC claims.

Wynne (1992) noticed that the discussions about uncertainty seemed to rely implicitly on the naive notion that inadequate control of environmental risks is due only to inadequate scientific knowledge. Wynne criticized this idea, and added the concept of indeterminacy as a category of uncertainty. Indeterminacy refers to the open-endedness (both social and scientific) in the processes of environmental damage caused by human intervention. Indeterminacy introduces the idea that contingent social behaviour also has to be included in the analytical and prescriptive framework. It also acknowledges the fact

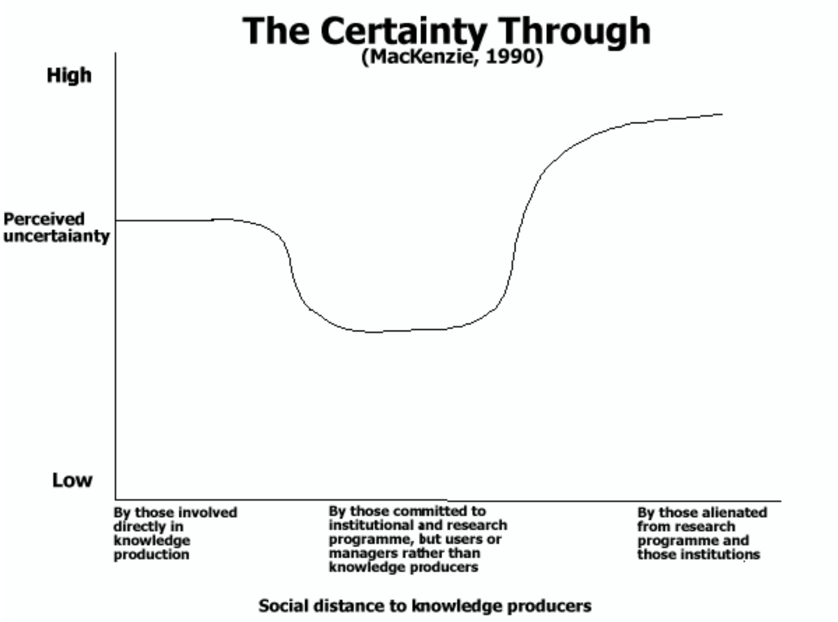

Figure 1.3. The certainty trough (MacKenzie, 1990)

that many of the intellectual commitments which constitute our knowledge are not fully determined by empirical observations. The latter implies that scientific knowledge depends not only on its degree of fit with nature, but also on its correspondence with the social world and on its success in building and negotiating trust and credibility for the science. We emphasize that this notion implies that *virtually every* scientific claim that comes under fire when it figures in a societal controversy with high decision stakes will turn out to be indeterminate and uncertain rather than foundational. It also implies that the history of uncertainty in climate change forecasting would be radically different if it concerned the forecasting of climate on a neighbouring planet rather than on the Earth, even if it relied on exactly the same science.

Other authors also have questioned the possibility of obtaining an objective definition of uncertainty. They argue instead that representation and perception of uncertainty are important factors, and that uncertainty is partly constructed as the product of implicit negotiation processes between scientists, policy-makers and the public (Collingridge and Reeve, 1986; Jasanoff, 1990; Van Eijndhoven and Groenewegen, 1991; Shackley and Skodvin, 1995).

We need to bear in mind that all actors with a stake in global warming have agendas of their own and are not always averse to manipulating uncertainty for various reasons. Uncertainties are often magnified and distorted to prevent the public from obtaining insight into the policy-making process and thereby obstructing it (Hellström, 1996). The uncertainty question can be (and is) actively used as a strategy to undermine the role of assessment as a shared source of information, and to achieve the postponement of measures.

What is needed is a better understanding of the limits of science in relation to the assessment community's task to provide a scientific basis for the climate policy debate, and a widening in focus from "reducing uncertainties" to "managing uncertainties and complexities".

1.5 Objective, research questions and thesis outline

The main objective of this dissertation is to gain insight into the processes by which assessments of the risks of anthropogenic climate change are constructed and more specifically into the way in which uncertainty management is conducted. This insight can help us to identify ways of managing uncertainties in the assessments better and ways of strengthening the role of assessment as a frame of reference acceptable to actors in the policy process. This dissertation investigates how scientific uncertainties are dealt with in climate risk assessment. Consequently, in this dissertation we seek answers to the following key research questions:

A. How has consensus been achieved and sustained regarding key elements in the assessments against a background of progressing scientific understanding, a growing body of climatic data, huge uncertainties and unresolved scientific puzzles surrounding the climate issue? (addressed in chapters 2 and 3)

B. 1. What are the different types and sources of uncertainties and their peculiarities? (addressed in chapters 4, 5 and 6)
   2. How have uncertainties been handled in the processes by which a scientific basis for the climate policy debate has been constructed? (addressed in chapters 2, 3, 5 and 6)

C. How can the management of uncertainties in the post-normal assessment practice be improved? (addressed in chapter 6)

In chapter 2 we analyse the construction and maintenance of consensus regarding the estimate of climate sensitivity (a key concept in the field of anthropogenic climate change) presented in assessment reports of the climate issue which were prominent in the international arena. In section 1.4 we argued that the scientific arenas, political arenas and social arenas in which experts act, all put different demands and constraints on the experts. It is not always possible for the experts to satisfy all the needs. For instance, often quantitative estimates are provided for policy purposes, although there is no sound scientific basis for drafting such estimates. There is a paradox: the greater the scientific uncertainty about the magnitude and the timing of a plausible risk, the greater is the demand for quantitative assessment of that risk, and the greater is the difficulty of making such an assessment. In the case of climate change, experts provided a quantitative estimate of 1.5°C to 4.5°C for climate sensitivity, in spite of the fact that the level of scientific understanding and the available data did not (and indeed, still do not) provide a sound basis for establishing these estimates with a reasonable level of certainty. The current range of quantitative estimates of 1.5°C to 4.5°C for climate sensitivity has persisted in successive assessments for two decades, despite new insights and information, including major changes in the climate models that form the starting points of the assessments. A key question in this chapter is how this stability has been maintained.

Science and technology studies have shown that scientific data, expert interpretation of these data, and the meaning of the expert interpretation for policy, are linked by (implicit or explicit) argumentative chains that after consolidation remain open to deconstruction and reconstruction in order to accommodate change on each side of the chain (Van Eijndhoven and Groenewegen, 1991). The interpretive space in the data and in the knowledge, which is made up by the uncertainty ranges and incomplete understanding, implies a flexibility in the argumentative chains that link data, expert interpretation and policy meaning. The tension between conflicting scientific, social and political needs can (at least partly) be absorbed by this flexibility (Van Eijndhoven and Groenewegen, 1991). This notion is central to our analysis in chapters 2 and 3 of the construction process of key elements of the assessments.

In chapter 3 we analyse the processes of closure in risk assessment in the international arena and in the Netherlands arena, and the diffusion of insights between the arenas, for two innovations in climate risk assessment which took place at the international level: the quantitative representation of climate sensitivity and the inclusion of non-CO_2 greenhouse gases in assessment studies. The central question in chapter 3 is: how did the closure on the estimates for climate sensitivity and on the inclusion of non-CO_2 greenhouse gases in the assessments of climate change take place in the Netherlands?

For the cases studied, we identify variability in the assessment reports in the Netherlands in the pre-IPCC period. In the Netherlands arena, the assessments in this period can be grouped as exponents of two different lines, a Netherlands line and an International line. We seek to identify what factors were decisive in the selection processes that resulted in closure of the visible disputes (visible in or across the assessment reports) for both cases. Our analysis reveals a remarkable difference in the adoption behaviour of two Dutch assessment groups, one being a committee of the Gezondheidsraad (Health Council) and the other being the Advisory Council for Research on Nature and Environment (RMNO), despite a large overlap in membership. Central questions are: Why did it take significantly longer to reach closure in the assessments in the Netherlands arena compared to the international arena, and what can we learn from the different modes of conduct of the committees of the Gezondheidsraad and the RMNO respectively?

In chapter 4 we present a modelling study which clearly illustrates the peculiarities of uncertainties regarding model structure. An important shortcoming of Earth System

Models is that many possibly important biogenic feedbacks are omitted. In chapter 4 we investigate the potential role of the biota in the carbonate-silicate geochemical cycle (hereafter referred to as CSGC) in the stability of the global climate, using a dynamic simulation model.

On a geological time-scale (> 10,000 years) the CSGC is believed to exercise a major control on atmospheric CO_2 and hence on the radiation balance of the Earth. The idea that the CSGC acts as a global thermostat was first put forward by Walker, Hays, and Kasting (1981). However, they did not include the biota in their model. To explore the effects of the biota on the operation of this thermostat we modify the BLAG'83-model (Berner *et al.*, 1983) of the CSGC, assuming temperature optimum response functions for major fluxes that are known to be substantially influenced by living systems. With the modified model we investigate the stability of the simulated global climate with respect to changes in solar luminosity, using a quantitative index for stability. We compare the stability obtained using biotic assumptions to the stability obtained using abiotic assumptions, and we investigate the relation between the parameters of the temperature optimum response functions and the simulated stability. As we will show, the inclusion of biota in Earth System Models can dramatically change not only their quantitative behaviour, but also their qualitative behaviour: in our case, the inclusion of the biota introduces the existence of a transition in the attractor of the system from equilibrium points into stable limit cycles.

The Integrated Assessment Models (IAMs) to analyse environmental issues are an important class of instruments that the policy process requires in order to explore possibilities, compare policy options, and balance the pros and cons of different response strategies. We discuss IAMs in chapters 5 and 6. A perfect IAM would model the complete causal chain, including all the feedbacks within this chain. The causal chain starts with socio-economic drivers which lead to economic activity and other practices, leading to emissions and other pressure on the environment, leading to environmental changes, leading to physical impacts on societies and ecosystems, leading to socio-economic impacts, and eventually the chain ends where it began, causing changes in the socio-economic drivers. The idea is that such an integrated model can be used as an instrument to evaluate and compare the consequences of combinations of policy measures, or to select an optimal mix of policy measures in order to meet a specified target.

There is a controversy regarding the usefulness of IAMs in the climate case, in the light of the huge uncertainties and unresolved scientific puzzles in this field. Further, because of the immaturity of the field, there are many different approaches to integrated assessment modelling and uncertainty management in IAMs. There is no shared body of knowledge and standards of 'best practice' for integrated assessment modelling (Parson, 1995).

In chapter 5 we analyse uncertainties in and limits to predictability encountered at each stage of the causal chain of climate change which IAMs attempt to represent. As a contribution to a better understanding of the limits of science in relation to its use for policy in IAMs, answers are sought to the following questions:

i) What are the possibilities and limitations of IAMs in relation to the task of modellers to model the complete cause-effect chain?

ii) What are the possibilities and limitations of IAMs in relation to the task of modellers to guide and inform the policy process?

We present an inventory of key uncertainties and limits to assessability in each step of the causal chain of anthropogenic climate change, and we analyse how these uncertainties and limitations are handled in the IAM practice. Also, we identify a major controversy regarding the usefulness of climate IAMs for policy analysis.

In chapter 6 we analyse the problem of uncertainty management in (Climate) Integrated Assessment Models (IAMs), focusing on the IMAGE 2-model (Integrated Model to Assess the Greenhouse Effect, Alcamo *et al.*, 1990). One of the findings of chapter 5 is that we can't reduce all uncertainties. Consequently we will have to learn to live with irreducible uncertainties. We notice that what the assessment community needs is to widen its focus from "reducing uncertainties" to "managing uncertainties and complexities." Therefore, we need to disentangle the uncertainty problem in such a way that we can identify the reducible uncertainties and the irreducible uncertainties in the model. As a contribution to that objective, answers are sought to the following questions:

i) What are the types and sources of uncertainties in climate IAMs?

ii) What are the main areas of improvement in uncertainty management in IAMs?

iii) How can we distinguish between reducible uncertainty and irreducible uncertainty?

iv) How can we attribute part of the overall potential for improvement of a model to its individual constituents?

An answer to the last question would enable us to identify those weak parts of the model which, if enhanced, would contribute most to the overall potential for improvement of the quality of the model outcome. This could be useful for the setting of research priorities to reduce uncertainties in climate risk assessment. Further, the distinction between rducible and irridicible uncertainties permits the development of adequate response strategies that take the irreducible uncertainties into account. On the basis of literature and interviews with model builders, we present an inventory of the way in which questions regarding uncertainty and quality are being addressed in IAMs. We analyse the scope of the current practice of uncertainty management, using a two-dimensional classification scheme that comprises the type and the source of uncertainty. We compile an inventory of methodologies available to address different types and sources of uncertainty in models. We identify major gaps in the current practice of uncertainty management in IAMs and look at the reasons for the existence of these gaps.

Chapter 7 gives an overview of the major findings of the thesis.

1.6 References

H. Abarbanel, S. Koonin, H. Levine, G. MacDonald, and O. Rothaus, *Issues in Predictability*, JSR-90-320, JASON, The MITRE Corporation, McLean, VA, 1991.

J. Alcamo (ed), IMAGE 2.0: Integrated Modeling of Global Climate Change, *Water, Air, and Soil Pollution*, **76**, Nos. 1/2, 1994a.

S. Arrhenius, On the influence of Carbonic Acid in the Air upon the Temperature of the Ground, *The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science*, **41**, 251, April 1896, p. 237-276.

R.A. Berner, A.C. Lasaga, and R.M. Garrels, The Carbonate Silicate Geochemical Cycle and its Effect on Atmospheric Carbon Dioxide over the Past 100 Million Years: American Journal of Science, **283**, 1983, p. 641-683.

S. Boehmer-Christiansen, Global Climate Protection Policy: the Limits of Scientific Advice, Part 1, *Global Environmental Change*. **4** (2), 1994a, p. 140-159.

S. Boehmer-Christiansen, Global Climate Protection Policy: the Limits of Scientific Advice, Part 2, *Global Environmental Change*. **4** (3), 1994b, p. 185-200.

B. Bolin, Science and Policy Making, *Ambio*, **23**, 1, 1994, p.25-29.

B. Bolin, B.R. Döös, J. Jäger, and R.A. Warrick, *The greenhouse effect, climatic change and ecosystems*, SCOPE 29, John Wiley & Sons, Chichester, 1986, 541 pp.

K. Bryan, Climate and the Ocean Circulation: III. The Ocean Model, *Monthly Weather Review*, **97**, 1969, p. 806-827.

G.S. Callender, The Artificial Production of Carbon Dioxide and its Influence on Temperature, *Q.J.R. Meteor. Soc.*, **64**, 1938, p.223-240.

G.S. Callender, Can Carbon Dioxide Influence Climate, *Weather*, **4,** 1949, p. 310-314

R.J. Charlson, J.E. Lovelock, M.O. Andreae and S.G. Warren, Oceanic Phytoplankton, Atmospheric Sulphur, Cloud Albedo and Climate, *Nature*, **326**, 1987, 655-661.

D. Collingridge and C. Reeve, *Science Speaks to Power*, Printer, London, 1986.

M.Z. Cutajar, *Berlin Mandate opens way for post-2000 commitments*, United Nations Climate Change Bulletin, Issue 7, 2nd Quarter 1995a.

M.Z. Cutajar, *Convention Parties start work on future commitments*, United Nations Climate Change Bulletin, Issue 9, 4th Quarter 1995b.

H. de Boois and J. Rotmans, Overzichtsmodel van de CO_2-Problematiek, in: *Berichten uit het RIVM 1985*, Bilthoven, 1986.

J. Emsley (ed.), *The Global Warming Debate, The Report of the European Science and Environment Forum*, Bourne Press, Dorset, 1996, 288 pp.

E. Eriksson, Report on an informal conference in atmospheric chemistry held at the Meteorological Institute, University of Stockholm, May 24-26 1954, *Tellus*, **6** (3), 1954, p.302-307

E. Eriksson, Report on the second informal conference on atmospheric chemistry, held at the Meteorological Institute, University of Stockholm, *Tellus*, **7** (3), 1955, p.388-394

S.O. Funtowicz and J.R. Ravetz, *Uncertainty and Quality in Science for Policy*, Kluwer,

Dordrecht, 1990.
S.O. Funtowicz and J.R. Ravetz, Three Types of Risk Assessment and the Emergence of Post- Normal Science, in S. Krimsky and D. Golding (eds), *Social Theories of Risk*, Westport CT, Greenwood, 1992, 251-273.
S.O. Funtowicz and J.R. Ravetz, Science for the Post-Normal Age, *Futures*, September 1993, p. 739-755.
J. Grinevald, *The Greenhouse Debate in Historical Perspective, A Selective Chronological Sketch, Département de Science Politique*, University of Geneva, September 1995 (Draft).
J. Grinevald, *L'Effet de Serre de la Biosphere: de la Révolution Industrielle à l'écologie Globale*, Paris, Flammarion, 1996.
A.D. Hecht and D. Tirpak, Framework Agreement on Climate Change: A Scientific and Policy History, *Climatic Change*, **29**, 1995, p. 371-402.
T. Hellström, The Science-Policy dialogue in transformation: model-uncertainty and environmental policy, in: *Science and Public Policy*, **23** (2), 1996, p.91-97.
J.T. Houghton, G.J. Jenkins, and J.J. Ephraums (eds.), *Climate Change, The IPCC Scientific Assessment*, Cambridge University Press, 1990, 365 pp.
J.T. Houghton, B.A. Callander and S.K. Varney (eds.), *Climate Change 1992 The Supplementary Report to the IPCC Scientific Assessment*, Cambridge University Press, 1992, 200 pp.
J.T. Houghton, L.G. Meira Filho, J. Bruce, Hoesung Lee, B.A. Callander, E. Haites, N. Harris, and K. Maskell (eds.), *Climate Change 1994 Radiative Forcing of Climate Change and An Evaluation of the IPCC IS92 Emission Scenarios*, Cambridge University Press, 1994, 339 pp.
J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.
ICSU, *The International Geosphere Biosphere Programme: A Study of Global Change*, Final report of the *ad hoc* planning group, ICSU 21st General Assembly, Berne, 1986, 21 pp.
IGBP, *Reducing Uncertainties*, International Geosphere Biosphere Programme, Stockholm, 1992.
J. Jäger and H.L. Ferguson (eds.), *Climate Change: Science, Impacts and Policy*, Proceedings Second World Climate Conference, Cambridge University Press, 1991, 578 pp.
J. Jäger, J. Cavender Bares, N. Dickson, A. Fenech, P. Haas, F. Hampson, P. Hughes, A. Liberatore, E.A. Parson, V. Sokolov, F. Toth, C. Waterton, J.P. van der Sluijs, J.C.M. van Eijndhoven, and B. Wynne 1997. Risk Assessment, in: The Social Learning Group, *Learning to Manage Global Environmental Risks: A Comparative History of Social Responses to Climate Change, Ozone Depletion and Acid Rain*. MIT Press, forthcoming (1998?).
S. Jasanoff, *The Fifth Branch, Scientific Advisers as Policy Makers*, Harvard University

Press, Harvard, 1990.
S. Jasanoff, G.E. Markle, J.C. Pietersen and T. Pinch (eds.), *Handbook of Science and Technology Studies*, Sage Publications, Thousend Oaks/London/New Delhi, 1995.
R.W. Kates, J.H. Ausubel, and M. Berberian, *Climate Impact Assessment: Studies of the Interaction of Climate and Society*, SCOPE 27, Chichester, Wiley, 1986, 625 pp.
M.E. Kowalok, Common Threads, Research Lessons from Acid Rain, Ozone Depletion and Global Warming, *Environment*, **35** (6), 1993, p.13-38.
A. Labrijn: Verandert ons klimaat? *Hemel en Dampkring*, **48**, 1950, p.170-173
J. Lanchbery and D. Victor, The Role of Science in the Global Climate Negotiations, in: H.O. Bergesen and G. Parmann, (eds.), *Green Globe Yearbook of International Co-operation and Development, 1995*, Oxford University Press, Oxford, 1995, p.29-40.
R.S. Lindzen, Some Coolness Concerning Global Warming, *Bulletin American Meteorological Society*, **71**, 1990, 288-299.
D. MacKenzie, *Inventing Accuracy*, MIT Press, Cambridge, MA, 1990.
S. Manabe, Climate and the Ocean Circulation: 2. The Atmospheric Circulation and the Effect of Heat Transfer by Ocean Currents, in: *Monthly Weather Review*, **97**, 1969, p.775-805.
S. Manabe and R.T. Wetherald, Thermal Equilibrium of the Atmosphere with a given distribution of relative humidity, in: *Journal of Atmospheric Science*, **24**, 1967, p. 241-259.
S. Manabe and J.L. Holloway, Jr., The Seasonal Variation of the Hydrological Cycle as simulated by a Global Model of the Atmosphere, *Journal of Geophysical Research,* **80**, 1975, p.1617-1649.
E. Masood, Head of climate group rejects claim of political influence, *Nature*, **381**, 1996, p. 455.
J.H. Mercer, West Antarctic ice sheet and CO_2 greenhouse effect: a threat of disaster in: *Nature*, **271**, 1978, p.321-325.
R.K. Merton, *The Normative Structure of Science*, in: R.K. Merton, The Sociology of Science; Theoretical and Empirical Investigations, Chicago: The University of Chicago Press, 1973, p.267-278.
MIT, *Inadvertent Climate Modification, Report of the Study of Man's Impact on Climate (SMIC),* MIT, Cambridge Massachusetts, 1971.
F. Möller, On the Influence of Changes in the CO2 Concentration in the Air on the Radiation Balance of the Earth's Surface on the Climate, *Journal of Geophysical Research*, **68** (13), 1963, p. 3877-3886.
W.A. Mordy: Report on the Third Annual Conference on Atmospheric Chemistry, May 28-30 1956, *Tellus*, **9** (1), 1957, p.127-134.
U.S. National Academy of Sciences 1979, *Carbon Dioxide and Climate: A Scientific Assessment*, Climate Research Board, U.S. National Academy of Sciences, Washington D.C., 22 pp.
U.S. National Academy of Sciences 1983, *Changing Climate, Report of the Carbon*

Dioxide Assessment Committee, Board on Atmospheric Sciences and Climate, Commission on Physical Sciences, Mathematics and Resources, National Research Council, Washington D.C., 495 pp.
G.H. Neumann, The fourth Annual Conference in Atmospheric Chemistry May 20-22 1957, *Tellus*, **10** (1), 1958, p.165-169
J. Nolin, *Stratospheric Ozone and Science: A Study of Post Normal Science* (in Swedish, with English summary), Ph.D. Thesis, Göteborg University, 1995, 302pp.
E.A. Parson, Integrated Assessment and Environmental Policy Making, In Pursuit of Usefulness, in: *Energy Policy*, **23** (4/5), 1995, p. 463-476.
J.P. Peixoto and A.H. Oort, *Physics of Climate*, American Institute of Physics, New York, fourth printing, 1995, 520 pp.
G.N. Plass, The Carbon Dioxide Theory of Climatic Change, *Tellus*, **8**, 1956, p. 140-154.
M.F. Price, The Evolution of Global Environmental Change, Issues and Research Programmes, in: Impact of Science on Society, **42** (2), 1992, p. 171-182.
S. Rahmstorf, Bifurcations of the Atlantic Thermohaline Circulation in Response to Changes in the Hydrological Cycle, *Nature*, **378**, 1995, p. 145-149.
R. Revelle and H.E. Suess, Carbon Dioxide Exchange between the Atmosphere and the Ocean and the Question of an Increase of Atmospheric CO_2 During the Past Decades, *Tellus*, **9**, 1957, p.18-27.
J. Rotmans, *IMAGE An Integrated Model to Assess the Greenhouse Effect*, (Thesis), Rijksuniversiteit Limburg, 1990, 295 pp.
C. Sagan and G. Mullen, Earth and Mars: Evolution of Atmospheres and Surface Temperatures: *Science*, **177**, 1972, p. 52-55.
B.D. Santer, K.E. Taylor, T.M.L. Wigley, T.C. Johns, P.D. Jones, D.J. Karoly, J.F.B. Mitchell, A.H. Oort, J.E. Penner, V. Ramaswamy, M.D. Schwartzkopf, R.J. Stouffer and S. Tett, A Search for Human Influences on the Thermal Structure of the Atmosphere. *Nature*, **382**, 4 July 1996, p. 39-46
S. Schneider, The Global Warming Debate Heats Up: An Analysis and Perspective, *Bulletin American Meteorological Society*, **71** (9), 1990, p. 1292-1304.
S.H. Schneider, Detecting Climatic Change Signals: Are There Any "Fingerprints"?, *Science*, **263**, 1994, p. 341-347.
Science and Policy Associates, inc., *Dutch National Research Programme On Global Air Pollution and Climate Change, NRP Phase I Final Review Report*, Science and Policy Associates, inc., Washington D.C., March 1995.
S. Shackley and B. Wynne, Global climate change and modes of international science and policy, in: A. Elzinga and C. Landström (eds.), *Internationalism and Science*, Taylor Graham, London, 1996, p. 199-222.
S. Shackley, P. Young, S. Parkinson and B. Wynne, Uncertainty, Complexity and Concepts of Good Science in Climate Change Modelling: Are GCMs the Best Tools?', *Climatic Change*, forthcoming.
S. Shackley and B. Wynne, *Integrating Knowledges for Climate Change, Pyramids, Nets and Uncertainties*, Global Environmental Change, 5 (2), 1995, p. 113-126.
S. Shackley and T. Skodvin, IPCC Gazing and the Interpretative Social Sciences, *Global*

Environmental Change, **5** (3), 1995, p. 175-180.
A. Stijkel, *On Managing Reproductive Risks of Occupational Exposure to Chemicals*, Ph.D. Thesis, Utrecht University, 1995, 174 pp.
K.E. Taylor and J.E. Penner, Response of the Climate System to Atmospheric Aerosols and Greenhouse Gases, *Nature*, **369**, 1994, p. 734-737.
The Social Learning Group, *Learning to Manage Global Environmental Risks: A Comparative History of Social Responses to Climate Change, Ozone Depletion and Acid Rain*. MIT Press, forthcoming (1998?).
H. Tennekes, The Limits of Science, The Conversion of a Meteorologist to Ecology, in: Zweers and Boersma (eds.), *Ecology, Technology and Culture*, White Horse Press, UK, 1994.
J.P. van der Sluijs, *Experts in the Netherlands and the risk of Global Climate Change, 1946 - present (working draft)*, contribution F6 to the Social Learning project (Research report NW&S No:92012), Utrecht University, February 1992.
J.C.M. van Eijndhoven and P. Groenewegen, The Construction of Expert Advice on Health Risks, *Social Studies of Science*, **21**, 1991, p. 257-278.
Victor, D.G. and W.C. Clark, *The Greenhouse Effect in the USA: A History of Science up to 1985*, Contribution Number 2 to the Project Social Learning in the Management of Global Environmental Risks, 1991, 81 pp.
Walker, J.C.G., Hays, P.B., and Kasting, J.F., A Negative Feedback Mechanism for the Long-Term Stabilization of Earth's Surface Temperature, *Journal of Geophysical Research*, **86**, 1981, p. 9776-9782.
WCPR, *World Climate Research Programme*, 1979.
A. Weinberg, Science and Transscience, *Minerva*, **10**, 1972, 209-222.
P. Williamson, Integrating Earth System Science, *Ambio*, **23**, 1, 1994, p.3.
World Meteorological Organization, *WMO statement on Modification of the Ozone Layer Due to Human Activities and Some Possible Geophysical Consequences*, WMO, 26 November 1975.
World Meteorological Organization, *Proceedings of the world climate conference : a conference of experts on climate and mankind*, World Meteorological Organization, Geneva, 1979, 791 p.
B. Wynne, Uncertainty and Environmental Learning, in: *Global Environmental Change*, **2**, 1992, p.111-127.

J.P. van der Sluijs, J.C.M. van Eijndhoven, B. Wynne, and S. Shackley (1998), Anchoring Devices in Science For Policy: The Case of Consensus Around Climate Sensitivity, Social Studies of Science, 28, 291-323.

2. Anchoring Devices in Science For Policy: The Case of Consensus Around Climate Sensitivity[1]

2.1	Introduction
2.2	Science for policy
2.3	The CO_2-doubling temperature: a history of sticking to the same numbers
2.3.1	The concept of 'climate sensitivity'
2.3.2	The construction of the estimate for climate sensitivity
2.3.3	The 'best guess' and the uncertainty about the climate sensitivity range
2.3.4	Different meanings and functions of climate sensitivity
2.3.5	Multiple functions and uses of climate sensitivity
2.3.6	The 1.5°C to 4.5°C temperature range as an *anchoring device* in the climate debate
2.4	Conclusion and discussion
2.5	Acknowledgements
2.6	Notes

Abstract

This chapter adds a new dimension to the role of scientific knowledge in policy by emphasizing the multivalent character of scientific consensus. We show how the maintained consensus about the quantitative estimate concerning a central scientific concept in the anthropogenic climate change field, namely climate sensitivity, operates as an anchoring device in science for policy. The consensus-estimate of 1.5°C to 4.5°C for climate sensitivity has remained unchanged for two decades in international assessments of the climate issue. Nevertheless, during these years, climate research has expanded enormously, and scientific knowledge and complexity of climate models changed accordingly. We empirically identify a repertoire of sources from which the experts managed to acquire flexibility in maintaining the same numbers for climate sensitivity whilst not ignoring changing scientific ideas. Sources of flexibility include changes in: (1) the modes of reasoning, (2) the types of uncertainty accounted for, (3) estimates of the best guess rather than changes in the range; (4) the connotation of the range, (5) the definition of climate sensitivity and (6) the implication of the range.

The remarkable ostensible stability of the climate sensitivity range may play a significant role in holding together a variety of different social worlds in a situation where the state of scientific knowledge does not grant the 1.5 to 4.5°C range a higher scientific status than an 'educated guess'. But the stability can also be seen as a function of an implicit social contract amongst the various scientists and policy specialists involved, which allows 'the same' concept to accommodate

---

This chapter was written as a co-authored paper: J.P. van der Sluijs, J.C.M. van Eijndhoven, B. Wynne, and S. Shackley, Anchoring Devices in Science For Policy: The Case of Consensus Around Climate Sensitivity, *Social Studies of Science* (forthcoming) *tacitly different local meanings. Thus the very multidimensionality of such scientific concepts is their resilience and value in bridging and perhaps reorganizing the differentiated social worlds typically involved in most modern policy issues. The different importance of particular dimensions of knowledge for different social groups may be one way in which pluralism is held together.*

2.1. Introduction

Experts started drafting assessment reports for policy-makers when research on anthropogenic climate change and especially climate modelling was still in an early stage of development. Assessment is the analysis and review of information derived from research for the purpose of helping someone in a position of responsibility to evaluate possible actions or think about a problem. Assessment usually does not mean doing new research. Assessment means assembling, summarizing, organizing, interpreting, and possibly reconciling pieces of existing knowledge, and communicating them so that they are relevant and helpful for the deliberations of an intelligent but inexpert policy-maker.[1] Assessments of anthropogenic climate change have been conducted since the 1970s.

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

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

The Toronto Conference in 1988 marked the beginning of high-level political debate on the risks of anthropogenic climate-change.[5] It recommended a world-wide CO_2 emission reduction of 20% in the year 2005, relative to 1988. Independent of the Toronto Conference, UNEP and WMO established in 1988 the Intergovernmental Panel on Climate Change (IPCC), chaired by Bert Bolin, a Swedish climate scientist. The panel consisted of three working groups: Working Group I (WGI) to assess the science; II to assess the impacts and III to formulate response strategies. In 1990 IPCC WGI issued their first report (hereafter to be called IPCC'90 report), a comprehensive state-of-the-art report, with an executive summary for policy-makers.[6] Hundreds of scientists from all over the world contributed to this report. At the Second World Climate Conference in Geneva (1990), the IPCC assessment was accepted by over 137 attendant countries as a vital scientific basis for international negotiations on a climate convention. The Framework Convention on Climate Change (FCCC) was prepared under the auspices of the United Nations from 1990 onwards and took effect on 21st March, 1994. Updates of the assessment were issued in 1992[7] and 1994[8]. A new state-of-the-art IPCC report was completed in 1995, and accepted in July 1996 by the Conference of Parties to the Climate Convention as the primary source of scientific and technical advice for the implementation of the FCCC[9].

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

We analysed the concept of climate sensitivity as used in each of the above-mentioned major assessments, and investigated the backgrounds to the establishment of ranges for climate sensitivity as well as the variation in meaning of the concept over these assessments. The major sources were the assessment reports themselves. We also interviewed key-persons involved in the assessments (mainly lead authors of the relevant sections of the reports) in order to bring to light considerations and decisions underlying the assessments when these were not clear to us from the texts of the assessment reports themselves. By making use of theoretical notions on the flexible science-policy interface we interpret the reasons behind the apparent stability. We asked some of the scientists contacted for their comments on the findings and incorporated their reactions in our discussion.[10] In this paper we explore how and why consensus on the climate sensitivity range has persisted, despite the massive uncertainties which are widely acknowledged to pervade the field of climate change.

2.2 Science for policy

Although there have been several attempts to provide alternative models of how science and policy interrelate, science studies have often continued to use concepts which tend to reinforce entrenched ideas of 'science' and 'policy' as distinctly defined worlds. Leading scientists in many fields of science relevant to policy have explicitly emphasized the importance of scientific consensus for policy legitimacy, and perceive it as an independent prior variable. Collingridge &

Reeve rightly challenge this conventional view that scientific disagreement, or uncertainty, compromises policy authority and effectiveness.[11] [12] In their ’under-critical’ and ’over-critical’ models, Collingridge & Reeve argued that science is used either to legitimate policies developed for non-scientific reasons or is ignored if the consensus contradicts policy or there is scientific ’dissensus’.

In some respects, the history of the role of knowledge about anthropogenic climate change supports Collingridge & Reeve’s thesis. Several authors (Boehmer-Christiansen[13], Hart & Victor[14]) have suggested that the uptake of climate change science by policy-makers occurred only when the institutional and political circumstances facilitated it, namely in the mid- to late-1980s, when a ’window of opportunity’ opened up in the socio-political landscape.[15] Scientific knowledge in such cases appeared to be the trigger for policy uptake, even though the socio-political setting was perhaps the primary reason. This argument is supported by the fact that a scientific consensus existed *prior* to the uptake of the global warming issue by policy-makers, thus illustrating the secondary importance of science in the emergence of policy-windows.

A major limitation to such theoretical approaches is that they do not readily account for how new scientific insights are absorbed into the scientific assessment process. Van Eijndhoven and Groenewegen showed that in a case where new scientific data and new practical situations arose, experts serving on advisory bodies on environmental standards showed an appreciable amount of flexibility when drawing policy conclusions from scientific data. They emphasized that the connections between given scientific data, expert interpretation of these data and policy meaning are more like chains of linked arguments and beliefs. New findings therefore do not necessarily imply support for a change in policy indicated by the new insights.[16] [17] In some cases particular linkages may be formed through a set of assumptions or convictions shared in an intermediary community which come to ’fix’ particular interpretations as ’given’. Van Eijndhoven and Groenewegen’s account thus emphasizes the actively constructed character of scientific knowledge in policy, as opposed to the idea of information simply being transferred from science to policy as a passive form of legitimation. They argue that more scientific knowledge often increases policy flexibility by introducing more possibilities for interpretation. This could be seen as greater uncertainty, hence as less policy cohesion. Scientific uncertainty, however, is itself socially modulated, so greater argumentative flexibility does not automatically translate into greater social disagreement and policy weakness.

Van Eijndhoven and Groenewegen’s empirically based findings are largely consistent with more theoretical accounts within the sociology of scientific knowledge which question the treatment of science and policy as distinctly defined worlds. From this perspective, accounts such as Collingridge and Reeve’s, and those that emphasize ’windows of opportunity’, are in danger of reducing all change to social factors, which in itself *presupposes* the distinction between the social and natural. The actor-network approach of Latour and co-workers, with its fruitful and radical dissolution of analytical precommitment to categories such as the natural and the social, has opened the sociological door (as it were) to impure categories and hybrid forms.[18] Although most of their work focuses on the building of sociotechnical networks rather than on explicit forms of policy-making concerning for instance environmental protection, the issue raised by their work concerning how categorical distinctions that fundamentally shape the world, such as science and policy, come themselves to be constructed and reproduced can cast new light on policy-oriented science too.

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

Several other bodies of work have been sensitive to the way in which hybrid communities and networks are built which link the worlds of science and policy in more complex ways than has been recognized by the conventional models of science and policy. Star and Griesemer's notion of *boundary objects*[19] which live in research and other worlds, and which allow a broad community of meaning across diverse social worlds and at the same time allow diverse meanings to be invested in those uniting concepts by different communities, corresponds to the multivalent character of 'consensus' and the importance of hybrid roles.

In her work on regulatory science, Jasanoff has also shown how US policy agencies have developed hybrid communities of advisers who combine the roles of scientific actor and policy actor in order to negotiate credible regulatory policies. These hybrids are rendered more credible precisely through their discursive repurification into the distinct public categories of science and policy.[20] In denying the existence of role-ambiguity, these discursive repurifications implicitly rely on some objective grounding in nature and scientific roles as their source of authority.

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

2.3 The CO_2-doubling temperature: a history of sticking to the same numbers

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

2.3.1. The concept of 'climate sensitivity'

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

Computer models of the climate system developed over the past few decades have included CO_2 by means of its impact on radiation, and hence climate variables. The climate sensitivity is calculated in a climate model by doubling the CO_2 atmospheric concentration instantaneously and then allowing the model to reach a new equilibrium which lets the interactive model processes adjust to the perturbation. This equilibrium temperature is compared with the result of the same model run in which the CO_2 concentration is kept constant. The difference between the two runs yields the equilibrium temperature change (climate sensitivity). This means that the climate sensitivity is the **potential** temperature change that will be realized fully only if a new equilibrium is established after a doubling of the CO_2 concentration. It is generally agreed that in reality the climate will take a long time to reach equilibrium because of the lag effect of the thermal inertia of the oceans (the latter having a much larger heat capacity than the atmosphere)[21]. Consequently, the temperature actually realized will always lag behind the equilibrium temperature that corresponds to a given CO_2 concentration.

The **realized** temperature change is the estimate of the non-equilibrium temperature change at the moment in time when the (gradually increasing) CO_2 concentration will have doubled. According to the IPCC'90, given the current rate of increase in CO_2 concentration, '*the realized temperature rise at any time is about 50% of the committed temperature rise if the climate sensitivity (. . .) is 4.5°C and 80% if the climate sensitivity is 1.5°C*'.[22]

A further difference between 'climate sensitivity' and 'realized temperature change at a given time' is that 'climate sensitivity' generally refers to the temperature change induced by a forced change in only one isolated variable: the radiative forcing caused by a doubling of the atmospheric CO_2 concentration.[23] In the model calculations of climate sensitivity, all the other climate-influencing variables - such as planetary albedo (reflectivity), aerosol concentration (particles suspended in the atmosphere) and evaporation - change only through their involvement in the internal feedback loops. The 'realized temperature change' will depend on changes in other external forcing variables that influence climate. An example is the local cooling effect of anthropogenic sulphate aerosol particles in the atmosphere.[24]

The estimates are generally based on the simulation results of General Circulation Models

(GCMs) which are widely regarded as the most advanced climate models available at present.[25] GCMs are idealized mathematical representations of the climate system, including the atmosphere, ocean, ice and land surface, together with the processes, interactions and feedbacks that serve to couple these components.[26] In GCM models the atmosphere and ocean systems are represented by a three-dimensional set of grid-points. Physical laws, such as the equation of state for a gas, the hydrostatic balance, the conservation of mass, the conservation of energy, and so on, are used to calculate the fluxes of heat, mass, momentum, and so on, between the grid points. The resolution of the GCM grid is typically 3 to 4 degrees of latitude and longitude with 10 to 20 layers in the vertical dimension.[27]

Six GCM modelling groups have dominated the field of anthropogenic climate change, though many more climate modelling groups are now moving into this area of research[28]:

- NCAR: National Center for Atmospheric Research (Boulder, CO, USA);
- UKMO: United Kingdom Meteorological Office model (the Hadley Centre group of John Mitchell);
- GISS: NASA Goddard Institute of Space Studies (the group of Jim Hansen);
- GFDL: Geophysical Fluid Dynamics Laboratory, (Princeton, USA, the group of S. Manabe, R.T. Wetherald and R.J. Stouffer);
- CCC: Canadian Climate Center (the group of G.J. Boer).
- MPI: Max Planck Institut für Meteorologie, Hamburg (the group of K. Hasselmann)

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

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

2.3.2 The construction of the estimate of climate sensitivity

In this section we analyse how the experts in successive assessments translated the model results and other evidence into a range for the CO_2-doubling temperature. In 1979 the US National Academy of Sciences (NAS) 'Ad Hoc Study Group on Carbon Dioxide and Climate', chaired by Jules Charney (from MIT), produced the first notable assessment of the CO_2-doubling temperature. The group started by making an inventory of existing GCM results, which gave a range of 2°C to 3.5°C. Following an additional examination of feedback mechanisms not yet included in the models at that time, they argued that: '*As we have not been able to find evidence for an appreciable negative feedback due to changes in low- and middle-cloud albedos or other causes, we allow only 0.5°C as an additional margin for error on the low side, whereas because of uncertainties in high-*

cloud effects, 1°C appears to be more reasonable on the high side.' This argumentation brought them to their conclusion: '*We estimate the most probable global warming for a doubling of CO_2 to be near 3°C with a probable error of ±1.5°C.*'[29]

Table 2.1 lists the estimates given for climate sensitivity in the assessments that have figured in the international arena since then. The table shows that the stated consensus range for climate sensitivity has remained unchanged for two decades, even though the range of the individual GCM-results has changed over time.

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

Table 2.1 Range of individual GCM CO_2-doubling results, estimated ranges of climate sensitivity and 'best guesses', as reported in successive assessments.

In analysing how the range of GCM outcomes have been translated into the estimated climate sensitivity range, we find several trends. Firstly the range of GCM outcomes of CO_2-doubling calculations widened between 1979 and 1985, 2 degrees being added to the upper end and 0.5 degrees to the lower end of the NAS'79 range (see Table 2.1). Secondly, there is a clear shift in the mode of reasoning used to 'translate' the results of GCM CO_2-doubling calculations into the estimated climate sensitivity as it appears in the conclusions of the assessment. The Charney committee (NAS'79) **widened** the range of GCM outcomes for climate sensitivity from 2°C - 3.5°C to 1.5°C - 4.5°C by including a margin of error based on an expert assessment of the shortcomings of the models. The full scientific text of the Villach'85 meeting shows that the GCM outcomes for climate sensitivity range from 1.5°C to 5.5°C.[30] Yet, without any further argumentation within the report itself, this range is **narrowed down** in the official Conference Statement - the section directed to policy-makers - back to the 1.5°C to 4.5°C estimate.[31] This raises the question of why the policy-makers' summary did not deviate from the previously accepted estimate in spite of the wider range given in the full scientific text. According to Robert Dickinson, the author of the chapter of the Villach report that presents the 1.5°C to 5.5°C figure: '*My 5.5 for Villach was inferred by showing you would get at least that if you took the current GCM with the strongest ice albedo feedback and combined it with the model with the strongest cloud feedback, so that both strong feedbacks were in the same model. At the meeting Suki Manabe was personally sceptical that such a large number could be achieved, and I recall that led the meeting to adopt the previous* *range.'*[32]

One climatologist who attended Villach has subsequently accounted for the meeting's rejection of the 5.5°C figure as too high for three reasons: firstly, from the 'intuitive judgement' that the climate system was unlikely to exhibit such a high sensitivity. Secondly, if the climate system was indeed so sensitive our models would be unable to represent it since, even at the point of CO_2-doubling, the system would be in a state well beyond current variability over such time-scales. Yet our models are calibrated to simulate the current and past climate with its lower degree of variability. The third reason given is that since Dickinson used a statistical approach for his analysis, physical scientists are at liberty to interpret the results quite flexibly.[33]

Regarding the second reason given by this climatologist, there is a paradox common to much simulation modelling. The greater the degree of extrapolation from past conditions, the greater must be the reliance on a model as the instrument of prediction; hence, the greater is the degree of difficulty in doing just this.[34] Modelled change which deviates too far from the current state is likely to be unreliable. For example, with an increase of 5.5°C Antarctic sea-ice could be completely wiped out, with massive changes in physical processes and feedbacks which would affect climate, but which are not yet reliably taken into account by current models. None of the three reasons, however, or any others, is included in the text of the Villach'85 report. It is notable that embedded in these three reasons is a self-confirming circular element in the science.

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

IPCC'90 employed the following line of argument in arriving at its 1.5°C to 4.5°C estimate. The GCM results evaluated in the IPCC'90 assessment produced a range of 1.9-5.2°C (see table 3.a. (reproduced here as Table 2.2) of the IPCC'90 assessment).[35] However, IPCC made a selection of these GCM results, which narrowed the range back down to 1.9-4.4°C, a range which fits more closely with the previously accepted 1.5-4.5°C estimate. The IPCC's argument reads as follows: *'On the basis of evidence from the more recent modelling studies (Table 3.a.* [here Table 2.2, JvdS] *entries 3,4, 7-9, 17-22) it appears that the equilibrium change in globally averaged surface temperature due to doubling* CO_2 *is between 1.9 and 4.4°C. The model results do not provide any compelling reason to alter the previously accepted range 1.5 to 4.5°C (US National Academy of Sciences, 1979; Bolin et al., 1986)'* [that are, NAS'79 and Villach'85 respectively, JvdS].

From a detailed examination of Table 2.2, however, it can be seen that since they chose 1988 as the dividing-line beyond which results were defined as 'recent', the GCM results that fall outside the 1.5°C to 4.5°C estimate (their entries 12, 15 and 16) are excluded. These are a 4.8°C

result from the GISS model from 1984 and two 5.2°C results from the UKMO model from 1987. The IPCC gave no reasons why they excluded the calculations from before 1988, nor did they provide scientific arguments as to why the recent results are automatically better than the less recent ones. It is particularly strange that the GISS results were omitted, because the GISS model has been regarded, at least by some climatologists, as one of the better models for the study of anthropogenic climate change[36], (although each of the GCMs has strengths and weaknesses[37]).

At first sight it might be considered legitimate to skip the two 5.2°C UKMO 1987 results, because these were succeeded by three recent results from the same modelling group which were included by IPCC'90 (from 1989, indicating values of 2.7°C, 3.2°C and 1.9°C). However, the main scientific difference between the 1987 and the 1989 UKMO results is the way in which clouds are represented in the model. In the 1987 UKMO simulations, clouds were represented as a function of relative humidity (RH) (i.e. cloud formation occurs when the water vapour exceeds a given threshold). In the 1989 UKMO simulation clouds were represented by an equation for cloud liquid water (CLW) (i.e. an attempt is made to represent cloud formation in terms of more fundamental physical processes). It is not claimed, however, that the CLW representation is better than RH representation or vice versa. Mitchell, Senior & Ingram state explicitly in their 1989 paper that: *'although the revised cloud scheme is more detailed it is not necessarily more accurate than the less sophisticated scheme'* [38] The fact that both schemes are scientifically tenable therefore contributes to the uncertainty range.[39] For the purposes of the IPCC's scientific assessment, the UKMO 1987 simulation results cannot be discarded on the grounds that they have been 'replaced by new results'.

Further there is an inconsistency in the way in which model results were selected in IPCC'90, because their selection includes entry 8 of Table 2.2, which is a 4.0°C result of Manabe and Wetherald (GFDL model) from 1986. In conclusion our analysis shows that the IPCC'90 excluded those model results which did not accord with the previously accepted 1.5°C to 4.5°C estimate, without providing a clear or consistent justification.

In the 1992 supplement to the IPCC'90 report it was concluded that: '*There is no compelling new evidence to warrant changing the equilibrium sensitivity to doubled CO_2 from the range of 1.5-4.5°C as given by IPCC 1990*'.[40] However, if we read the report in more detail, we see that the new GCM results evaluated include figures of 4.8°C (CSIRO) and 5.3°C (LMD), and on p.118 we read: *'New equilibrium GCM simulations have widened the range slightly to 1.7°C (Wang et al., 1991a) and 5.4°C (Senior and Mitchell, 1992a), but no dramatically new sensitivity has been found.'* [41]

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

1. *'Recently, additional estimates of the climate sensitivity have been made by fitting the observed temperature record to the evolution of temperature produced by simple energy-balance climate/upwelling models, assuming that all the observed warming over the last century or so was due solely to increases in greenhouse gases (. . .) Schlesinger et al. (1991) obtain a value of 2.2±0.8°C (. . .)'*.[42]

2. *'Energy-balance model considerations bring previous estimates of sensitivity (IPCC, 1990) more in line with the IPCC 'Best guess''*.[43]

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

According to the first author of the section on the climate sensitivity in the IPCC'92 report, Lawrence Gates (Lawrence Livermore National Laboratory, USA), the climate sensitivity range was not extended in IPCC'92 because: *'In the absence of a comprehensive exploration of parameter space in the doubled CO_2 context, there appeared to be no compelling scientific evidence to change the earlier estimated 1.5-4.5°C range (which was itself an educated guess) since such a step would have given greater credibility to any new values than was justified.'* [44]

According to Bert Bolin, chairman of IPCC wrote to one of us on this point: *'In the preparations of the 92 assessment there was an extensive discussion about whether the uncertainty range could possibly be reduced to 2-4°C or not. Since there were no good scientific arguments to do so, the estimate remained unchanged.'* and added that: *'The importance to be able to justify a change scientifically was more important than the need for continuity in the results from the assessments.'* [45]

Both quotes reveal that the IPCC experts felt a great need for unambiguous scientific evidence to change the range. Apparently, however, there is no equally great need for evidence to *maintain* the range. What is clear from the comments of Dickson, Bolin and Gates is that the initial 1.5-4.5°C range is not derived from a procedure they regard as scientifically sound. Apparently, the need for scientific rigour applies more strongly to *changing* the climate sensitivity range than it does to its maintenance. This suggests that the scientific status of the temperature range is much lower than is generally perceived by the public, and this is not solely due to (necessary) simplification for public and policy comprehension. To argue that changing the values would require weighty scientific justification is also to implicitly acknowledge that the public view of the 1.5°C to 4.5°C figures is that they *have* been rigorously and precisely justified scientifically, whereas they have not. In the IPCC'95 report, the pattern of arguing is the same as in the previous IPCC reports. The range is not changed because *"No strong reasons have emerged to change these estimates of the climate sensitivity"*.[46]

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

2.3.3 The ’best guess’ and the uncertainty about the climate sensitivity range

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

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

Apparently, there was some discussion at IPCC’90 about whether the best-guess value should be changed to 3°C, this being closer to the middle of the range of model results (1.9°C - 5.2°C) namely approximately 3.5°C, and the figure of 3°C had also been accepted in the previous 1979 and 1983 assessments by the NAS. Some experts argued that the IPCC should not change the best guess unless very confident that it was scientifically justified, which was exactly the same argument as successfully maintained the range at 1.5°C to 4.5°C during IPCC’90. In the case of the ’best guess’, however, the argument was rejected. According to one participant, this was because of the evidence from the statistical analysis that the best guess was approximately 2.5°C, and also because if the sensitivity was 3°C more observational evidence of warming should have emerged. Both arguments are problematic, however, for reasons already noted in Section 2.3.2: a) the statistical method is regarded by modellers as less rigorous than GCM output and hence allowing more flexibility in the interpretion of the output, which allowed the IPCC to reject Dickinson’s statistically derived range of 1.5°C to 5.5°C; b) climate sensitivity as defined by the IPCC cannot be easily related to the observed temperature change since the latter does not include all the relevant forcing factors and is not at equilibrium (illustrating yet further the ambiguity in the definition of climate sensitivity).

According to an industrial scientist, some of the scientific organizers in the preparations for the IPCC'90 wished to go even further than providing a 'best guess' and requested the modellers to provide a probability value for the 1.5°C to 4.5°C range. This source claims that the Chairman of IPCC WGI argued that scientists should be able to use their own intuitive judgement in providing a probability value for the range. A figure of 80% likelihood was quoted, according to this source, i.e. giving a 20% chance that the sensitivity would be out of this range.[47] That pressures to provide subjective probability judgements were being exerted upon the experts is revealed by the following statement made by a participant modeller: *'What they were very keen for us to do at IPCC* [1990], *and modellers refused and we didn't do it, was to say we've got this range 1.5 - 4.5°C, what are the probability limits of that? You can't do it. It's not the same as experimental error. The range is nothing to do with probability - it is not a normal distribution or a skewed distribution.* ***Who knows what it is*** *?'* (our emphasis, JvdS).

Informally at least, some climate scientists are prepared to provide a probability estimate regarding the climate sensitivity range, and to indicate how confidence levels might have changed over time. The reluctance of the assessment community to do so formally in a publication is probably a reflection of the lack of any explicable scientific methodology for underpinning the exercise. Recently, however, a few attempts have been made by decision-analysts and climate impact assessors to obtain from climate modellers their subjective probability distributions for climate sensitivity.[48] Hence, once again because the tacit knowledge of modellers is not formally included in the climate sensitivity range, the changing interpretations of the latter are not effectively communicated to the non-specialist. Such scientific judgements are seemingly discredited in policy-relevant contexts such as the IPCC, but in the absence of elaboration they come to look somewhat arbitrary and inconsistent. In a comment on an earlier version of our chapter, the climate modeller Steve Schneider stressed the *ad-hoc* character of the range: *'The range was never established by a firm decision-analytic protocol in the first place, but rather was a heuristic from a responsible, but somewhat sloppy, community in the 1970s.'* In the assessments no method for screening outlier estimates was ever established which was other than *ad hoc*. According to Schneider, guessing was anathema to these experts: and any new procedure would also be *ad hoc* and would not yield more rigorous results. Consequently they just let the range stay as it was.

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

2.3.4 Different meanings and functions of climate sensitivity

The concept climate sensitivity and the '1.5°C to 4.5°C temperature range' are ambiguous entities. Firstly, there is ambiguity regarding what the number range implies (e.g. the total range of possibility, 90% confidence interval, or an educated guess?), and secondly there is ambiguity about the scientific status of the number range (well-established knowledge or an educated guess?). In Table 2.3 we list some statements relating to the meaning of the 1.5°C to 4.5°C range from the policy-makers summary, or equivalent, of successive assessments reports.

NAS '79:	Summary: *'When it is assumed that the CO_2 content of the atmosphere is doubled and statistical thermal equilibrium is achieved, the more realistic of the modelling efforts predict a global surface warming of between 2°C and 3.5°C, '* (p.1) After discussing model shortcomings and assessing their consequences for the figure the NAS concluded: *'We estimate the most probable global warming for a doubling of CO_2 to be near 3°C with a probable error of 1.5°C.'* (p.2)
NAS '83:	Executive summary: *'Results of most numerical model experiments suggest that a doubling of CO_2, if maintained indefinitely, would cause a global surface air warming of between 1.5°C and 4.5°C.'*
Villach '85:	Conference statement: *'The most advanced experiments with general circulation models of the climatic system show increases of the global mean equilibrium surface temperature for a doubling of CO_2 concentration, or equivalent, of between 1.5 and 4.5°C (. . .) values outside this range cannot be excluded.'* (p.xxi)
IPCC '90:	Policy-makers summary: *'The long term change in surface air temperature following a doubling of carbon dioxide (referred to as the climate sensitivity) is generally used as a benchmark to compare models. The range of results from model studies is 1.9-5.2°C. Most results are close to 4.0°C but recent studies using a more detailed but not necessarily more accurate representation of cloud processes give results in the lower half of this range. Hence the model results do not justify altering the previously accepted range of 1.5 to 4.5°C.'* (p. xxv)
IPCC '92:	*'the evidence from the modelling studies, from observations and the sensitivity analyses indicate that the sensitivity of global mean surface temperature to doubling CO_2 is unlikely to lie outside the range 1.5 to 4.5°C'* (p.5).
Bolin 1995:	*'There is uncertainty about the most likely change of climate that would be associated with a given increase of greenhouse gases in the atmosphere, but earlier estimates of warming by 1.5 - 4.5°C for a doubling of the equivalent carbon dioxide concentration, remain unchanged. It is important to stress that this range does not include zero. In other words, the scientific community is confident that, if greenhouse gases continue to increase, there will be a climate change.'*[49]
IPCC'95:	Technical summary: *'The likely equilibrium response of global surface temperature to a doubling of equivalent carbon dioxide concentration (the "climate sensitivity") was estimated in 1990 to be in the range 1.5 to 4.5°C, with a "best estimate" of 2.5°C. (. . .) No strong reasons have emerged to change these estimates of climate sensitivity.'*

Table 2.3 Formulations used to present the 1.5°C to 4.5°C estimate of climate sensitivity in the parts of the assessments directed at the policy-makers.

Below we discuss some of the key ambiguities and changes in the (apparent) definitions of climate sensitivity and the meaning of the associated 1.5°C to 4.5°C temperature range.

Equilibrium change or realized change?

Table 2.3 indicates that until 1990, the climate sensitivity was designated as an equilibrium temperature change (entries NAS'79, NAS'83, Villach'85 and IPCC'90). Yet strangely enough, the IPCC'92 and Bolin'95 quotations make no reference to the notion of equilibrium. However, the IPCC'95 quote re-includes the notion of equilibrium, and makes a clear distinction between transient and equilibrium responses to CO_2-doubling. The linguistic imprecision of leaving out the notion of equilibrium, may allow other experts and policy-makers to interpret the range as referring to actual change with further committed change still to be realized. Such leeway in the interpretation of the 1.5°C to 4.5°C range allows data from a range of disciplines associated with GCM modelling and climate change to be more readily introduced into the argumentative chains.

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

In scientific textbooks on climate modelling, the *climate sensitivity parameter* is usually defined as the response of the globally averaged surface air temperature to a unit-change in *forcing*.[50] This quantity was designed as a simple measure of intercomparing feedback mechanisms in 0-dimensional climate models. Later, the same quantity was also used to investigate and intercompare feedback mechanisms in GCMs.[51] The GCMs at that time usually calculated an equilibrium climate for an increase in radiative forcing corresponding to CO_2-doubling, namely 4 W/m^2. The climate sensitivity parameter was then calculated by dividing the global mean CO_2-doubling temperature by this 4 W/m^2. From the very beginning, it was recognized that this method implicitly assumed that the climate sensitivity parameter *"is essentially independent of the type of forcing (e.g. a change in solar constant, an increase in atmospheric CO_2, or incorporation of natural tropospheric aerosols)"*[52]

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

In IPCC assessments, the term *climate sensitivity* indicates the globally averaged surface temperature increase associated with a doubling of atmospheric CO_2 concentration. Although the *climate sensitivity parameter* and the *climate sensitivity* (to CO_2-doubling) are in principle different concepts (and are expressed in different physical units), they are used in an inconsistent way, both in IPCC reports and in scientific publications.[53] At some places in the IPCC'95 report, the term *climate sensitivity parameter* is used to mean the climate sensitivity to CO_2-doubling.[54] The confusion of the concepts can be illustrated from the glossary in IPCC'95: *'In IPCC reports, climate sensitivity usually refers to the long-term (equilibrium) change in global mean surface temperature following a doubling of atmospheric CO_2 (or equivalent CO_2) concentration. More generally, it refers to the equilibrium change in surface air temperature following a unit change in radiative forcing (°C/Wm^{-2}).'*.

As can be seen from Table 2.3, in the policy-makers' summaries the NAS'79, NAS'83, IPCC'90 and IPCC'92 assessments use a more restricted definition of climate sensitivity which refers specifically to *CO_2-doubling* rather than to an equivalent change in radiative forcing. If the climate sensitivity is defined relative to **CO_2** only, we will refer to the *narrow definition*, but if it is defined relative to **forcing** associated with CO_2-doubling, or to equivalent CO_2-concentration, we will refer to the *wide definition*.

Villach '85 referred to: *'doubling of CO_2 concentration, or <u>equivalent</u>' whilst* the Bolin '95 quote and the IPCC'95 quote from Table 2.3 also talk of doubling of the <u>equivalent</u> carbon dioxide concentration. These ambiguous formulations introduce flexibility into the interpretation of the concept for which the range is given since it is no longer clear whether the wide or the narrow definition is intended. The wider definition does seem to be partially embedded in the definition provided in the full scientific text of IPCC'92; viz. they say that climate sensitivity *'is a measure of the response of a climate model to a change in radiative forcing'* associated with CO_2-doubling.[55] However, in the policy-makers' summary of IPCC'92, the narrower definition is used.

Wang *et al.* showed that the use of an equivalent CO_2 concentration rather than the individual spatio-temporal forcing characteristics of each greenhouse gas may lead to an incorrect assessment of greenhouse warming. In their study they included the greenhouse gases CH_4, N_2O, CFC-11 and CFC-12. They calculated a global surface equilibrium warming of 4.2°C using the equivalent CO_2 concentration, but 5.2°C using increased individual greenhouse gases.[56]

Recent work on the cooling effects of sulphate and other aerosols in GCMs illustrates the various current definitions of climate sensitivity.[57] Such work has questioned the assumption that the mechanisms of response from different sources of radiative forcing would be nearly the same (because different response mechanisms and feedbacks are implicated). It really might not be possible to simplify a multi-causal climate perturbation by means of a single climate sensitivity parameter.[58]

The experts can move between two definitions of climate sensitivity: the narrow one, which is especially suitable for providing a common currency in which policy-makers and other scientists can talk about how a given set of models responds to a specific, policy-relevant, anthropogenic forcing, and the wider one, which includes all sources of radiative forcing and which is a more suitable basis for dialogue with, and for maintaining credibility with, other climate scientists. The flexibility thereby acquired also permits the experts more leeway in accounting for why the 1.5°C to 4.5°C range should not be changed - that is, they can shift from the narrow, CO_2-only definition to the wider definition and introduce aerosols, the historic record, and so on, as reasons why the sensitivity range should remain the same. If the wider definition is used, point (1) on p.11 becomes a more legitimate consideration.[59]

This episode illustrates that within the climate modelling research community, the concept of climate sensitivity is much more complex and indeterminate than is acknowledged in the IPCC reports or other assessments cited, and causes attendant problems concerning the stabilization of the associated temperature range.[60]

Ambiguity regarding what the range of numbers means

As already noted in section 2.3.3, the assessment reports themselves do not indicate how the 1.5°C to 4.5°C range for climate sensitivity should be understood. When we asked Dickinson about this he commented: *'Villach* [1985] *like most committee considerations of this topic could not agree on what the range meant; i.e. whether it was a one-sigma or two-sigma probability range or something else; I expect all would agree it was never intended to be the total range of possibility. That means, I suppose that the numbers could live indefinitely, provided we changed their definition with further understanding. What this all means is that there is no good agreement upon methodology to determine what is the uncertainty range, and that the perceived uncertainty (as opposed to real if such a thing exists) has not changed much in the last 18 years.'* [61]

In delivering the IPCC Statement to the first session of the international negotiations on a climate treaty[62] Bert Bolin clearly used the range to claim that the scientific community is confident that no climate change is an impossibility (see Table 2.3 entry Bolin'95). This claim is much stronger than Dickinson's qualification of the number range, and than the qualification provided by the Villach'85 conference statement (Table 2.3) that: '*(. . .) values outside this range cannot be excluded*'. In similar vein are the earlier-cited quotes of the modellers themselves who referred to the range as an *'educated guess'* or stated simply *'who knows what it is?'*.

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

2.3.5 Multiple functions and uses of climate sensitivity

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

For the GCM modellers themselves, one major use of the concept 'climate sensitivity' is that it serves as a benchmark for intercomparing GCM models.[63] In addition, climate sensitivity (in its wider definition) is implicated in discussions between modellers concerning whether different forcings produce different sorts of responses. This research issue is concerned with the spatio-temporal characteristics of different forcing factors, and the micro-physical properties of the atmosphere. The wider definition of climate sensitivity is in effect a hypothesis that the global equilibrium temperature response at the surface is independent of the source and type of climate forcing. This contrasts strongly with the narrow definition of climate sensitivity, which is not based on these assumptions. Those climate modellers who use simpler models than GCMs also use climate sensitivity as a parameter in their models. They can use the temperature range to compare elements of their models, or to provide ’independent’ evidence for comparison with GCMs. Climate sensitivity is in addition a useful device with which to aggregate diverse bodies of knowledge such as: GCMs, simple climate models, observational data and palaeoclimate data.

In the preparation of assessments of climate change for the policy world, climate sensitivity is a means of summarizing a highly complex field of science in a way that can be easily appreciated by policy-makers. Climate sensitivity provides policy-makers and advisors with a ’window’ onto the world of GCM modelling.

In the impact assessment communities, the temperature range is called a ’three-fold range’: ’high estimate’ (4.5°C), ’best estimate’ (2.5°C), ’low estimate’ (1.5°C)[64]. By the avoidance of a precise definition of what the 1.5°C to 4.5°C range is, and by the use of the ambiguous terms ’high estimate’, ’best estimate’ and ’low estimate’, a broad community of meaning is able to exist across the diverse social worlds involved in the climate issue, whilst at the same time diverse meanings are invested in these uniting concepts by different communities.[65] Climate sensitivity also allows those who use GCM-output - such as the ’climate impact’ community who assess the consequences of climate change on agriculture, hydrology, ecology, and so on - or those who work with GCM modellers by adding new processes to the models - such as ecological modellers and atmospheric chemists - to have a way of indexing the range of different GCMs. It is important to have a simple way of indexing GCMs given that these scientists cannot possibly understand GCMs in all their complexity or be privy to the tacit knowledge which surrounds the models. Climate sensitivity is also a useful indicator and an interpretive resource in research which couples different GCMs together with ecological / chemical models or which uses different GCMs to drive impact models. Integrated Assessment Models (IAMs) (used for scenario studies of the climate problem[66]) combine a simplified climate model with a range of other models - impacts, carbon-cycle, atmospheric chemistry, economics, and so on. IAMs directly use the consensus estimate for climate sensitivity as an input parameter.

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

2.3.6 The 1.5°C to 4.5°C temperature range as an *anchoring device* in the climate debate

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

The experts have to negotiate support and credibility for their assessment reports with both their scientific peer groups and policy ’customers’. Their problem consists of translating scientific knowledge into a form appropriate for policy actors, whilst keeping favour with the surrounding research communities. Star and Griesemer’s concept of boundary-objects is useful here, since it addresses the question of how heterogeneity in the perspectives and practices of the various actors involved in scientific work can co-exist with the co-operation between these actors required for doing assessment.[67] Given that without translation, a particular sort of scientific practice - in this case scientific assessment - will not occur, how is translation achieved without alienating those other actors as a result of over-coercion (which is unlikely to succeed)? One means, Star and Griesemer suggest, is through boundary-objects, these being, for example, relatively stable and reproducible things, people, projects, texts, maps and ideas which facilitate (or make possible) communication between different actors or ’social worlds’. The climate sensitivity range appears to function much like a boundary-object, helping to hold a variety of scientific and policy endeavours together in a common envelope of interpretation whilst more specific meanings emerge for the different constituencies.

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

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

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

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

2) We note that modellers are wary about suggesting that the most recent model calculations are automatically 'better' than earlier ones. However, experience indicates that policy actors often do make this presumption, sometimes with important political repercussions.[68] This consideration relates to the indeterminacy surrounding climate change modelling, which limits the confidence of modellers in any single model run. Hence the assessment community has invested much of their confidence in the consensus range as agreed by previous assessment exercises and been wary of relying too heavily on more recent calculations which are relatively few in number and less analysed by the research community.

3) The emergence of a distinctive analytical framework or paradigm for climate change modelling in the 1970s is a further contribution to the maintenance of the temperature range. The most important three feedbacks within climate models (sea-ice albedo, water vapour / lapse rate, and cloud properties) have persisted since that time despite the addition of more and more processes and complexity. Although climate models have changed enormously, much of the temperature response to a doubling of CO_2 is still produced by the same three feedbacks as were dominant in the early climate models.

4) The need for some consistency in the scenarios of climate change used by the climate impacts community may also contribute to the maintenance of the temperature range (perhaps through the influence of funding agencies).

5) Advisory scientists may also have felt a need to create and maintain a robust scientific basis for policy action, which in our case means a consistent range of climate sensitivities. In many studies of science for policy, this has been seen as the necessary pre-requisite for maintaining the support and credibility from all the actors and social worlds involved. If the apparent scientific rationale for policy were to be too closely tied to new scientific findings, the basis for policy actions could be undermined, especially within the context of a highly politicized and polarized societal debate on the issue at hand (a parallel case being the ozone-depletion issue).[69] [70]

A political demand for scientific consensus and unambiguous quantitative information in the assessment process would be likely to grow as science moves nearer to the context of policy making and the political process surrounding the climate negotiations. Hence, as the scientific assessment process became formalized at the IPCC, and as the climate negotiations have progressed, so the demand from policy makers for the presentation of certainty, consistency and robustness of scientific knowledge may have become more pressing upon scientists. Supporting evidence for this comes from two close observers of the IPCC process: Lanchbery and Victor have argued that the combined effects of the size of the IPCC, the consensus mechanism, and its role of providing balanced scientific judgments, tend to lead to the IPCC rarely making recommendations of a radical nature or reaching conclusions which are at all controversial.[71] We have seen that modellers were clearly reluctant to make strong claims in an IPCC consensus document about the best-estimate, or the likelihood of the sensitivity falling outside of the 1.5°C to 4.5°C range.[72] The experts, taking into account the need for some consistency and certainty, presented the model-derived range of climate sensitivity values as well within the bounds of what can be stated with certainty, whilst treating precise probability statements as lying beyond these bounds. Changes in the ’best guess’ figures and in the implicit (and informal) distribution function absorbed new insights and knowledge without challenging the impression of certainty emerging from the stability of the 1.5°C to 4.5°C range.

Even prior to the emergence of the IPCC assessment process, we found evidence of social pressure against any deviation from the climate sensitivity range in public and policy contexts. In an internal KNMI memorandum prof. C.J.E. Schuurmans provided an account of the Toronto 1988 Conference on the Changing Atmosphere. He ends with some personal impressions, one of which reads: *’Schneider (NCAR) objected to the inclusion in the Conference Statement of the 1.5°C to 4.5°C temperature increase within 50 years. He thought that such an exact estimate was unwarranted in such a report. Yet, the conference was not willing to drop these numbers, as they were adopted from the Villach report, which forms the scientific basis of conferences such as this one. In general, questioning scientific judgments at this conference was not popular.’*[73]

In our view, it is the interplay of the above five elements which accounts for the anchoring of climate sensitivity despite changing knowledge and multiple interpretations, which we understand to be accommodated by the multivalency of boundary-objects.[74] [75]

2.4 Conclusion and discussion

According to the predominant ’classical’ view[76] of science for policy more knowledge means less uncertainty and therefore leads to more policy cohesion. On the other hand, as controversy studies in the science-studies field[77] indicated long ago, more scientific knowledge often leads to more elaborate scientific polarization and conflict, thus to greater, or at least to undiminishing uncertainty.

Policy conclusions and expert interpretations are ’underdetermined’ by any given scientific knowledge because of the repertoire of interpretive possibilities existing at each link in the argumentative chain. New data introduce more flexibility, although negotiated interpretive links, once made, are consolidated as if determined naturally, not by the subtle redefinition of ancillary linkages and meanings. Hence, in the science-policy nexus, scientific knowledge is frequently mutually, inseparably and synchronically constituted with policy responses, policy processes and even policy identities.

In this chapter we have added a new dimension to the role of scientific knowledge in policy by emphasizing the multivalent character of scientific consensus. The tacit constitution of a community of shared meaning regarding particular bodies of knowledge seems to be a key-aspect of the processes by which assessments are made.

We have found that a central scientific concept in the anthropogenic climate change field, namely climate sensitivity (to CO_2-doubling), operates like a boundary-object in the sense first proposed by Star and Griesemer[78]. Boundary-objects hold widely separate communities of different practice together in a larger 'minimalist' shared identity, whilst at the same time allowing local communities to assign their own specific local meanings to these 'common' objects in the boundary.

The international scientific consensus (as achieved by the IPCC and its precursors) about the most likely range of climate sensitivity to CO_2-doubling is multivalent. One might well have expected the estimate of 1.5°C to 4.5°C for climate sensitivity to have changed with fast expanding and developing scientific research. Instead, it has remained remarkably stable over two decades.

We have introduced the concept of an *anchoring device* to describe such a stable boundary-object, which constitutes multivalent consensus knowledge, and interfaces science with policy in a context of scientific and social flux. Anchoring devices seem to function as a means of managing uncertainty in that they prevent the primary scientific case from drifting, and this serves to constrain the discourse. This concept of an *anchoring device* and how it relates to and differs from the concept of *boundary-objects* and Knorr-Cetina's notions of *multivalent consensus*[79] will be discussed below:

- The stable temperature range of 1.5°C to 4.5°C conceals other dimensions of scientific knowledge which were unstable. Our research has shown that these other dimensions are characterized by ambiguities and changes of interpretation and meaning.
- Rather than interpret the mixture of stability and ambiguity in the temperature range as evidence of unscientific imprecision and even duplicity, it seems more appropriate to treat it as an illuminating part of the intrinsic 'underdetermination' of knowledge-claims from given premises, modelling assumptions and observations. Also, this mixture seems a likely property of knowledge shared and developed across open and diverse networks.

Consensus in such networks is much more complex and multi-dimensional than a simple agreement based on shared beliefs and uniform interpretations. Consensus here seems to be better seen in the spirit of Knorr-Cetina's notion of genealogy[80] which involves a shift from 'consensus formation' to *selection* through processes of reconfiguration. Genealogy is used here in the sense of temporal organizations of agreement formation, involving distinctive 'generations' of efforts organized into overlapping sequences. However, drawing on research on experimental high energy physics (HEP), Knorr-Cetina suggested that the nature of 'consensus' may vary from one arena of practice to another, depending upon the 'social ontology' of the arena.

- One key property of the anthropogenic climate change arena in contrast with Knorr-Cetina's HEP arena and Star and Griesemer's zoology museum, is its policy relevance. This seems to add new dimensions to the role of climate sensitivity and the associated temperature range as a boundary-object, which did not figure in the case of the zoology museum pieces analysed by Star and Griesemer. The whole arena in which climate risk assessment takes place is much more politically-charged and controversial, with much greater societal stakes and resources implicated, than in the relative arcane world surrounding the museum of vertebrate zoology. Hence in the climate case, the boundary-object plays a more proactive role and spans different social worlds which are more complex and ill-defined. As a result tacit differences of meaning attributed to the boundary-object may be articulated more explicitly, as more active, detailed and robust co-ordination is sought between diverse areas of practice and interpretation. However, one would expect this process to be limited by counterpressures. For example, the IPCC tries to maintain its legitimacy and credibility with research communities yet at the same time offers usable products to policy communities. The more interfaces across which a boundary-object comes to operate, and the more pro-active a vehicle (for enrolment) it is, the greater the tension may become between the tangential need for differences to be articulated and the need for flexibility. Ultimately this tension could cause the boundary-object to collapse.

- A further point of difference between the role of boundary-objects as introduced by Star and Griesemer and our findings regarding the role of the climate sensitivity range is perhaps a function of the wide coverage of this concept, which may even function as a global cognitive-social plenum. An essential part of this function is the putative construction of an as-yet non-existent community namely a new collective social identity of global agency and responsibility that focuses on greenhouse gas controls, adaptation to projected climate change effects, technology transfer, joint implementation and a range of further social innovations and corresponding identity changes. The 'user communities', to whom scientific assessment has been directed are ambiguous and diffuse, to say the least. The identities of 'user' or 'policy-maker' in relation to global climate policies may never be achieved in reality, but they can be considered as 'imagined communities' shaping the cognitive processes and commitments of the global science-assessment. Anderson's account of colonial authorities' construction and use of maps and censuses showed that the abstract assumptions about societies built into those constructs (e.g., ethnic distinctions, political borders, etc.) eventually began to order them materially, as routine administrative functions began to reflect and consolidate them in the identities and relationships of their subjects.[81] In imagining into being a global policy community, practitioners of global climate science may likewise re-shape the world in the image of their scientific tools and analyses, though the significant sources of resistance are diverse and quite unknown.

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

The remarkable ostensible stability of the climate sensitivity range may play a significant role in holding together a variety of different social worlds in a situation where the state of scientific knowledge does not grant the 1.5 to 4.5°C range a higher scientific status than an 'educated guess'. But the stability can also be seen as a function of an implicit social contract amongst the various scientists and policy specialists involved, which allows 'the same' concept to accommodate tacitly different local meanings. Thus the very multidimensionality of such scientific concepts is their resilience and value in bridging and perhaps reorganizing the differentiated social worlds typically involved in most modern policy issues. The different importance of particular dimensions of the knowledge for different social groups may be one way in which pluralism is held together. However, this multidimensionality can also be regarded as a lack of precision, which is usually seen as antithetical to 'good science'. It can be argued therefore that conventional normative notions of ’good science’ in research cultures may not necessarily be appropriate for science in policy arenas, even though this is typically how these notions have been institutionalized in policy advisory processes.

Two further issues which arise from the kind of analysis given here are worth noting. First the theoretical notion of the *boundary-object*, including the adapted version of it: *anchoring device* in science for policy, presented in this chapter, in its abstract form suggests clear distinctions between the levels at which different identities are engaged with the objects in question. Ideally, no overspills of meaning and ensuing confusion would result from the flexible shifting between different registers, from the perhaps vaguer but functional overall uniting concept e.g. of the climate sensitivity range, to a more detailed local interpretation. However, in practice this flexibility is extremely unlikely without the occurrence of overspills and ’leakages’ of meanings from one social group into those of others. The groups and forms of communication are not in practice so distinct and clear-cut as in the theoretical model. This introduces an additional potential for confusion if a particular interpretation articulated by or for one group, say a particular set of users, is encountered by another social group with different needs, understandings, assumptions and interests. Furthermore, this kind of confusion can involve evasions of responsibility. For example, the expression of climate sensitivity as a quantitative range with upper and lower limits *invites* further questions: what is the best estimate? and is the upper limit the same as the worst case? According to Table 2.1, the worst case seems to be 5.4°C rather than 4.5°C. This difference can have a significant impact on the answer to policy questions such as ’how high should our dikes be to maintain our safety standard to prevent the risk of flooding in the next decades’. These questions are not born of ignorance and immaturity, but are a reasonable step. Who - the policy user, or the scientist-advisor - is responsible for any ensuing misunderstanding of the science, is an open question.

A second implication of the notion of anchoring which we have related to the multivalent boundary-object idea, is a dilemma posed by science in policy arenas. The positive effects of anchoring are that it creates a common plenum within which negotiation of positions beyond the immediate scientific questions can be conducted. Without such anchors there might be no coming together of disparate parties and no negotiation at all, with disintegration of any incipient policy community. A key question becomes, not is it right or is it wrong, but which actors and which forms of argument are enfranchised and which disenfranchised by the use of such a discourse in constraining and shaping the negotiations process.

The scientific claims that anchor the climate debate, may be identified as more ambiguous than a simple literalistic or scientistic reading would suggest, but this may be beside the point when one sees them as anchoring the negotiated construction of a global policy identity, with corresponding senses of natural responsibility and engagement. However the negative side of this flexible and multivalent constitution is not only the room for confusion which it brings, but also its capacity to constrain the terms of the policy debate.

Paradoxically, the very capaciousness of the scientific concept to accommodate multiple interpretations as a good boundary-object between diverse social groups also has a reverse. This same property may lull the policy consciousness from recognizing issues beyond what are in essence variations around the existing expressed range of sensitivity. For example the very success of the 1.5°C to 4.5°C range, including the arguments about where the best estimate value is, what the margins mean, etc., may focus attention on possible variations and regional differentiations within this frame, but defocus attention from more troubling questions, such as the possibility of more abrupt changes in climate as it passes through possible critical thresholds, and whether we have really addressed, or presumed the answer to the original climate research question, which was *whether* long term climate processes and human interferences with it, are predictable. These kinds of question exist in the margins of the current climate scientific debate, but are screened out of the assessments and representations of the science for policy; as they would upset the existing commitment by some (though by no means all) policy actors and analysts to the idea of smooth and manageable forms of anthropogenic climate change. The more fundamental scientific questions could well sustain, and be sustained by, a different more urgent and radical public policy agenda. In such an agenda the climate sensitivity would not necessarily be an anchoring concept. Thus anchoring and the flexible quality of conceptual boundary-objects have a double-edged character which it may be important, not only to acknowledge (after all it is akin to Kuhn’s notion of the double-edged quality of scientific paradigms in normal science), but systematically to examine, for example for their deeper cultural dimensions and forms of reinforcement.

2.5 Acknowledgements

This chapter has benefited from the discussions held in the Risk Assessment working group, chaired by Jill Jäger, during the 1993 summer meeting in Wuppertal of the project ’Social Learning in the Management of Global Environmental Risks’, coordinated by Bill Clark (Harvard University). We would like to thank Wim Turkenburg, Jill Jäger, Stephen Healy, Kaat Schulte Fischedick, Anne Stijkel and further Paul Edwards, Stephen Schneider and two anonymous reviewers for useful comments on earlier versions of this chapter. We would like to thank Nancy Dickson, Mick Kelly, John Mitchell, Brian Flannery, Mike Schlesinger, Joyce Penner, Robert Dickinson, Lawrence Gates and Bert Bolin for co-operating with our research and for providing a look behind the scenes of the assessment reports.

2.6 Notes

1. Edward A. Parson, ‘Integrated Assessment and Environmental Policy Making, in Pursuit of Usefulness', *Energy Policy*, Vol. 23 (4-5, 1995) 463-475.
2. U.S. National Academy of Sciences, *Carbon Dioxide and Climate: A Scientific Assessment* (Climate Research Board, U.S. National Academy of Sciences, Washington D.C., 1979).
3. U.S. National Academy of Sciences, *Changing Climate, Report of the Carbon Dioxide Assessment Committee* (Board on Atmospheric Sciences and Climate, Commission on Physical Sciences, Mathematics and Resources, National Research Council, Washington D.C., 1983).
4. Bert Bolin, Bo R. Döös, Jill Jäger and Richard A. Warrick (eds.) *The greenhouse effect, climatic change and ecosystems*, (SCOPE 29 John Wiley & Sons, Chichester, 1986).
5. The World Conference on the Changing Atmosphere: Implications for Global Security.
6. John T. Houghton, G.J. Jenkins and J.J. Ephraums (eds.), *Climate Change, The IPCC Scientific Assessment* (Cambridge University Press, 1990).

[7.] John T. Houghton, B.A. Callander and S.K. Varney (eds.) *Climate Change 1992 The Supplementary Report to the IPCC Scientific Assessment* (Cambridge University Press, 1992).
[8.] John T. Houghton, L.G. Meira Filho, J. Bruce, Hoesung Lee, B.A. Callander, E. Haites, N. Harris and K. Maskell (eds) *Climate Change 1994 Radiative Forcing of Climate Change and An Evaluation of the IPCC IS92 Emission Scenarios* (Cambridge University Press, 1995).
[9.] John T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change* (Cambridge University Press, 1996).
[10.] For the scientific history of the greenhouse problem which focuses on the background of the successive assessments, see the comprehensive article on this subject which we have drawn upon here: David G. Victor and William C. Clark, *The Greenhouse Effect in the US: A History of Science up to 1985*, Contribution I-2 on the Project on Social Learning in the Management of Global Environmental Risks (1991).
[11.] David Collingridge and C. Reeve, *Science Speaks to Power: The Role of Experts in Policy-Making* (London: Frances Printer, 1986).
[12.] Scientific disagreement and political disagreement are coupled: political disagreement gives rise to articulation of scientific uncertainties, see e.g. Arie Rip, 'Risicocontroverses en de Verwevenheid van Wetenschap en Politiek', *Kennis en Methode*, (1992) 1, 63-80, also cf. B.L. Campbell, 'Uncertainty as Symbolic Action in Disputes Among Experts', *Social Studies of Science*, Vol. 15 (1985), 429-53.
[13.] Sonja Boehmer-Christiansen, 'Global Climate Protection Policy: the Limits of Scientific Advice' part 1, *Global Environmental Change*, Vol. 3 (1993), part 2, *Global Environmental Change*, Vol. 4 (1994) 185-200.
[14.] David M. Hart and David G. Victor, 'Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74', *Social Studies of Science*, Vol. 23 (1993), 643.
[15.] The concept *policy windows* has its roots in: Anthony Downs, 'Up and down with ecology - the Issue Attention Cycle', *The Public Interest*, 28 (1972), 38-50; and has been further defined by : J.W. Kingdon, *Agendas, Alternatives and Public Policies*, (Little, Brown and Company, Boston/Toronto, 1984).
[16.] Josee C.M. van Eijndhoven and Peter Groenewegen, 'The Construction of Expert Advice on Health Risks', *Social Studies of Science*, Vol. 21 (1991), 257-78.
[17.] Such connections and meanings are contingently constructed, as indeed Rosenberg noted much earlier (C.E. Rosenberg, 'Scientific Theories and Social Thought', in: B. Barnes (ed.), *Sociology of Science: Selected Readings*, (Penguin Books, Harmondsworth, Middlesex, 1972), 292-306) in the context of diametrically opposite 19th Century and 1960s political connotations of hereditarian and 'environmental' scientific views of the causes of criminal behaviour.
[18.] Michel Callon, John Law and Arie Rip (eds.), *Mapping the Dynamics of Science and Technology* (London: Macmillan, 1986); Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA: Harvard University Press, 1987); Bruno Latour, Philippe Mauguin and Geneviève Teil, 'A Note on Socio-Technical Graphs', *Social Studies of Science*, Vol. 22 (1992), 33-57.
[19.] Susan Leigh Star and James R. Griesemer, 'Institutional Ecology, "Translations" and coherence: Amateurs and professional in Berkeley Museum of Vertebrate Zoology, 1907-39.' , *Social Studies of Science*, Vol. 19 (1989), 387-420.
[20.] Sheila S. Jasanoff, 'Contested Boundaries in Policy Relevant Science', *Social Studies of Science*, Vol. 17 (1987), 195-230; Sheila S. Jasanoff, *The Fifth Branch: Scientific Advisers as Policymakers* (Cambridge, Mass.: Harvard University Press, 1990).
[21.] E.g. Tom M.L. Wigley, 'Climate Scenarios', in: *European Workshop on Interrelated Bioclimatic and Land Use Changes*, Noordwijkerhout, The Netherlands, October 17-21, 1987, 52; or: Houghton *et al.*, 1990 op. cit. note 6.
[22.] Houghton *et al.* 1990, op. cit. note 6, xxvi.

[23] The radiative forcing is defined as the net effect of a greenhouse gas on the average net radiation at the top of the troposphere, caused by a change in either solar or infrared radiation. A radiative forcing perturbs the balance between incoming and outgoing radiation (Houghton *et al.*, 1995, op. cit. note 9).
[24] As will be shown later in this chapter, some recent models do include the anthropogenic sulphate aerosols.
[25] Mike Schlesinger and John Mitchell, 'Climate Model Simulations of the Equilibrium Climate Response to Increased Carbon Dioxide', *Review of Geophysics*, Vol. 25 (1987), 760-98.
[26] W. Lawrence Gates, 'The Earth's Climate System', *WMO Bulletin*, Vol. 41, 2 (1992), 413-20.
[27] Gordon A. McBean, 'Global Change Models - A Physical Perspective', *Ambio*, 23**,** 1, (1994), 13-18.
[28] Other GCM modelling groups whose results are cited in the assessment reports are: Oregon State University (OSU), the Main Geophysical Observatory in Leningrad (MGO), Commonwealth Scientific & Industrial Research Organization, Australia (CSIRO) and the Japanese Meteorological Research Institute (MRI).
[29] U.S. National Academy of Sciences, 1979, op. cit. note 2.
[30] Robert E. Dickinson, *How Will Climate Change? The Climate System and Modelling of Future Climate*, 262 in: Bolin *et al.* (eds.), 1986 op. cit. note 4.
[31] Bolin *et al.* (eds.), 1986, op. cit. note 4, xxi.
[32] E-mail message from Robert Dickinson (April 25, 1995).
[33] In particular, the skewed distribution of the estimates of climate sensitivity, due to the amplified positive feedbacks at the higher sensitivities, was not properly taken into account by Dickinson according to some modellers.
[34] M. Bruce Beck, 'Understanding Uncertain Environmental Systems', in: J. Grasman and G. van Straten, *Predictability and Nonlinear Modelling in Natural Sciences and Economics* (Kluwer, Dordrecht, 1994), 294-311.
[35] Houghton *et al.* (eds.). 1990, op. cit. note 6.
[36] For this reason it was selected as the starting point for scenarios used in the study for the European Workshop on Interrelated Bioclimatic and Land Use Changes, October 1987, which was one of the follow-up conferences from Villach'85, see: Wilfried Bach, 'Scenario Analysis', *European Workshop on Interrelated Bioclimatic and Land Use Changes*, (Noordwijkerhout, The Netherlands, October 17-21, 1987); Jaap Kwadijk, 'Central Discussion Paper', *European Workshop on Interrelated Bioclimatic and Land Use Changes*, (Noordwijkerhout, The Netherlands, October 17-21, 1987).
[37] W. Lawrence Gates, Ann Henderson-Sellers, G.J. Boer, C.K. Folland, A. Kitoh, B.J. McAvaney, F. Semazzi, N. Smith, A.J. Weaver and Q.-C. Zeng, 'Climate Models - Evaluation', in Hougton *et al.*, 1996, op. cit. note 9.
[38] J.F.B. Mitchell, C.A. Senior and W.J. Ingram, 'CO_2 and climate: a missing feedback?', *Nature*, Vol. 341 (14 September, 1989), 132-34.
[39] W. Lawrence Gates, John F.B. Mitchell, G.J. Boer, U. Cubasch and V.P. Meleshko, 'Climate Modelling, Climate Prediction and Model Validation', 116 and 117 in: Houghton *et al.* (eds.) 1992 op. cit. note 7.
[40] Houghton *et al.*, 1992 op. cit. note 7, 111, table B2.
[41] Gates *et al.*, 1992, op. cit. note 44, 118.
[42] Ibid. 118.
[43] Ibid. 118.
[44] E-Mail message from W. Lawrence Gates (May 18, 1994).
[45] Fax message from Bert Bolin (August 17, 1994).
[46] Houghton *et al.*, (eds.) 1996 op. cit. note 9, 34.
[47] This same source claims that the modeller who came up with the 80% probability value did so by canvassing the opinions of 8 modellers, 5 or whom replied. 4 said that the sensitivity would fall

into the 1.5°C to 4.5°C range, 1 said not. Hence the 20%.

48. See for instance M. Granger Morgan and David W. Keith, Subjective Judgements by Climate Experts', *Environmental Science and Technology*, Vol. 29 (1995) 468A-476A, and: James G. Titus and Vijay Narayanan, The Risk of Sea Level Rise, A Delphic Monte Carlo Analysis in which Twenty Researchers Specify Subjective Probability Distributions for Model Coefficients within their Respective Areas of Expertise, *Climatic Change*, Vol 33 (1996), 151-212.

49. Bert Bolin, *IPCC Statement to the first session of the Conference of Parties to the UN Framework Convention on Climate Change* (Berlin, 28 March, 1995).

50. e.g. as defined by Jeffrey T. Kiehl, in Kevin E. Trenberth (ed.), *Climate System Modeling*, (Cambridge University Press, 1992), 321.

51. Robert D. Cess and Gerald L. Potter, 'A Methodology for Understanding and Intercomparing Atmospheric Climate Feedback Processes in General Circulation Models', *Journal of Geophysical Research*, Vol. 93 (D7, 1988) 8305-8314.

52. Ibid.

53. We find increasingly that the scientific literature uses the IPCC's definition of climate sensitivity. The emergence and spread of the concept in the early 1980s seems to have been of importance to the current framing of the anthropogenic climate change discourse, and vice versa.

54. for instance on page 423 of Houghton *et al.*, 1996 op. cit note 9.

55. Houghton *et al.*, (eds.), 1992 op. cit. note 7, 118.

56. W.C. Wang, M.P. Dudek, X.-Z. Liang and J.T Kiehl, 'Inadequacy of effective CO_2 as a proxy in simulating the greenhouse effect of other radiatively active gases', *Nature*, Vol. 350 (1991) 573-577.

57. e.g. K.E. Taylor and J.E. Penner, 'Response of the Climate System to Atmospheric Aerosols and Greenhouse Gases', *Nature*, Vol. 369 (30 June 1994) 734-37.

58. See also: Tom M.L. Wigley, 'Outlook becoming hazier', *Nature*, Vol 396, (30 june 1994), 709-708.

59. The recent IPCC'94 report also uses a wider definition in the full scientific text, see pages 169 & 170 op. cit. note 8.

60. In personal communication (July 4, 1994), Mike E. Schlesinger stressed the underdetermined character of the climate sensitivity: 'The number of unknowns is much greater than the number of independent pieces of information we have.'

61. op. cit. note 32.

62. More precisely, the Conference of Parties to the UN Framework Convention on Climate Change, held in Berlin, on 28 March, 1995.

63. e.g. IPCC 1990 p. xxv: *'....climate sensitivity .. is generally used as a benchmark to compare models'*, op. cit. note 6.

64. Note that a translation of the 'best guess' into the 'best estimate' has taken place in such uses of climate sensitivity, implying greater certainty in the figure.

65. A remaining issue concerns the extent to which the above sources of flexibility arise from linguistic imprecision, i.e. the lack of use of an unambiguous definition of climate sensitivity, rather than the lack of existence of one (as argued in another context by M. Granger Morgan and Max Henrion, *Uncertainty, a Guide to Dealing with Uncertainty in Quantitative Risk and Policy Analysis* (Cambridge University Press, Cambridge, 1990)). If scientists kept to more precise definitions of climate sensitivity, then it should in principle be possible to reduce the interpretive flexibility from sources 4 and 5 (and to a less extent 2 and 6). The fact that more precision is not sought for by a tightening-up of definitions, however, does not reflects a conscious desire by advisory scientists to maintain interpretive flexibility in order to facilitate boundary interactions. Such would imply too much conscious and instrumental strategy on the part of the advisors. Rather, it reflects the hypothetical, hence pre-definitional, character of climate sensitivity from a climate research perspective. Linguistic precision may be rejected because it would commit scientists to the notion that climate sensitivity, in its wider formulation, is actually a 'proven' scientific concept.

The IPCC can give the appearance of exercising linguistic precision because the narrow definition it uses is purely operational, as befits ’science for policy’ perhaps, though as we have seen the IPCC trades heavily upon definitional flexibility.

66. e.g. the Integrated Model to Assess the Greenhouse Effect (IMAGE), developed at the (Dutch) National Institute of Public Health and Environmental Protection (RIVM) and the Atmospheric Stabilization Framework (ASF) developed at the (United States) Environmental Protection Agency (EPA).

67. Star and Griesemer, 1989 op. cit. note 19.

68. For example, President Bush’s science adviser, Allan Bromley, used the UKMO’s 1989 model calculations, which produced a climate sensitivity of 1.9K by inclusion of water phase change in the representation of clouds, as a reason for questioning the robustness of the scientific basis for human-induced climate change. According to an article in Science on 3 November 1989: "Asked by Gore whether the scientific evidence is inadequate to justify curbs on greenhouse emissions, Bromley responded that recent adjustments to climatic models by British climate modeller John Mitchell have made him uneasy about the reliability of predictions (. . .)’That such simple and obvious changes in [Mitchell’s] model can make major changes in predictions underscores my own feeling (. . .) that we have a substantial distance to go yet’". This article prompted a British official to write to a colleague in government that: "It is rather frustrating to see John [Mitchell]’s results being used in this way. (. . .) John places no more confidence in the 1.9K result than the 5.2K result". This sort of experience of how the latest results of model simulations are widely received by influential policy makers such as Bromley may be quite important in how modellers and other advisors have come to represent the consensus view of the climate sensitivity.

69. See also: Y. Ezrahi, ’Utopian and Pragmatic Rationalism: The Political Context of Scientific Advice’, *Minerva*, Vol. 18 (1980), 111-31; Arie Rip, ’Expert Advice and Pragmatic Rationality’, in N. Stehr and R.V. Ericson (Eds.); *The Culture and Power of Knowledge, Inquiries into Contemporary Societies*, (Walter de Gruyter: Berlin, 1992).

70. W.J. Kakebeeke, official of the Dutch Ministry on Housing, Physical Planning and the Environment, in a lecture at 280th scientific meeting of RIVM (Netherlands National Institute of Public Health and Environmental Protection) (24 March 1994).

71. John Lanchbery and David Victor, ’The Role of Science in the Global Climate Negotiations’, in: H.O. Bergesen and G. Parmann (eds.), *Green Globe Yearbook of International Co-operation and Development* (Oxford University Press, 1995) pp. 29-40.

72. There is even circumstantial evidence to suggest that modellers were dissuaded from staking-out claims based on intuitive and tacit judgements because of fears that some industrial scientists might criticise the absence of robust methodology.

73. KNMI memorandum DM-88-12, July 1988: original Dutch quote: ’Schneider (NCAR) maakte nog bezwaar tegen het opnemen van de getallen 1,5-4,5°C temperatuurstoename binnen 50 jaar, in het conference statement - hij vond die precisie in zo’n rapport niet verantwoord - maar de conferentie was niet bereid deze getallen te laten vallen, want ze staan in het Villach rapport en dat vormt de wetenschappelijke basis van conferenties als deze. Het aanvechten van wetenschappelijke uitspraken was op deze conferentie in het algemeen niet populair.’

74. We emphasize that it would be misleading to emphasize the role of policy too much, for three reasons: First there is a lack of supporting empirical direct evidence; Second, there exist alternative interpretations of the IPCC from that presented above (e.g. regarding the IPCC as making rather bold claims, as in the some what controversial 1995 report); And third, it is questionable whether relatively minor changes in the temperature range might have had significant de-stabilising effect upon climate policy-making and the international negotiations.

75. We thank Stephen Schneider for raising this issue.

76. Brian Wynne, ’Carving out Science (and Politics) in the Regulatory Jungle’, *Social Studies of Science*, Vol. 22 (1992), 745-58.

[77] e.g. Dorothy Nelkin, 'The Political Impact of Technical Expertise', *Social Studies of Science*, Vol. 5 (1975), 35-54.
[78] Star and Griesemer, 1989, op. cit. note 19.
[79] Karin Knorr-Cetina, 'How Superorganisms Change: Consensus Formation and the Social Ontology of High Energy Physics Experiments', *Social Studies of Science*, Vol. 25 (1995), 119-47.
[80] Ibid.
[81] Benedict Anderson *Imagined Communities*, (New Left Books, London, 1981, revised and extended 1991).

J.P. van der Sluijs and J.C.M. van Eijndhoven (1998), Closure of Disputes in the Assessments of Climate Change in the Netherlands Arena Environmental Management, 22 (4), .597-609.

Closure of Disputes in the Assessments of Climate Change in the Netherlands Arena[1]

3.1 Introduction
3.2 The closure time lines
3.3 The construction of the Gezondheidsraad assessments
3.3.1 The construction of the Gezondheidsraad estimate of Climate Sensitivity
3.3.2 The CO_2 focus of the Gezondheidsraad
3.4 Discussion and Conclusion
3.5 References

Abstract

This chapter presents an analysis of the closure of visible disputes in the assessments of climate change in the Netherlands with regard two key constituents of the assessments: the estimate of climate sensitivity and the inclusion of non-CO_2 greenhouse gases in assessment studies. For the cases studied, we identify variability in the assessment reports in the Netherlands in the pre-IPCC period. In the Netherlands arena, the assessments in this period can be seen as exponents of two different lines, a Netherlands line and an International line. We seek to identify what factors were decisive in the selection processes that resulted in the closure of visible disputes (visible in or across the assessment reports) for both cases. Our analysis reveals a remarkable difference in the adoption behaviour of two Dutch assessment groups, despite a large overlap in membership. We provide evidence that it is not the paradigmatic predisposition of the experts in the committee that was decisive for the closure of visible disputes, but it was the context in which the experts operated and the commitments they had made in each setting.

3.1 Introduction

Anthropogenic climatic change is a relatively new area of research. In this field, experts started drafting assessment reports for policy-makers when research on anthropogenic climate change was still in an early stage of development. Assessment is the analysis and review of information derived from research for the purpose of helping someone in a position of responsibility to evaluate possible actions or think about a problem. Assessment usually does not mean doing new research. Assessment means assembling, summarizing, organizing, interpreting,

[1] This chapter was written as a co-authored paper: J.P. van der Sluijs and J.C.M. van Eijndhoven, Closure of Disputes in the Assessments of Climate Change in the Netherlands Arena (submitted to *Environmental Management*).

and possibly reconciling pieces of existing knowledge, and communicating them so that they are relevant and helpful for the deliberations of an intelligent but inexpert policy-maker (Parson, 1995).

The current record of assessment reports on anthropogenic climate change, covers about two decades. In this period closure of visible disputes can be identified on a number of key constituents of the climate risk assessments (Jäger *et al.*, forthcoming). These closures appear primarily in the international climate risk assessment community that emerged in the eighties and diffused from there to the national arenas. The emergence of an international assessment community led to an important conference on the climate change problem in Villach, 1985 and resulted in the establishment within the United Nations system of the Intergovernmental Panel on Climate Change (IPCC) in 1988. Nowadays the IPCC is the leading forum that carries out - and brings about closure in - climate risk assessment. Fourteen Dutch scientists contributed to the 1990 report by IPCC Working Group I (Houghton *et al.*, 1990). In the pre-IPCC period Netherlands expert committees carried out their own climate risk assessments. The most important pre-IPCC assessment group in the Netherlands was the CO_2 committee of the Gezondheidsraad (Netherlands Health Council). This committee had relatively weak links with the emerging international assessment community. For the elements studied, the assessments by the Gezondheidsraad differ significantly from the assessments made by the international community in that period. This situation changed with the preparation and publication of the IPCC 1990 report. Therefore the climate change assessment studies in the Netherlands constitute an excellent case by which to investigate mechanisms leading to the closure of visible disputes in science for policy.

For the purpose of this study we define closure of visible disputes as the achievement of consensus among the assessment community in the arena concerned. In the strict sense of the word, consensus means agreement by all. However, agreement by all would be lethal to the science process and is inconsistent with the very nature of science as a process of putting question marks rather than exclamation marks. Funtowicz and Ravetz (1990) have designed an operational yard-stick for colleague consensus in research practice that does justice to this notion. This yard-stick for consensus is part of the so-called pedigree matrix for research proposed by Funtowicz and Ravetz, which is a tool to identify the strength or *robustness* of science-based information in terms of social and cognitive criteria such as theoretical structure, data input, peer acceptance and colleague consensus (see Table 3.1). In this view, consensus is a social component of robustness.[1]

---

[1] It should be noted that the social and cognitive dimensions of robustness are not independent of each other: consensus formation clearly is facilitated by achievement of strength in the cognitive dimensions of robustness and vice versa. According to Everdingen (1988), the scientific foundations are the corner-stone of consensus formation; he also stresses the reciprocity between the actors involved in consensus formation and the consensus knowledge that is formed. Star (1988) stresses the importance of the aggregation of viewpoints in the achievement of robustness: *"Each actor, site or node of a scientific community has a viewpoint, a partial truth consisting of local beliefs, local practices, local constants, and resources, none of which are fully verifiable across all sites. The aggregation of all viewpoints is the source of robustness in science."* Rip (1991) defines expert advice to be robust if it is not easy to undermine. According to Rip, robustness is a hard-won achievement, and it is not simply the outcome of trying to be 'objective' all the time. Rip argues that pragmatic rationality is crucial in the achievement of robustness. Robustness increase is the driving force of the hybrid social cognitive process of assessment.

Code	Peer acceptance	Colleague consensus
4	total	all but cranks*
3	high	all but rebels
2	medium	competing schools
1	low	embryonic field
0	none	no opinion

*. "Rebels" have some standing among their colleagues, whereas "cranks" have none. Who is a "crank and who a "rebel" may be time bound (Funtowicz and Ravetz, 1990).

Table 3.1 The social phase of the pedigree matrix for research as proposed by Funtowicz and Ravetz (1990). Note that the process of closure on answers inferred from scientific research does not necessarily follow this scale linearly from low to high, and that once the upper end of the scale is reached this is not necessarily the definite end-point, because closure can always be followed by re-opening.

In this study we have opted for an operational definition of closure on the level of the assessment reports. That is, we speak of closure if we observe the emergence of consensus over time across the various assessment reports produced in successive periods. We operationalize consensus on the level of the reports produced by the assessment communities, rather than on the level of the assessment communities as such. We identify the closure process in terms of increase in level of consensus, which in turn we derive from comparing statements in the existing assessment reports in succeeding time periods. We assume that the level of consensus in the assessment community is reflected in its reports. Expressed in terms of the scale of Funtowicz and Ravetz, our operationalization of consensus is presented in Table 3.2. In terms of this operationalization, closure on an element of the assessment is reached at the moment in time when visible inter-assessment variability regarding that element has disappeared.

code	Indicator for Level of Consensus
4	absence of inter-assessment variability
3	minority views are mentioned explicitly in the assessment reports
2	reports can be grouped as exponents of a limited amount of different views
1	ad hoc assessment-initiatives/large inter-assessment variabilities
0	absence of assessment reports

Table 3.2 Our operationalization of Funtowicz and Ravetz' scale for consensus on the level of assessment reports.

In this chapter we analyse the closure process in the Netherlands arena for two cases: the estimate of climate sensitivity (namely the range 1.5°C to 4.5°C) and the inclusion of non-CO_2

greenhouse gases in the assessments.[1] For this purpose we investigated the time series of Netherlands climate risk assessments against the background of the closures in the international time series of assessments.

For the international arena, the successive reports of interest are the assessments produced by the US National Academy of Sciences (US-NAS) from 1979 and 1983; the report of the 1985 Villach 'Conference on the Assessment of the role of Carbon Dioxide and of Other Greenhouse Gases in Climate Variations and Associated Impacts', the reports by the German Enquete Kommission from 1988 and 1990 and the report by Working Group I from the Intergovernmental Panel on Climate Change (IPCC) from 1990 and its supplements from 1992 and 1994, and finally IPCC's Second Assessment Report of 1995 (US-National Academy of Sciences, 1979; 1983; Bolin *et al.*, 1986; Deutsche Buntestag Enquete Kommission, 1988; 1990; Houghton *et al.*, 1990; Houghton *et al.*, 1992; Houghton *et al.*, 1994; Houghton *et al.*, 1995). All these reports had an international impact and have been cited in policy documents.

For the scientific history of the greenhouse problem figuring on the background, we have studied several comprehensive articles on this subject (Victor and Clark, 1991; Jones and Henderson-Sellers, 1990; Handel and Risbey, 1992; Victor, 1995; Hecht and Tirpak, 1995) and several review articles of the climate problem, extended with scientific publications by, and personal communication with, the experts that carried out the Dutch assessments.

In the Netherlands arena six different advisory groups have issued assessments of the climate problem. These are the Scientific Council for Government Policy (WRR), the National Steering Group Environmental Research (LASOM), the Gezondheidsraad, The Netherlands Advisory Council for Research on Nature and Environment (RMNO), the National Institute of Public Health and Environmental Protection (RIVM) and the Central Council for Environmental Hygiene (CRMH) (see Table 3.3). From two groups the assessments have explicitly been used in Dutch policy documents (see Table 3.5): the assessments by the Gezondheidsraad[2] of 1983 and 1986, and the assessment by the RMNO[3] of 1984 (see also Van Eijndhoven *et al.*, forthcoming).

The central question in this chapter is how the closure on the estimates of climate sensitivity and on the inclusion of non-CO_2 greenhouse gases in the assessments of climate change, took place in the Netherlands arena. In section 3.2 we show that closure occurred in the Netherlands arena by diffusion of the closure reached in the international arena. This diffusion process took

---

1 The construction of the estimate of climate sensitivity in the international series of assessments has been analysed in chapter 2 of this thesis. The inclusion of non-CO_2 greenhouse gases in the international assessments is described by Jäger *et al.*, 1997.

2 The Gezondheidsraad is an influential standing advisory body that was set up under the 1956 Health Act to assist the Netherlands government. Its function is to provide the Netherlands Government with objective information on scientific developments on all matters relating to health and environmental protection. Reports are made by ad hoc committees of experts, appointed by the President of the Council.

3 The RMNO was set up in 1981 and is one of the so-called "sector counsels" (in Dutch: "sectorraden") that were formed in the Netherlands in the eighties. Sector councils are advisory bodies dealing with the programming of research for a medium-term period. They advise the government and the relevant ministries. The RMNO focuses on research on nature and environment. In contrast to the Gezondheidsraad, committees of the RMNO are composed not only of scientists but also of policy-makers and representatives of NGOs.

several years. In the transition period inter-assessment variability was observed. For the two cases studies, the most important assessment reports constructed in the Netherlands (the ones by the Gezondheidsraad of 1983 and 1986) deviated significantly from the international line and from another influential Netherlands assessment (by the RMNO, 1984) which did adopt the results from the international arena.

In section 3.3 of this chapter, the central questions are why it took significantly longer to reach closure in the assessments in the Netherlands arena than in the international arena, and what we can learn from the different modes of conduct of the committees of the Gezondheidsraad and the RMNO respectively. Therefore we analysed how the Gezondheidsraad assessment reports were constructed. We extended our analysis of the assessment reports with an analysis of the minutes of the meetings of the committee who wrote the Gezondheidsraad reports. We gathered additional information from interviews and personal communication with several experts who were involved in the assessments.

3.2 The closure time lines

Table 3.3 presents the closure time lines for the estimate of climate sensitivity in the assessments in the international arena and in the Netherlands arena. Note that all international assessments played a role in the Netherlands arena, whereas none of the Netherlands assessments played a role in the international arena[1]. The climate sensitivity is a key quantity in the assessments. It indicates the global mean equilibrium temperature rise associated with an instantaneous doubling of the atmospheric CO_2 concentration and acts as a highly aggregated simplified quantitative summary of the outcome of complex scientific studies (see chapter 2). The table shows that closure took place in the international arena in 1983 with the US-NAS’83 assessment (see also chapter 2 of this thesis). In the Netherlands it was not until 1990 before closure was reached. In the pre-IPCC period we observed inter-assessment variability. The assessments of climate sensitivity in this period can be divided into two lines: the line of the Gezondheidsraad and the line of the international community on climate change. What actually happened was that the international line superseded the Gezondheidsraad line, up from 1990 when the first IPCC assessment report was issued.

[1]. This holds despite the fact that the findings of the first report of the Gezondheidsraad were summarized in an article in *Ambio* (Hekstra, 1986), and the second report of the Gezondheidsraad was translated into English (Gezondheidsraad, 1987).

Assessment	Corresponding reference	Arena	estimate of climate sensitivity (°C)	estimate adopted from	level of consensus (codes: see Table 3.2)	
					I	NL
WRR'78	Schuurmans, 1978	NL	-			1
US-NAS'79	U.S. National Academy of Sciences, 1979	I / NL	1.5-4.5		1	1
LASOM'79	LASOM, 1979	NL	2-3			1
US-NAS'83	U.S. National Academy of Sciences, 1983	I / NL	1.5-4.5		4	1
GR'83	Gezondheidsraad, 1983	NL	2			1
RMNO'84	RMNO, 1984	NL	1.5-4.5	US-NAS'83		2
Villach'85	Bolin *et al.*, 1986	I / NL	1.5-4.5		4	2
GR'86	Gezondheidsraad, 1986	NL	2-4			2
RIVM'87	De Boois *et al.*, 1987	NL	1.5-4.5	Villach'85		2
DBEK'88	Deutscher Bundestag Enquete Kommission, 1988	I / NL	1.5-4.5		4	2
CRMH'88	CRMH, 1988	NL	1.5-4.5	RIVM'87		2
IPCC'90	Houghton *et al.*, 1990	I / NL	1.5-4.5		4	4
DBEK'90	Deutscher Bundestag Enquete Kommission, 1988	I / NL	1.5-4.5		4	4
IPCC'92	Houghton *et al.*, 1992	I / NL	1.5-4.5		4	4
IPCC'94	Houghton *et al.*, 1995	I / NL	1.5-4.5		4	4
IPCC'95	Houghton *et al.*, 1996	I / NL	1.5-4.5		4	4

Table 3.3 The estimates for climate sensitivity in assessment reports in the international (I) and the Netherlands (NL) arenas. Note that the indicated level of consensus in each arena in the right hand columns is a meta-measure of the period in which the reports were drafted rather than a measure of the individual reports.

Table 3.4 presents the closure time lines for the inclusion of non-CO_2 greenhouse gases (non-CO_2 GHGs) in the assessments. The non-CO_2 GHGs, such as CH_4, N_2O and CFCs, started to get attention in the assessments in the second half of the eighties. In scientific publications, the non-CO_2 greenhouse gases showed up significantly earlier. In 1975 V. Ramanathan discovered the greenhouse effect of CFCs (Victor and Clark, 1990). The first WMO statement on the greenhouse effect of CFCs dates from 26 November 1975 (WMO, 1975). The significance of the greenhouse effect of anthropogenic CH_4 and N_2O was recognized in 1976 (Jäger *et al.*, forthcoming). It was also known that human activities influenced the atmospheric concentrations of these gases. It took more than ten years for the non-CO_2 GHGs to get a place in the assessments. Table 3.4 lists the non-CO_2 GHGs that were mentioned in the assessment reports analysed.

The US-NAS study of 1978 dealt with CO_2 only. The US-NAS'83 report included a small chapter on "Effects of non-CO_2 greenhouse gases". The Villach conference in 1985 was the first assessment that comprehensively addressed the non-CO_2 GHGs. This conference brought closure regarding the inclusion of non-CO_2 GHGs in the assessments into the international arena. In the IPCC assessment of 1990 and its supplements of 1992 and 1994, the non-CO_2 greenhouse gases have a prominent place within the assessments.

In the Netherlands arena we see the same pattern as in the climate sensitivity case. In the pre-IPCC period we observe inter-assessment variability. Again the assessments in that period can be divided into the Gezondheidsraad line and the international line. Again closure occurred because the international line superseded the Gezondheidsraad line after 1990 when the first IPCC assessment report was issued.

Assessment	Arena	Non-CO_2 greenhouse gases mentioned	M=Mentioned qualitatively A=Assessed quantitatively	level of consensus (codes: see Table 3.2)	
				I	NL
WRR'78	NL	none			1
US-NAS'79	I / NL	none			1
LASOM'79	NL	H_2O[1], O_3 (stratospheric, indirect)[2]	M		1
US-NAS'83	I / NL	N_2O, CH_4, O_3, CFCs	M (A)	1	1
GR'83	NL	none			1
RMNO'84	NL	CH_4, N_2O, CFC-11, CFC-12, CFC-22, CCl_4, CF_4, CH_3Cl_3, CH_2Cl_2, $CHCl_3$, CH_3CCl_3, C_2H_4, SO_2, NH_3, O_3 (tropospheric), H_2O (stratospheric)	A		1
Villach'85	I / NL	CH_4, N_2O, CFCs, O_3, (tropospheric)	A	4	2
GR'86	NL	CH_4, N_2O, CFCs, O_3, (tropospheric)	M		2
RIVM'87	NL	CH_4, N_2O, CFCs, O_3, (tropospheric)	A		2
DBEK'88	I / NL	CH_4, N_2O, aerosols, tropospheric O_3, CO, stratospheric H_2O, CFC-11, CFC-12, CFC-13, H-CFC-22, CFC-113, CFC-114, CFC-115, CH_3CCl_3, CFC-116, CCl_4, CH_3Cl, Halon-1211, Halon-1301, CH_3Br.	A	4	2
CRMH'88	NL	CH_4, N_2O, CFCs, O_3, halons	A		2
IPCC'90	I / NL	CH_4, N_2O, halocarbons, O_3 (both stratospheric and tropospheric) and the ozone precursors CO, non-Methane Hydrocarbons, Reactive Nitrogen Oxides.	A	4	4
DBEK'90	I / NL	CH_4, N_2O, O_3 (tropospheric), CFC-11, CFC-12, CO, stratospheric H_2O, aerosols, CFC-113, CFC-114, CFC-115, CCl_4, CFC-14, H-CFC-22, CH_3-CCl_3, CFC-116, CH_3Cl, Bromocarbons, Halon-1211, Halon-1301, CH_3Br.	A	4	4

Assessment	Arena	Non-CO_2 greenhouse gases mentioned	M=Mentioned qualitatively A=Assessed quantitatively	level of consensus (codes: see Table 3.2)	
				I	NL
IPCC'92	I / NL	CH_4, N_2O, halocarbons, O_3 (both stratospheric and tropospheric) and the ozone precursors CO, non-Methane Hydrocarbons, Reactive Nitrogen Oxides.	A	4	4
IPCC'94	I / NL	CH_4, N_2O, CFC-11, CFC-12, CFC-13, CFC-113, CFC-114, CFC-115, HCFC-22, HCFC-123, HCFC-124, HCFC-141b, HCFC142b, HCFC-225ca, HCFC-225cb, CCl_4, CH_3CCl_3, CF_3Br, HFC-23, HFC-32, HFC-43-10mee, HFC-125, HFC-134a, HFC-152a, HFC-143, HFC-143a, HFC-227ea, HFC-236fa, HFC-245ca, $CHCl_3$, CH_2Cl_2, SF_6, CF_4, C_2F_6, c-C_4F_8, C_6F_{14}, O_3 (both stratospheric and tropospheric) and the ozone precursors CO, non-Methane Hydrocarbons, Reactive Nitrogen Oxides.	A	4	4
IPCC'95	I / NL	CH_4, N_2O, CFC-11, CFC-12, CFC-13, CFC-113, CFC-114, CFC-115, HCFC-22, HCFC-123, HCFC-124, HCFC-141b, HCFC142b, HCFC-225ca, HCFC-225cb, CCl_4, CH_3CCl_3, CF_3Br, $CBrClF_2$, $CBrF_2CBrF_2$, HFC-23, HFC-32, HFC-41, HFC-43-10mee, HFC-125, HFC-134, HFC134a, HFC-152a, HFC-143, HFC-143a, HFC-227ea, HFC-236fa, HFC-245ca, HFOC-125e, HFOC-134e, CF_3I, $CHCl_3$, CH_2Cl_2, SF_6, CF_4, C_2F_6, C_3F_8, C_4F_{10}, C_5F_{12}, c-C_4F_8, C_6F_{14}, CH_3Cl, CH_3Br, O_3 (both stratospheric and tropospheric) and the ozone precursors CO, non-Methane Hydrocarbons, Reactive Nitrogen Oxides, industrial dust, soot, sulphate aerosols, nitrate aerosols.	A	4	4

1. The LASOM considered that if in the future H_2 were to be used on a large scale for electricity generation, emissions of water vapour might influence the climate.

2. They mention the possibility of climate change by stratospheric ozone depletion by CFCs, supersonic transport and NO_2.

Table 3.4 The inclusion of non-CO_2 GHGs in assessment reports and policy documents. In the second column, I stands for international, N for the Netherlands. The fourth column of the table shows whether the effects of non-CO_2 GHGs mentioned and their future concentrations were addressed quantitatively in the reports, or whether it was only mentioned that these gases also influence climate, without providing numbers. The last two columns reflects the level of consensus in each arena. Note that the indicated level of consensus is a meta-measure of the period in which the reports were drafted rather than a measure of the individual reports.

In the Netherlands arena, the major user of the assessments was the Netherlands Ministry of Housing Physical Planning and the Environment (VROM). In the period studied, VROM issued three policy documents that were entirely devoted to the climate problem. These are described briefly below.

In 1984 a working group of the ICMH/CIM (Interdepartmental Coordination Committee Environmental Hygiene/Coordination Committee Concerning International Environmental

Affairs) of VROM issued the policy document "Kooldioxide, Signalering van een Beleidsvraagstuk" ("Carbon Dioxide, Signalling a Policy Issue"). The scientific part of this document was based on the GR'83 and RMNO'84 assessments (Ministry of Housing, Physical Planning and the Environment 1984).

In 1987 the Minister of VROM presented a memorandum to parliament on "Climate Change by CO_2 and other trace Gases". This is the official government reaction to the second advice-report of the Gezondheidsraad (GR'86). As can be seen from Table 3.5, this reaction adopted the Villach'85 estimate rather than the GR'86 estimate of climate sensitivity, whereas it also deals with the non-CO_2 greenhouse gases, despite the focus of the Gezondheidsraad on CO_2. Chapter two of VROM'87 gives a scientific review of causes and consequences of the greenhouse effect being based mainly on the Villach'85 report rather than on the GR'83 and '86 reports. (Ministry of Housing, Physical Planning and the Environment, 1987).

In 1991 the Minister of VROM issued the Memorandum on Climate Change (Nota Klimaatverandering). This Government Paper explicitly adopts the IPCC'90 assessment as the scientific starting point for policy development, and opted explicitly for a (greenhouse) gas-by-gas approach (Ministry of Housing, Physical Planning and the Environment, 1991; 1992).

Table 3.5 presents the estimates of climate sensitivity and the non-CO_2 greenhouse gases given in these three policy documents. The table also shows what assessments they were based upon.

Policy document	Assessments used:	estimate of climate sensitivity (°C)	estimate adopted from	non-CO_2 greenhouse gases mentioned
VROM'84	GR'83 RMNO'84	not given		same as RMNO'84 and CO [1]
VROM'87	Villach'85 GR'86 GR'83	1.5-4.5	Villach'85	same as Villach'85
VROM'91	IPCC'90	1.5-4.5	IPCC'90	same as IPCC'90

1. The document refers to the RMNO'84 report for a list of relevant trace gases, and adds CO, making reference to a 'recent publication by Khalil and Rasmussen in Science'.

Table 3.5 Estimates of climate sensitivity and the mentioning of non-CO_2 greenhouse gases in Netherlands policy documents.

Table 3.5 shows that for the non-CO_2 greenhouse gases, VROM adopted the all-gases approach already in its first policy document on the climate issue. It also follows that the GR'83, RMNO'84 and GR'86 were the Netherlands assessments that were explicitly used in the policy documents. The Gezondheidsraad reports had the greatest impact in getting the climate problem on the Netherlands political agenda (Dinkelman, 1995). The more interesting it is that they constitute a line that significantly deviates from the internationally achieved closure.

3.3 The construction of the Gezondheidsraad assessments

In the following we analyse how the Gezondheidsraad constructed its estimate of climate sensitivity and how it dealt with the non-CO_2 greenhouse gases. In particular we consider the scientific and other underpinning of choices made in each case, against the background of the competing international assessments. We seek to identify factors that accounted for the difference in the adoption behaviour of the Gezondheidsraad committee and another important assessment group, the committee of the RMNO.

In 1980 the Gezondheidsraad established a CO_2 Committee. The initiative to set up a CO_2 Committee was born in another committee, the Philosophy Committee on Radiation Hygiene (filosofie commissie stralingshygiene). According to Mr. Swager[1], the reason was that the Gezondheidsraad had published a report on the health and safety aspects of the use of nuclear energy. In this context, a report on health aspects of CO_2 emissions from fossil fuel consumption was expected to make the debate on the pros and cons of nuclear and fossil fuel energy more even handed (E-mail message from Mr. J. Swager to Ms. G. Dinkelman, 17 May 1995, referred to in Dinkelman, 1995). This led to the establishment of a CO_2 working group of the Philosophy Committee on Radiation Hygiene in 1980, which later that year became the CO_2 Committee of the Gezondheidsraad.

At the beginning the CO_2 Committee consisted of nine experts (later eleven), including one representative from the Ministry of Health and Environmental Affairs (the precursor of VROM) and one secretary from the Gezondheidsraad. By means of lobbying, they managed to evoke an official request for advice on the adverse effects of CO_2 emissions from the Minister of VROM (Personal communication with Mr. Schuurmans, 19 December 1991). In response to this request, they issued in 1983 their first assessment: "Deeladvies inzake CO_2 problematiek" ("partial advice concerning the CO_2 problem", Gezondheidsraad, 1983). This report is focused on the scientific aspects of the CO_2-problem, and recognized the CO_2 problem as an important problem for the Netherlands. In 1986 de Gezondheidsraad issued its second assessment: "CO_2 problem, Scientific Opinions and Impacts on Society" This report focused on the social and economic impacts of climate change for the Netherlands.

[1] Swager was an official of the Ministry of Health and Environmental Affairs (the precursor of VROM) at that time. At present he works at the UNEP-FCCC, Climate Change Secretariat.

3.3.1 The construction of the Gezondheidsraad estimate of Climate Sensitivity

According to the GR'83 assessment a doubling of CO_2 would lead to a 2°C rise in temperature. This figure was based on the outcome of one single GCM (General Circulation Model) calculation by Manabe and Stouffer (1980). In GR'86 this estimate was widened to 2-4°C, although the committee suggests that its earlier estimate had not changed: "*The conclusions with respect to climate reactions to a CO_2 increase have not changed since the publication of the first advice in 1983: after a doubling of CO_2 the global mean surface temperature will rise by 2°C to 4°C, (. . .)*" (Gezondheidsraad, 1987, p.25). In the following we show that the Gezondheidsraad was familiar with the international assessments and hence with the 1.5°C to 4.5°C estimate.

For the members of the committee, the representative of the Ministry of VROM, Mr. Hekstra, produced summaries of the US-NAS'79 report and the US-NAS'83 report. It is striking that neither of the Gezondheidsraad reports makes an explicit reference to the US-NAS reports. However, the GR'83 report contains an appendix with a list of recommended literature. This list does include the US-NAS'79 and the US-NAS'82 report (of which the US-NAS'83 report is a revised version).

In the Gezondheidsraad-report references are made to the draft report of the Villach conference. From the minutes we also found that at least two members of the Gezondheidsraad committee (Mr. Goudriaan and Mr. Hekstra) attended the Villach'85 conference. Further, the minutes of the 27th meeting (26 March 1986) mention that Hekstra was of opinion that the draft advice makes too little use of the results of the Villach'85 conference.[1] As a response, the chairman (Mr. Schuurmans) asked all participants to closely review the text of the draft report in the light of the results of Villach. However after a comprehensive discussion the committee decided to maintain the figure of 2°C to 4°C for CO_2 doubling[2]. No details of the discussion are given in the minutes. Consequently, the reasons why the committee decided not to adopt the Villach estimate remain unclear.

In the minutes of the meetings of the CO_2 Committee one can find additional evidence to show that the committee was abreast of the 1.5°C to 4.5°C estimate: During the 19th meeting of the committee (10 December 1984) the EPA (US Environmental Protection Agency) high scenario for sea level rise was discussed. This high scenario is based on the US-NAS high estimate of a 4.5°C temperature increase for CO_2 doubling. The chairman of the committee, Mr. Schuurmans[3] was of the opinion that a temperature increase of 4.5°C cannot

---

1 Note that the 1.5°C to 4.5°C range is one of the conclusions from Villach.

2 Literally: "Na een uitgebreide diskussie over de in het advies aan te geven temperatuurtoename wordt besloten hiervoor 2° tot 4° aan te houden bij een verdubbeling van het CO_2 gehalte." (minutes 27th meeting, March 26, 1986, p.4)

3 C.J.E. Schuurmans is a climatologist and a prominent member of the Royal Dutch Meteorological Institute KNMI and of Utrecht University.

be ruled out since it falls within the existing uncertainty range. It is all the more surprising therefore that the Gezondheidsraad 1986 report presents a range of only 2-4°C.

According to Mr. Schuurmans the Gezondheidsraad committee made limited use of the US-NAS reports and the Villach conference because they had a preference for original journal articles (Personal communication C.J.E. Schuurmans, 23 January 1996).

In the same period as the Gezondheidsraad made its assessments, the 'Ad hoc Working Group CO_2' of the RMNO issued an inventory of climate research in the Netherlands (RMNO 1984). In this report a sketch of the state of the art of the climate problem is given, based almost completely on the US-NAS'83 report, including the 1.5 to 4.5°C estimate of climate sensitivity. The composition of the ad hoc working group of RMNO and the CO_2 committee of the Gezondheidsraad overlapped to a large extent: Table 3.6 shows that six of the ten experts in the Gezondheidsraad Committee were also members of the RMNO working group. It is remarkable that two groups with such a large overlap in composition produced different figures in the same period.

Expert:	Gezondheidsraad	RMNO
E.C. van Ballegooyen	X	
A.P.M. Baede		X
H. de Boois	**X**(up from 1984)	**X**
M. Booij		X
A. Dop		X
J. Goudriaan	**X**	**X**
G.P. Hekstra	**X**	**X**
P. Ketner	X	
J.J. Hofstra		X
W.G. Mook	**X**	**X**
R. Mureau	X	
H. Postma	**X**	**X**
P.G. Schipper	**X**	**X**
C.J.E. Schuurmans	X	
H.Weyma		X
F.C. Zuidema		X

Table 3.6 Overlap in membership in 1983 between the CO_2 Committee of the Gezondheidsraad and the Working Group CO_2 of the RMNO. Members sitting on both committees are printed in bold typeface.

Despite the closure on the level of the assessment reports, in the scientific community in the Netherlands the 1.5°C to 4.5°C is still a subject of debate. A workshop of Dutch scientists on the IPCC'90 report provided a deviating figure. In a lecture that summarized the results of the workshop, prof. Turkenburg said: "*The uncertainty in the projected temperature increase of 0.3°C/decade has a broader range than indicated by IPCC. According to IPCC, in the case of business as usual, the temperature increase will range from 0.2 to 0.5 °C/decade. There are good reasons for arguing that the temperature increase might be 0.1°C/decade, or 0.6-0.7°C/decade."* (Turkenburg, 1991). The figure of 0.2 to 0.5°C per decade is derived directly from the 1.5°C to 4.5°C values for climate sensitivity combined with the so-called *Business as Usual* emission scenario (Houghton *et al.*, 1990), so the workshop did in fact provide a wider range for climate sensitivity. Another example is a paper by Slanina (1994) in the Dutch journal *Energie en Milieuspectrum*. In this paper he suggests that a doubling of CO_2 would lead to a 2° to 7°C rise in temperature. This apparent 'dissensus' is not reflected at the level of assessment reports. This is however partly due to the fact that the intergovernmental IPCC assessments made the continuation of domestic assessment efforts superfluous.

3.3.2 The CO_2 focus of the Gezondheidsraad

The first advice-report of the Gezondheidsraad focused entirely on CO_2 (Gezondheidsraad, 1983). Other gases were not mentioned. In its second advice-report, issued in 1986, the Gezondheidsraad still focused mainly on CO_2. A very small section dealt with the effects of other trace gases. In this section the committee wrote: *"Although the committee will restrict itself in this advice to the CO_2 increase and its causes one must account for the amplification (possibly a doubling) of the climate effect by the increase of other trace gases. Even if the CO_2 increase would not occur then there may still occur climate effects as a result of the increase in other trace gases.*" Furthermore the 1986-conclusions state explicitly that in addition to the 1983-conclusions: *"The CO_2 problem is not an isolated problem. The concentration of other trace gases, that cause similar climate effects as CO_2, increases as well."*

The fact that the CO_2 Committee of the Gezondheidsraad in 1983 and in 1986 hardly paid any attention to non-CO_2 GHGs contrasts with scientific publications of individual members of the committee in the period 1980-1986. In these articles the other trace gases were mentioned from 1979, so right from the start, at least some experts in the committee were abreast of the significance of non-CO_2 greenhouse gases. We found a contribution made by Mr. Hekstra (1979) to a seminar in Ljubljana which presents a table compiled from three different sources with the estimated temperature change due to changes of the concentrations of N_2O, CH_4, $CFCl_3$, CF_2Cl_2, CFC-11, CFC-12, CCl_4, CH_3Cl, NH_3, C_2H_4, SO_2 and O_3. Another case of the early consideration of non-CO_2 greenhouse gases is the KNMI 1980 report "Antropogene Klimaatverandering, overzicht van de stand van zaken" (Anthropogenic climate change, state of the art review) (Reiff *et al.*, 1980). Mr Schuurmans, the chairman of the CO_2 Committee of the Gezondheidsraad, was one of the editors. This review integrally included a WMO statement on the greenhouse effect of CFCs (WMO, 1978). In this statement it was recognized that CFCs are strong greenhouse gases and that: *"It has been estimated that* *a continued release of chlorofluoromethanes at the 1977 rate, taken in isolation of other factors, could in this way produce an average temperature rise at the surface of 0.5°C. Such a change in the mean temperature may well be of significance."* In another publication (Schuurmans *et al.*, 1980) CH_4, NH_3, N_2O and Freon were mentioned as non-CO_2 anthropogenic greenhouse gases. It is also interesting to note that in its 1984 report the Working Group on CO_2 from the RMNO paid more attention to non-CO_2 GHGs than the Gezondheidsraad did in its 1986 report. They even presented a table, based on seven different sources, showing quantitative estimates of the temperature effect of a doubling of the concentrations of 16 different trace gases. There is no doubt that this was known to the Gezondheidsraad committee as well: as we found in the previous section, six members of the CO_2 Committee of the Gezondheidsraad were also members of the RMNO working group (see Table 3.6).

The reasons why the committee of the Gezondheidsraad paid relatively little attention to non-CO_2 GHGs become clear when we take a closer look at the minutes of their meetings. The first time the effects of non-CO_2 GHGs were mentioned in the minutes of the committee was before the first report was issued: at the 7th meeting (8 June 1982), Mr. Goudriaan gave an account of an AAAS (American Association for the Advancement of Science) conference, where Hoffman (United States Environmental Protection Agency, EPA) had stressed that other gases, such as N_2O and chloro-fluoro carbons can enhance the greenhouse effect of CO_2 by 20-40%. At the 11th meeting of the Committee (28 March 1983), while discussing the incoming correspondence, Mr. Hekstra stressed that from an article (no specification was given) it can be concluded that the influence of trace gases enhances the CO_2 temperature effect by a factor between 1.5 and 2. He also referred to a conference in Osnabrück where this subject was discussed. In Osnabrück it was stressed that the feedbacks in favour of other trace gases should get more attention.[1] Mr. Van Ballegooien reacted by stating that in quantifying these things, prudence is desirable since negative feedbacks occur as well.

During the 14th meeting (19 December 1983), the possible contents of the next advice were discussed. Mr. Hekstra suggested that the second advice should start with new insights, including positive feedbacks in favour of other trace gases. When at the 20th meeting (14 January 1985) the detection of the CO_2 effect was under discussion, the chairman stressed that for detecting the CO_2 effect the effect of other trace gases and aerosols should be taken into account. Mr. Van Ballegooien responded by considering that the committee could not pay much attention to these gases because the committee had a lack of expertise. Mr. Schuurmans suggested expressing the effect of other gases as an enhancement factor of the CO_2 effect. Mr. Hekstra said that this factor would probably amount to 2. Mr. Van Ballegooien responded that if the effect was indeed comparable in magnitude to the CO_2 effect, more attention should be paid to the trace gases. Mr. Schuurmans stressed that the consequences of a given enhancement factor should be considered. He suggested that a separate study into trace gases

[1] Although the minutes are not clear on this point, our impression is that this refers to feedbacks such as the release of methane from the thaw of permafrost.

might be advisable. Mr. Goudriaan stated that the relative importance of trace gases might change in the future, in the light of expected future emissions and residence time of the trace gases in the atmosphere. Mr. Schipper noticed that this underlines the necessity of further study on this point.

During the handling of the incoming correspondence at the 21th meeting (25 February 1985) an article "Doubling of atmospheric methane supported" was discussed. Mr. Schuurmans remarked that this article concludes that methane is responsible for 38% of the total effect and that the effect of all trace gases together is of the same order of magnitude as the CO_2 effect itself.

Mr Schuurmans stated that in the second recommendation attention will be paid to the effect of the other trace gases. Mr. Hekstra thought that the committee couldn’t go into details. He proposed using as a starting point a report of the Coordination Committee on the Ozone Layer (CCOL, an international committee).

At the 22th meeting it was considered to consult P. Crutzen to get more information about the other trace gases. This indicates the awareness of the committee that there was a lack of expertise within the committee regarding the non-CO_2 GHGs. The plan to consult Crutzen was not realized.

At the 27th meeting (26 March 1986) Mr. Hekstra made a case for including the non-CO_2 greenhouse gases in the Gezondheidsraad study. He announced that the government would issue an IMP-lucht (Indicative Multi-year Program on Air) around September 1986. He also referred to the work of the ICMH (Inter Departmental Committee Environmental Hygiene), and said that both the IMP and the ICMH would not be restricted to CO_2 but would pay attention to the other trace gases as well.

Later, during the same meeting, a discussion evolved about the moment at which CO_2 doubling would be reached. Mr. Hekstra stressed that the committee should consider the other trace gases. After a comprehensive discussion, the committee decided not to treat the trace gases in more detail than the draft report did then . The main argument was that the committee had no insights in the future concentrations of these gases. The only thing the committee could do was to refer explicitly to the report by the Coordination Committee on the Ozone Layer.

This reconstruction shows that the explicit considerations not to pay much attention to the non-CO_2 GHGs - despite the recognition of the significant contribution to the enhanced greenhouse effect of these gases - were lack of insight in future concentrations of these gases and lack of expertise on these gases within the committee and lack of initiative to fill up this gap.

3.4 Discussion and Conclusion

For the cases studied we identified closure of visible disputes in the assessments in the Netherlands arena after 1990, mainly because the Netherlands expert committees on climate were succeeded by the IPCC. In the pre-IPCC period we identified inter-assessment variability in the Netherlands arena for both the estimate of climate sensitivity and for the inclusion of non-CO_2 greenhouse gases in the assessments of climate change. In the Netherlands arena, the assessments in this period can be grouped as two lines: the line constituted by the Gezondheidsraad assessments and the assessments adopting the international closure.

In the policy-makers summaries of the successive international assessment reports the 1.5°C to 4.5°C estimate of climate sensitivity has not changed since 1979 (see chapter 2 of this thesis). The assessments by the Gezondheidsraad show different quantitative representations for climate sensitivity: 2°C in the 1983 report, and 2°C to 4°C in the 1986 report, although the Gezondheidsraad was familiar with the international quantitative representation. In the same period in which the Gezondheidsraad drafted its second assessment, another Dutch advisory body (the RMNO) did adopt the international quantitative representation for climate sensitivity, that is the temperature range 1.5°C to 4.5°C, rather than the Dutch 2°C (Gezondheidsraad-) representation. This difference is surprising, considering the large overlap in the membership of the two expert committees. Although we showed that in the period after the first IPCC report Netherlands scientists advocated broader ranges of climate sensitivity than the IPCC estimate, this did not become visible at the level of assessment reports, because since 1990 the IPCC assessments have been the only official (that is: drafted by a scientific body and endorsed by the government) ones in the Netherlands arena.

Our analysis regarding the inclusion of the non-CO_2 greenhouse gases in the assessments of climate change shows that it took about ten years for the scientific knowledge to reach the assessments. In the international line of assessment reports, the non-CO_2 GHGs were first mentioned in 1983, and acquired a substantial place in the assessments after the 1985 Villach conference. The main reports in the Netherlands line of assessments (namely the Gezondheidsraad reports of 1983 and 1986) were in essence focused on CO_2.

Our analysis of the minutes of the Gezondheidsraad reveals that it did know of the scientific evidence indicating effects of other gases. Lack of scientific expertise within the committee with respect to the future trends of these other gases explains the inertia that the Gezondheidsraad showed with respect to the inclusion of these gases in its reports. Again almost the same group of experts, but in another context, did include the non-CO_2 greenhouse gases in the assessment of the RMNO committee. In the RMNO context, the experts closely followed international developments.

We seek to identify what factors are decisive in the selection process: Why did the RMNO take the US-NAS’83 assessment as a starting point for its state of the art sketch rather than the comprehensive 1983 state of the art report by the Gezondheidsraad, whereas those persons sitting on both committees, when in the context of the Gezondheidsraad committee seemed to ignore the US-NAS’83 assessment and the Villach’85 assessment and instead built further on its own 1983 assessment. Why did the RMNO committee adopt the international innovation of including the non-CO_2 GHGs whereas those persons sitting on both committees, when in the context of the Gezondheidsraad committee remained focused on CO_2 almost exclusively. What can we learn from different modes of conduct of both committees?

In the classical view on controversies in science, actors are assumed to be paradigmatically predisposed to preserve their former interpretation as long as possible. According to that view experts represent a specific viewpoint which is left unaltered as long as the scientific evidence allows it (e.g. Kuhn, 1962, Collingridge and Reeve, 1986). This view contrasts with our finding that at the same time two expert committees with a large overlap in membership put forward a different interpretation and performed a significantly different pattern of adoption. Our analysis gives several indications that it was not the (dominant views of the) experts in the committee that were decisive in the adoption process, but it was the context in which the experts operated and the commitments they had made in each setting.

The phenomenon we observe here has much in common with the findings of Van Eijndhoven and Groenewegen (1991). They argue that in studies of the advisory practice, little attention is paid to the amount of flexibility an expert may introduce into the argumentative strategy when new scientific data or new practical situations arise. They show that despite the availability of scientific data that calls for a change in the assessment, the context can drive experts to stick to their former conclusions, whereas from the same data other conclusions can be constructed if the context changes. Regarding the assessments in the international arena, in chapter 2 we identified sources from which the assessors acquired flexibility in maintaining the 1.5°C to 4.5°C estimate of climate sensitivity without ignoring changing scientific ideas. There, we argued that expert interpretations are 'underdetermined' by any given scientific knowledge thanks to the repertoire of interpretive possibilities existing at each link in the argumentative chain. Often, new data introduce more flexibility, although negotiated interpretive links, once made, are consolidated as if naturally determined by the subtle redefinition of ancillary linkages and meanings. In chapter 2 we have introduced the concept '*anchoring devices*' for actively maintained expert interpretations that preserve consensus by an unstated social contract amongst the diverse scientists and policy specialists involved which allows the same portion of information to accommodate tacitly different local meanings.

The notion that the repertoire of interpretive possibilities of scientific evidence gives experts the flexibility to deconstruct and reconstruct argumentative chains that connect scientific data, expert interpretation and policy meaning, implies vice versa that, in a different setting with other commitments, the same experts can construct different conclusions from the same information. This is what we may observe when we compare the modes of conduct of the Gezondheidsraad committee and the RMNO committee.

We found a difference in barriers of adopting the international estimate of climate sensitivity within each committee. The difference was related to differences in setting, context, origin and orientation. In the following we will analyse these differences. The Gezondheidsraad committee had already completed half of its second impact assessment study, when the results from Villach came available. The first assessment report was a "state of the art" report on scientific insights regarding the CO_2 problem. The second report was primarily an *impact* assessment of climate change, explicitly addressing the impacts for the Netherlands. The climate sensitivity is an important indicator and is used as a key input parameter for calculating impacts of climate change such as sea level rise. The second assessment was drafted in the period 1983 - 1986. If the Gezondheidsraad had changed its estimate of climate sensitivity by adopting the 1985 Villach estimate, this could have changed its impact assessment. For instance, the figures for sea-level rise would have been somewhat different. This means that if - following the Villach results - they would have replaced its 1983 estimate (2°C) by the 1985 Villach estimate (1.5-4.5°C), they would have had to re-do part of the impact assessment calculations. In other words, if they had changed two major fundamentals of the impact study - namely the estimate of climate sensitivity and the restriction to one greenhouse gas, CO_2 - this could have caused a delay in the process. The RMNO committee was in that sense somewhat more free to adopt the international line.

According to Mr. Schuurmans, who was the chairman of the Gezondheidsraad committee, the barrier-argument did not play any significant role: the impact assessment by the Gezondheidsraad was not intended to be quantitative in such a way that the calculations would have to be redone if new climate data were to be used (personal communication with Schuurmans, 23 January 1996). However, the 1986 report did present scenario calculations and provided quantitative figures for sea level rise. The calculations were not complex. Even if we agree with Mr. Schuurmans that the barriers of adopting the 1.5°C to 4.5°C international estimate would have been low for the Gezondheidsraad, they would still have been higher than the barriers for the RMNO committee. Regarding the inclusion of non-CO_2 GHGs we identified another type of barrier: expertise was lacking in the Gezondheidsraad committee, so the committee would have had to recruit a non-CO_2 GHGs expert, or the expertise had to be acquired otherwise, if the committee were to adopt the innovation.

Another difference is in the terms of reference, the assignment and the genesis of both committees. The CO_2 committee of the Gezondheidsraad proceeded from the "Philosophy Committee on Radiative Protection". The motive for setting up start the CO_2 committee was closely related to the nuclear energy discussion. This origin of the committee and its assignment to assess the 'CO_2 problem' led automatically to a focus on CO_2. Mr. Schuurmans confirmed this bias-by-appointment, stating that *"If the GR-report from 1986 had the greenhouse problem as such as the topic, the non-CO_2 greenhouse gases would presumably have got more attention."*[1] Originally, the assignment of the committee even focused on the health-effects of CO_2. According to Mr. Goudriaan "*This question* [the health effects of CO_2, JvdS] *was answered within one hour. Then the committee continued with an inventory of knowledge of and questions about an increased CO_2 concentration and the greenhouse effect*"[2]

---

[1] Personal communication with Mr. Schuurmans, 21 March 1996. Mr. Schuurmans further explains the focus on CO_2 from the notion that for the impacts it is not important whether the climate change is caused by CO_2 or by other greenhouse gases. There is however no scientific consensus regarding the latter claim, as has been discussed in chapter 2.

[2] Jan Goudriaan in an interview with Ton van Burgsteden. It should be noted that nowadays there is an increasing interest in the health effects of global warming, especially regarding the spread of tropical diseases.

In the context of the Gezondheidsraad Committee, the rationale for starting the assessment was primarily connected with concerns about adverse effects of large-scale fossil fuel use rather than concerns about the risks of climatic change. The RMNO was interested in the climate problem from a research perspective and hence was biased towards existing climate research in the Netherlands, rather than to the risks of fossil fuel use focused on CO_2. This difference in bias made it somewhat easier for the RMNO to investigate the non-CO_2 greenhouse gases.

The RMNO-committee had the task of designing a research agenda for Dutch climate change research. Its advice was built partly on the Gezondheidsraad 1983 report, but in total much more use was made of the assessments by the US-NAS. Further, in their multi-year plan from 1983 the RMNO explicitly stated that one of their objectives was to seek alignment with the international research community (RMNO, 1983).

Also we have seen that in the diffusion of the innovations from the international arena to the Netherlands, the representative of the Ministry of VROM on the two committees, Gerrit Hekstra, played a special role. Our analysis shows that it was Mr. Hekstra who time and time again made a case for the use of international assessments: He attended the first World Climate Conference in 1979. He distributed summaries of both US-NAS reports to all members of the Gezondheidsraad committee. He attended the Villach’85 conference and pleaded for the use of the Villach results in the second assessment report of the Gezondheidsraad. He was involved in the influential assessments by the German Enquete Commission on the climate issue. He undertook several unsuccessful attempts to convince the Gezondheidsraad committee to include the non-CO_2 greenhouse gases in the assessment study. But he did succeed in adopting the international estimate of climate sensitivity and the all gases approach in the government reaction to the Gezondheidsraad reports.

The Gezondheidsraad line of assessments and the international line of assessments can to some extent be viewed as competing schools in climate risk assessment in the Netherlands arena. Beauchamp (1987) distinguished five ways in which a situation of competing schools comes to an end. These are: *sound argument closure*, *consensus closure*, *procedural closure*, *natural death closure*, and *negotiation closure*. The estimate of climate sensitivity established in the international assessment community of the climate problem (1.5-4.5°C) can be viewed as a mixture of *sound argument closure* and *consensus closure* (see chapter 2). The diffusion of this estimate to the Netherlands, where it replaced the quantitative representations presented in the Gezondheidsraad assessments, is an example of *natural death closure*. Natural death closure occurs if some of the main protagonists die or grow old. Our analysis shows another scenario: it is not the protagonists who die, but it is the context that ’dies’ and gets replaced by another context. We even saw that the same scientists operating in another context (the RMNO instead of the Gezondheidsraad) start acting as protagonists of the other line. The Gezondheidsraad line ended when its assessments were succeeded by the assessments of the IPCC.

Rip (1992) argued that the increase in robustness is a driving force in science for policy. The anchoring function of maintained consensus brings about robustness (see chapter 2). In the analysed period, a situation arose where anchoring of the Gezondheidsraad interpretations became dysfunctional because the intergovernmental assessment community (IPCC and its precursors) was taking over. In terms of robustness increase it became more opportune to join the international club. A natural death of the Gezondheidsraad committee and its line of assessments was the inevitable consequence, which made closure occur in the official assessments (which we defined here as those assessments that are drafted by a scientific body and endorsed by the government).

We have shown that in the Netherlands arena in the pre-closure period, the context in which the experts operated and the commitments they had made in each setting were more decisive for the selection of one of the two interpretations that co-existed than the paradigmatic predispositions of the experts involved.

Acknowledgements

We would like to thank Wim Turkenburg, Jill Jäger and Cor Schuurmans for useful comments on earlier versions of this chapter. We are grateful to Sheila McNab for linguistic and stylistic improvements.

3.5 References

Beauchamp, T.L. 1987. Ethical Theory and the Problem of Closure. Pages 27-48 *in* Engelhardt, H.T., Jr. and A.L. Caplan (eds.), Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology. Cambridge University Press, Cambridge.

Bolin, B. Döös, B.R., Jäger, J and Warrick, R.A. 1986 The greenhouse effect, climatic change and ecosystems. SCOPE 29, John Wiley & Sons, Chichester, 541 pp.

Collingridge, D. and C. Reeve 1986. Science Speaks to Power: The Role of Experts in Policy Making. London, Frances Printer, 175 pp.

CRMH 1988. Grootschalige Atmospherische Veranderingen als Gevolg van Antropogene Emissies. Report 88/8, Centrale Raad voor de Milieuhygiene, The Hague, 50 pp.

De Boois, H., J. Rotmans, and R.J. Swart 1988. Stand van Zaken en Perspectieven van de Broeikasproblematiek, Report 758471001, RIVM, Bilthoven, 12 pp.

Deutscher Bundestag Enquete Kommission, 1988. Schutz der Erdatmosphaere: eine Internationale Herausforderung. Zwischenbericht der Enquete Kommission des 11 Deutschen Bundestages. Deutscher Bundestag, Bonn, 583 pp.

Deutscher Bundestag Enquete Kommission, 1990. Schutz der Erdatmosphaere: eine Internationale Herausforderung. Zwischenbericht der Enquete Kommission des 11 Deutschen Bundestages. Economica, Bonn, 633 pp.

Dinkelman, G. 1995. Verzuring en Broeikaseffect, de Wisselwerking tussen Problemen en

Oplossingen in het Nederlandse Luchtverontreinigingsbeleid (1970-1994). (Ph.D. thesis), Jan van Arkel, Utrecht 319 pp.

Everdingen, J.J.E. 1988. Consensusontwikkeling in de geneeskunde. Scheltema Holkema, Utrecht, 260 pp.

Funtowicz, S.O., and J.R. Ravetz 1990. Uncertainty and Quality in Science for Policy. Kluwer, Dordrecht, 229 pp.

Gezondheidsraad 1983. Deeladvies inzake CO_2 problematiek. Gezondheidsraad ’s-Gravenhage, 199 pp.

Gezondheidsraad 1986. Advies inzake CO_2 problematiek, Gezondheidsraad, ’s-Gravenhage, 150 pp.

Gezondheidsraad 1987. CO_2 Problem, Scientific Opinions and Impacts on Society, second advice. Gezondheidsraad, ’s Gravenhage, 140 pp.

Handel, M.D. and J. Risbey (eds.) 1992. An Annotated Bibliography on the Greenhouse Effect and Climate Change. *Climatic Change*, 21:97-255.

Hecht, A.D. and D. Tirpak 1995. Framework Agreement on Climate Change: A Scientific and Policy History. *Climatic Change*, 29:371-402.

Hekstra, G.P. 1979. The Case for a Global CO2-Emission Ceiling. Paper presented at *UNEP/ECE Regional Seminar on Alternative Patterns of Development and Lifestyles*, Ljubljana, 3-8 December 1979, 10 pp.

Hekstra, G.P. 1986. Will Climatic Changes Flood the Netherlands? Effects on Agriculture, Land-Use and Well-Being. *Ambio*, 15:316-326.

Houghton, J.T., G.J. Jenkins and J.J. Ephraums (eds.) 1990. Climate Change, The IPCC Scientific Assessment. Cambridge University Press, 365 pp.

Houghton, J.T., B.A. Callander and S.K. Varney (eds.) 1992. Climate Change 1992 The Supplementary Report to the IPCC Scientific Assessment. Cambridge University Press, 200 pp.

Houghton, J.T., L.G. Meira Filho, J. Bruce, Hoesung Lee, B.A. Callander, E. Haites, N. Harris and K. Maskell (eds.) 1995. Climate Change 1994 Radiative Forcing of Climate Change and An Evaluation of the IPCC IS92 Emission Scenarios. Cambridge University Press, 339 pp.

Houghton, J.T., L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg and K. Maskell (eds.) 1996. Climate Change 1995, The Science of Climate Change, Cambridge University Press, 572 pp.

ICMH/CIM 1984. Kooldioxide, Signalering van een Beleidsvraagstuk. Report Publicatiereeks Milieubeheer nr. 19, Ministerie van Volkshuisvesting Ruimtelijke Ordening en Milieubeheer, Directoraat-Generaal voor de Milieuhygiëne, The Hague, 38 pp.

Jäger, J., J. Cavender Bares, N. Dickson, A. Fenech, P. Haas, F. Hampson, P. Hughes, A. Liberatore, E.A. Parson, V. Sokolov, F. Toth, C. Waterton, J.P. van der Sluijs, J.C.M. van Eijndhoven, B. Wynne (forthcoming). Risk Assessment. *In* Clark, W.C., J.C.M. van Eijndhoven and J. Jäger (eds.) Social Learning in the Management of Global Environmental Risks. MIT Press.

Jones, M.D.H. and A. Henderson-Sellers 1990. History of the greenhouse effect. *Progress in Physical Geography*, 14:1-18

Kuhn, T. 1962. The Structure of Scientific Revolutions. Chicago, 172 pp.

LASOM, Landelijke Stuurgroep Onderzoek Milieuhygiene 1979. Signalering en Evaluatie van oorzaken en effecten van milieuveranderingen. 3e interim rapport, bijlage IV nr. 8, 23 pp.

Manabe, S. and R.J. Stouffer 1980. Sensitivity of a Global Climate Model to an Increase of CO_2 Concentration in the Atmosphere. *Journal of Geophysical Research*, 85(C10):5529-5554.

Ministry of Housing, Physical Planning and the Environment (VROM) 1987. Notitie Klimaatverandering door CO_2 en andere sporegassen. Leidschendam, 27 pp.

Ministry of Housing, Physical Planning and the Environment (VROM) 1990. Nota Klimaatverandering. Staatsuitgeverij, The Hague, 143 pp.

Ministry of Housing, Physical Planning and the Environment (VROM) 1992. Measures taken within the Netherlands National Programme on Climate Change. CCD/paper 3, Leidschendam, 32 pp.

Parson, E.A. 1995. Integrated Assessment and Environmental Policy Making, in Pursuit of usefulness. *Energy Policy* 23(4-5):463-475.

Reiff, J. and C.J.E. Schuurmans (eds.) 1980. Antropogene Klimaatsverandering, Overzicht van de Stand van Zaken. Report V-347, KNMI, De Bilt 22 pp.

Rip, A. 1991. The Danger Culture of Industrial Society. Pages 345-365 *in* R.E. Kasperson and P.J.M. Stallen (eds.) Communicating Risks to the Public. International Perspectives, Kluwer Academic Publishers, Dordrecht/Boston/London.

Rip, A., Risicocontroverses en de Verwevenheid van Wetenschap en Politiek. *Kennis en Methode* 1992(1):63-80.

Rip, A. 1992. Expert Advice and Pragmatic Rationality. Pages 363-379 *in* The Culture and Power of Knowledge, Inquiries into Contemporary Societies, Nico Stehr and Richard V. Ericson (Eds.); Walter de Gruyter: Berlin.

RMNO 1983. RMNO Meerjarenprogramma. RMNO publcatie no. 2, Rijswijk.

RMNO 1984. Onderzoek in Nederland naar de gevolgen van toenemende CO_2 en andere sporegassen in de atmosfeer door menselijke activiteiten. RMNO, Rijswijk, 48 pp.

Rotmans, J. 1990. IMAGE An Integrated Model to Assess the Greenhouse Effect. (Ph.D. Thesis), Rijksuniversiteit Limburg, 295 pp.

Schuurmans, C.J.E. 1978. Klimaatschommelingen, oorzaken en mogelijke consequenties, Nota ten behoeve van de commissie Algemene Toekomst Verkenningen van de Wetenschappelijke Raad voor het Regeringsbeleid. De Bilt, 12 pp.

Schuurmans, C.J.E., H.M. Van den Dool, and J.A. Wisse 1980. Luchtverontreiniging en Wereldklimaat (1). Zenit 7(1):8-13.

Star, S.L. 1988. The Structure of ill-structured solutions: Boundary Objects and heterogenious distributed problem solving. Pages 37-54 *in* M. Huhns and L. Gasser (Eds.), Distributed Artificial Intelligence, Morgan Kauffman, Menlo Park, CA. (Cited in: H. Watson-Verran and D. Turnbull 1995. Science and other Indigenous Knowledge Systems. Pages 115-139 *in* S. Jasanoff, G.E. Markle, J.C. Pietersen and T. Pinch (eds.), *Handbook of Science and Technology Studies,* Sage Publications, Thousand Oaks/London/New Delhi.)

Turkenburg, W.C., Broeikaseffect en Wetenschap: een evaluatie van het IPCC-rapport 'Climate Change', in: Bakker, H. J., Dronkers, J. and Vellinga, P. (eds.) 1991. *Veranderend wereldklimaat : Nederlandse visies, reacties en commentaren*. Delwel, The Hague, p. 21-26 and p. 105-111.

U.S. National Academy of Sciences 1979, Carbon Dioxide and Climate: A Scientific Assessment, Climate Research Board, U.S. National Academy of Sciences, Washington D.C., 22 pp.

U.S. National Academy of Sciences 1983. Changing Climate, Report of the Carbon Dioxide Assessment Committee. Board on Atmospheric Sciences and Climate, Commission on Physical Sciences, Mathematics and Resources, National Research Council, Washington D.C., 495 pp.

Van Eijndhoven, J.C.M. en P. Groenewegen 1991. The construction of Expert Advice on Health Risks. *Social Studies of Science* 21:257-278.

Van Eijndhoven, J.C.M., G. Dinkelman, J.P. van der Sluijs and R. Pleune (forthcoming). Finding your Place, A History of the management of Global Environmental Risks in the Netherlands. *In* W.C. Clark, J.C.M. van Eijndhoven and J. Jäger (eds.) "Social Learning in the Management of Global Environmental Risks", MIT press.

Victor, D.G. 1995. On Writing Good Histories of Climate Change and Testing Social Science Theories. *Climatic Change* 29:363-369.

Victor, D.G. and W.C. Clark 1991. The Greenhouse Effect in the USA: A History of Science up to 1985. Contribution Number 2 to the Project Social Learning in the Management of Global Environmental Risks, 81 pp.

World Meteorological Organization 1975. WMO statement on Modification of the Ozone Layer Due to Human Activities and Some Possible Geophysical Consequences, 26 November 1975, WMO, ## pp.

World Meteorological Organization 1978. Second WMO Statement on Modification of the ozone layer due to human activities and some possible geophysical consequences, WMO, 6 pp.

4. Biogenic Feedbacks in the Carbonate-Silicate Geochemical Cycle and the Global Climate*

4.1 Introduction
4.2 The carbonate-silicate geochemical cycle
4.3 The model
4.4 Results
4.5 Conclusions and discussion
4.6 Acknowledgements
4.7 References
appendix A The 0-dimensional temperature model

Abstract

The carbonate-silicate geochemical cycle is believed to act as a long-term global thermostat by controlling the atmospheric CO_2 concentration. We investigated the role of the biota in this system using a dynamic simulation model. The model (a modification of the BLAG'83 model) describes five geochemical reservoirs: atmospheric CO_2, lithospheric $CaSiO_3$ (silicate) and $CaCO_3$ (carbonate), oceanic Ca^{2+}, and HCO_3^- (whereby Mg^{2+} was treated as if it were Ca^{2+}). The fluxes between the reservoirs are due to $CaSiO_3$- and $CaCO_3$-weathering, $CaCO_3$-precipitation, and metamorphic magmatic decarbonation of $CaCO_3$.

We modelled the role of the biota as ideal optimum responses of the rates of weathering and $CaCO_3$-precipitation to temperature change. These responses are superimposed on a physico-chemical temperature response. The temperature was calculated from a 0-dimensional radiation balance climate model, which takes the greenhouse gases CO_2 and H_2O into account.

It is shown that, depending on the parameter values, the introduction of optimum responses can either increase or decrease the stability of the simulated global climate with respect to changes in solar luminosity. We achieved enhanced stability by introducing a biological response of weathering that had an optimum temperature higher than the steady state temperature at the start of a simulation, whereas biological $CaCO_3$-precipitation led to reduced stability.

An increase in solar luminosity was offset by a decrease in CO_2 concentration. However, above a critical level of solar luminosity, certain values for the optimum curve

* This chapter was written as a co-authored paper and was published as J.P. van der Sluijs, G.J. de Bruyn, and P. Westbroek, Biogenic Feedbacks in the Carbonate-Silicate Geochemical Cycle and the Global Climate, *American Journal of Science*, **296** (8), 1996, p.932-953.

parameters led to a stable pattern of oscillations. Under near-critical conditions, the model showed frequency-dependent amplification of periodic perturbations in solar luminosity (Milankovitch-type forcing). We speculate that biogenic feedbacks in the carbonate-silicate geochemical cycle played a key role in the major glacial-interglacial cycles of the Pleistocene.

4.1 Introduction

It is generally agreed that when the solar system was formed some 4.6 by ago the sun was between 25 and 30 per cent dimmer than it is today. Since then solar luminosity appears to have increased linearly with time (Newman and Rood, 1977; Gough, 1981). Calculations suggest that, given the current atmospheric composition, a 25 per cent dimmer sun would lead to frozen oceans (Sagan and Mullen, 1972). Nevertheless, the geological record shows that liquid oceans and life have both existed for more than 3 by. This "faint young sun paradox" was first pointed out by Sagan and Mullen (1972) and later reviewed by Kasting (1989). It has been suggested that the biota together with its immediate environment automatically provides a stable environment on a global scale (Lovelock and Margulis, 1974a, 1974b; Watson and Lovelock, 1983; Lovelock, 1988).

On a geological time-scale (> 10,000 years) the carbonate-silicate geochemical cycle (hereafter referred to as CSGC) is believed to exercise a major control on atmospheric CO_2 (Berner, Lasaga, and Garrels, 1983) and hence on the radiation balance of the Earth. The idea that the CSGC acts as a global thermostat was first put forward by Walker, Hays, and Kasting (1981), but they did not include the biota in their model. To explore the effects of the biota on the operation of this thermostat we constructed a CSGC model containing temperature optimum response functions for major fluxes that are known to be substantially influenced by living systems. We emphasize that we used theoretical rather than empirical functions to describe the biological activity, although we tried to keep the model reasonably realistic.

4.2 The carbonate-silicate geochemical cycle

The chemical basis of the carbonate-silicate geochemical cycle is expressed by the reactions formulated by Urey (reaction 1) and Chamberlin (reaction 2) (Berner, Lasaga, and Garrels, 1983). In (1) and (2) M represents the sum of Mg and Ca.

$$CO_2 + MSiO_3 \underset{metamorphism}{\overset{weathering}{\rightleftarrows}} MCO_3 + SiO_2 \qquad \textbf{(1)}$$

In the Urey reactions, the atmospheric CO_2 concentration results from the balance between uptake of CO_2 by weathering of silicate rock to ultimately form carbonate, and

$$CO_2 + MCO_3 + H_2O \underset{precipitation}{\overset{weathering}{\rightleftarrows}} M^{++} + 2HCO_3^- \quad \textbf{(2)}$$

its release by metamorphism-magmatism. The Chamberlin reactions in combination with erosion and run-off are responsible for the transport of carbonate from the continents towards the ocean floor: carbonate on the continents dissolves by reacting with CO_2 and H_2O. The reaction products, Ca^{2+} and HCO_3^-, are washed out and transported via river run-off toward the oceans. In the oceans biomineralization and abiotic precipitation form the carbonate sink which accumulates on the ocean floor. Magmatic and metamorphic decarbonation (1), completes the cycle. Perturbations in (1) or (2) may result in changes in the atmospheric CO_2 concentration.

Living systems play a key role in weathering and in $CaCO_3$-precipitation. It is widely held that plants accelerate rock weathering by secreting organic acids, by extracting ions from soil solutions, by retaining and recycling water, and by acting as a counter-diffusive pump for transferring CO_2 from low concentrations in the atmosphere to high concentrations in soils (Cochran and Berner, 1992). Cochran and Berner (1992) claim that higher (vascular) plants are the most effective species for accelerating silicate weathering rates. Weathering results from the attack by organic and carbonic acid (H_2CO_3) on carbonate and silicate minerals. Carbonic acid is generated in soils by the photosynthetic fixation of CO_2 and the subsequent oxidation of organic matter by microbial and root respiration (Berner, Lasaga, and Garrels, 1983, Kump and Volk, 1991).

The major oceanic sinks of $CaCO_3$ consist of carbonate platforms and calcifying pelagic organisms (Westbroek, 1991). Inorganic precipitation does not play an important role in the oceans today.

4.3 The model

The model we used is a simplified, slightly modified version of a geochemical computer model of the CSGC constructed by Berner, Lasaga, and Garrels (the BLAG'83 model, Berner, Lasaga, and Garrels, 1983). The main simplification is that we combined the calcite and dolomite reservoirs and treated them as if they were $CaCO_3$. Since we do not consider Mg separately, the volcanic seawater reaction in the BLAG'83 model, in which Mg^{2+} and Ca^{2+} are interchanged, is omitted. Berner, Lasaga, and Garrels (1983) concluded that changes in the relative masses of the calcite and dolomite reservoirs have no decisive influence on the resulting atmospheric CO_2 concentrations. In his GEOCARB I (Berner 1991) and GEOCARB II (Berner, 1994) models, Berner made the same simplification by lumping all carbonate minerals together. Other simplifications are that seafloor spreading and land surface are kept constant over time (in terms of BLAG'83: $f_A(t)=1$ and $f_{SR}(t)=1$). One of our modifications is that we use a different greenhouse function to calculate the temperature from the atmospheric CO_2-content (see app.).

The model describes five geochemical reservoirs interconnected by an appropriate suite of fluxes. The reservoirs are *AC* (Atmospheric Carbon dioxide: CO_2), *LS* (Lithospheric Silicate: $CaSiO_3$ and $MgSiO_3$), *LC* (Lithospheric Carbonate: $CaCO_3$ and $CaMg(CO_3)_2$), *OC* (Oceanic dissolved Calcium: Ca^{2+}), and *OB* (Oceanic Bicarbonate: HCO_3^-). The fluxes between the reservoirs are governed by three key processes: weathering, $CaCO_3$-precipitation, and metamorphic magmatic decarbonation. We assumed that at time t=0 the model was in steady state. The steady state values of the model variables are adopted from the original BLAG'83 model and reflect the present-day values. The consequences of perturbing the steady state are modelled using rate-law expressions for all fluxes between the reservoirs, on the assumption that the reaction rates were first order linear dependent on the sizes of the reservoirs (for further details we refer to Berner, Lasaga, and Garrels, 1983). We defined two different types of assumptions concerning the temperature dependence of the rate-law expressions: (A) the "BLAG'83 assumptions", which have a physico-chemical character, and (B) the "bio-assumptions", which take into account an ideal optimum response to the temperature. First we will discuss the expressions based on the BLAG'83 assumptions.

Weathering Fluxes (BLAG'83 Assumptions).——The rate-laws for the fluxes due to CO_2 weathering of silicates and carbonates are given by:

$$F_{W_{ij}} = k_{W_{ij}} \cdot M_i \qquad \textbf{(3)}$$

where F_{Wij} stands for the flux of compound j caused by weathering of reservoir i (in 10^{18} moles my^{-1}), and k_{Wij} stands for a first order weathering rate constant with respect to reservoir i (in my^{-1}) and component j. M_i stands for the mass of reservoir i (in 10^{18} moles) and $i \in \{LS, LC\}$; $j \in \{AC, LC, OB, OC\}$. The temperature dependence of k_W is modelled in the BLAG'83 model as:

$$k_{W_{ij}} = k_{W_{ij}}(0) \cdot f(T) \qquad \textbf{(4)}$$

where $f(T)$ is the relative weathering rate (relative to the steady state value at t=0), and $k_{Wij}(0)$ is the t=0 value of k_{Wij}. Berner, Lasaga and Garrels (1983) presented an empirically derived expression for the relative weathering rate $f(T)$:

$$f(T) = (1 + 0.038 \cdot (T - T_0)) \cdot (1 + 0.049 \cdot (T - T_0)) \qquad \textbf{(5)}$$

where T_0 represents the steady state temperature (t=0). The weathering flux of component j caused by the weathering of reservoir i can now be written as:

$$F_{W_{ij}} = k_{W_{ij}}(0) \cdot f(T) \cdot M_i. \qquad \textbf{(6)}$$

If the values of the fluxes (F) on t=0 are given, we can rewrite (6) as:

$$F_{W_{ij}}=F_{W_{ij}}(0)\cdot f(T)\cdot\frac{M_i}{M_{i0}}. \quad \textbf{(7)}$$

Fluxes Due to Magmatism-Metamorphism (BLAG'83 Assumptions).——The rate-laws for the fluxes due to magmatic-metamorphism are of the same format as (3) but the subscript is *M* instead of *W*. In our model k_M is assumed to be constant over time. If the flux at time *t*=0 is given, we can write:

$$F_{M_{ij}}=F_{M_{ij}}(0)\cdot\frac{M_i}{M_{i0}} \quad \textbf{(8)}$$

with $i=LC$; $j \in \{AC,LC,LS\}$.

Fluxes Due to $CaCO_3$-Precipitation (BLAG'83 Assumptions).——In the ocean, carbonate is precipitated according to the following chemical equilibrium reaction:

$$Ca^{2+}+2HCO_3^- \rightleftharpoons CaCO_3+CO_2+2H_2O. \quad \textbf{(9)}$$

This equilibrium is governed by the kinetic equilibrium equation (Berner, Lasaga, and Garrels, 1983):

$$K_{eq}=\frac{M_{OC}\cdot M_{OB}^2}{M_{AC}} \quad \textbf{(10)}$$

in which K_{eq} is the equilibrium constant, M_{OC}, M_{OB} and M_{AC} are the masses of the Ca^{++}, HCO_3^-, and CO_2 reservoirs (10^{18} moles) respectively. Precipitation can be interpreted as a deviation from the equilibrium governed by the equilibrium equation (10). Consequently, we can calculate the flux of component *j* due to the precipitation of carbonate from:

$$F_{P_{ij}}=k_{prep\,j}\cdot(M_{OC}\cdot M_{OB}^2-K_{eq}\cdot M_{AC}) \quad \textbf{(11)}$$

where k_{prep} is the precipitation rate constant, $i=LC$; $j \in \{AC,LC,OB, OC\}$.
Notice that the part between brackets in (11) is:

- zero in the case of thermodynamic equilibrium;
- positive if the ocean is supersaturated, which leads to net precipitation of carbonate;
- negative if the ocean is undersaturated, which leads to dissolution of carbonate.

The precipitation rate constant (k_{prep}) determines the speed at with the system returns to equilibrium. By solving (11) with $j=AC$, Berner, Lasaga, and Garrels (1983) found a response time τ given by:

$$\tau = 1/(k_{prep\,AC}\cdot K_{eq}). \quad \textbf{(12)}$$

In our model we assumed a response time τ of 0.001 my. The original BLAG'83 model used a minimum response time of 0.0005 my and a maximum of 0.01 my. The constants k_{prep} and K_{eq} can be calculated from τ and F_{Pij0}, using (11) and (12).

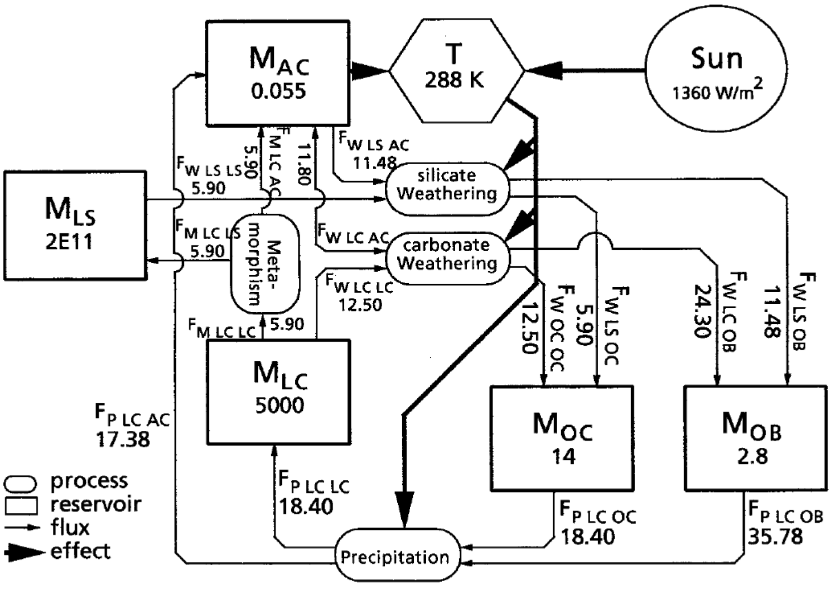

Fig. 1. The model with the t=0 steady state values: fluxes in 10^{18} moles/my, reservoirs in 10^{18} moles. For the symbols we refer to the symbol list.

The fluxes between the reservoirs and their steady state values at time t=0 (F_{kij0}) are given in figure 1 (values adopted from BLAG'83). The fluxes of CO_2 (AC) and HCO_3^- (OB) are corrected for weathering by H_2SO_4 (for details see Berner, Lasaga, and Garrels, 1983).

From figure 1 and the derived equations we can write down the five first-order differential equations which account for the changes in each reservoir M_i in an infinitesimal time step dt:

$$dM_{AC}/dt = -F_{W\,LS\,AC\,0}\cdot f(T)\cdot R_{LS} \quad -F_{W\,LC\,AC\,0}\cdot f(T)\cdot R_{LC} \quad +F_{P\,LC\,AC\,0}\cdot R_P \quad +F_{M\,LC\,AC\,0}\cdot R_{LC} \qquad (13)$$

$$dM_{LS}/dt = -F_{W\,LS\,LS\,0}\cdot f(T)\cdot R_{LS} \quad +F_{M\,LC\,LS\,0}\cdot R_{LC} \qquad (14)$$

$$dM_{LC}/dt = -F_{W\,LC\,LC\,0}\cdot f(T)\cdot R_{LC} \quad +F_{P\,LC\,LC\,0}\cdot R_P \quad -F_{M\,LC\,LC\,0}\cdot R_{LC} \qquad (15)$$

$$dM_{OC}/dt = F_{W\,LS\,OC\,0}\cdot f(T)\cdot R_{LS} \quad +F_{W\,LC\,OC\,0}\cdot f(T)\cdot R_{LC} \quad -F_{P\,LC\,OC\,0}\cdot R_P \qquad (16)$$

$$dM_{OB}/dt = F_{W\,LS\,OB\,0}\cdot f(T)\cdot R_{LS} \quad +F_{W\,LC\,OB\,0}\cdot f(T)\cdot R_{LC} \quad -F_{P\,LC\,OB\,0}\cdot R_P \qquad (17)$$

where: $R_{LS}=M_{LS}/M_{LS\,0}$; $R_{LC}=M_{LC}/M_{LC\,0}$; $R_P=F_{P\,LC\,AC}/F_{P\,LC\,AC\,0}$ (with (11)) and $f(T)$ given by eq (5). The subscript zero refers to the steady state or t=0 values. For the other symbols and subscripts we refer to the symbol list. As argued by Berner (1991)

LIST OF SYMBOLS

F_{kij}	Flux of component *j* due to process *k* affecting reservoir *i*.
I	Instability quotient (statistical function)
K_{eq}	Equilibrium constant for calcium carbonate precipitation kinetics
K_{prep}	Precipitation constant for calcium carbonate precipitation
k_{kij}	First-order rate constant for the formation of component *j* by process *k* of reservoir *i*.
M_i	Mass of reservoir *i* (10^{18} mole).
R_i	Mass ratio of reservoir *i* relative to its steady state or *t*=0 value.
r_k	Intrinsic rate parameter of the optimum curve for process *k*.
SF	Stability factor relative to a no-feedback reference run (statistical function).
$T_{k\,opt}$	Optimum temperature of the optimum curve for process *k*.
σ	Dispersion (parameter of an optimum curve)
τ	Response time (my)

Subscripts:

0	Steady state or *t*=0 value
i	Index referring to reservoir: $i \in \{AC, LC, LS, OB, OC\}$ *AC* Atm. Carbon dioxide; *LC* Lith. Carbonate; *LS* Lith. Silicate; *OB* Oceanic Bicarbonate; *OC* Oceanic dissolved Calcium.
j	index referring to component: $j \in \{AC, LC, LS, OB, OC\}$, see *i*.
k	index referring to process: $k \in \{M, P, W\}$ *M* Magmatic metamorphism; *P* Precipitation; *W* Weathering
opt	Optimum

eq (14) can be simplified to:

$$dM_{LS}/dt = 0 \qquad \textbf{(14b)}$$

because the silicate reservoir (*LS*) can be considered an infinite source, given that its reservoir size is in the order of 10^{39} mole, while the rate of change is in the order smaller than 10^{18} mole/my. For the same reason we can simplify: R_{LS}=1. This set of differential equations (13 through 17) forms the dynamic part of the model (BLAG assumptions).

Assumptions Concerning the Role of the Biota.—Several efforts have been made to include the biota explicitly in long term carbon cycle models (e.g. Volk, 1987; Berner, 1991). Both Volk and Berner included the biota by assuming that the weathering rate is modulated by the vegetation via the CO_2 concentration in the soil. In their models the CO_2 concentration in the soil is directly related to the global terrestrial plant productivity, which follows a Michaelis-Menton response to atmospheric CO_2 (Volk, 1987, 1989; Berner, 1991, 1994).

In our model study, we explored a temperature-dependent biogenic feedback. We modelled this feedback by incorporating a temperature optimum response superimposed on the physico-chemical temperature response in the expression for the relative weathering rate $f(T)$, and by replacing the constant k_{prep} by a function $k_{prep}(T)$ which contains a temperature optimum response. The ideal optimum-response function we used (18) has two parameters: the optimum (x_{opt}) and the dispersion (σ) (De Bruyn, 1976). It describes the dependence of a quantity (in our case a biogenic flux F) on an environmental factor x:

$$\frac{F}{F_{opt}} = e^{-\left(\frac{x - x_{opt}}{\sigma}\right)^2} \qquad (notice:\ 0 < \frac{F}{F_{opt}} \leq 1) \tag{18}$$

where F_{opt} stands for the biogenic flux at the optimum value x_{opt} of environmental factor x. In eq (18), x, x_{opt}, and σ must be expressed in the same units.

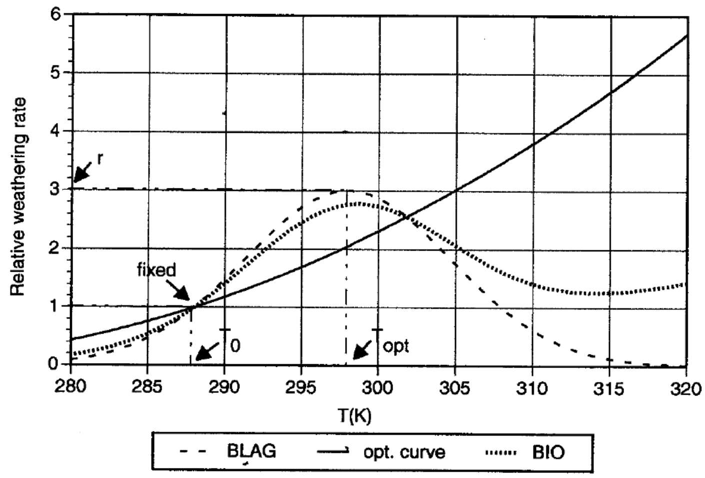

Fig. 2 The relative weathering rate as a function of temperature for 'BLAG'- (5) and 'BIO'- (20) assumptions. The optimum curve (part between [] in (20)) is also plotted. Note that 'BIO'= 0.25·'BLAG'+0.75·'Opt.curve'.

Weathering Fluxes ("Bio-Assumptions").—When the "bio-assumptions" model is used, a correction factor f_{Wbio} is introduced to calculate the weathering fluxes. This factor is an optimum response to the temperature:

$$f_{W_{bio}}(T)=e^{-\left(\frac{T-T_{W_{opt}}}{\sigma_W}\right)^2} . \quad \textbf{(19)}$$

If it is further assumed that at time t=0 a fraction p_{Wabio} is due to abiotic weathering and a fraction p_{Wbio} is due to biogenic weathering, we can replace (5) by:

$$f(T)=p_{W_{abio}}\cdot f_{BLAG}(T)+p_{W_{bio}}\cdot\left[\frac{f_{W_{bio}}(T)}{f_{W_{bio}}(T_0)}\right] \quad \textbf{(20)}$$

where $f_{BLAG}(T)$ is given by (5). T_0 refers to the temperature at t=0 (present temperature). The resulting expressions for the weathering fluxes in the case of the "bio-assumptions" model are given by (7) combined with (20). In our model we assumed that p_{Wabio} = 0.25 and p_{Wbio} = 0.75.
We defined a parameter r_W that represents the intrinsic rate parameter for the biogenic part of the weathering flux. This quantity can be regarded as the maximum relative biogenic weathering rate (relative to the steady state value), that is, the maximum of the part between square brackets in (20):

$$r_W=\frac{1}{f_{W_{bio}}(T_0)}=\frac{1}{e^{-\left(\frac{T_0-T_{W_{opt}}}{\sigma_W}\right)^2}} . \quad \textbf{(21)}$$

This maximum is achieved at the optimum temperature T_{Wopt}, which can be seen from figure 2. As long as we choose optima different from T_0 (288 K), we can use T_{Wopt} and r_W (r_W > 1) instead of T_{Wopt} and σ_W in order to parameterize the optimum curve. Note that the point {T=T_0, *Relative weathering rate*=1} is fixed and independent of the values of the optimum curve parameters (the expression between square brackets in eq 20 always includes this point). With this in mind one can see that increasing T_{opt} in figure 2, at constant r decreases the slope of the curve, whereas increasing r at constant T_{opt} increases the slope. The slope of the curve can be seen as a measure of the feedback strength, since an increase in temperature is counteracted by higher weathering rates which remove CO_2 from the atmosphere. The advantage of using r rather than σ for the parameterization of the optimum curve is that r correlates better to the slope of the curve in the immediate environment of T=T_0.

At this point we make a few remarks about the way in which we incorporate "bio-assumptions" in the model. Firstly, the empirical relation (5) used in the "BLAG'83 assumptions" implicitly takes into account the effects of the biota as they are derived from the real Earth. However, it is uncertain in which temperature domain these relations are quantitatively valid. One of the main features of the biota, that is, its optimum response to temperature and other environmental factors, does not show up in the "BLAG-83 assumptions". Therefore we expect the empirical relation (5) to be quantitatively valid in a narrow temperature range only. Qualitatively we see the "BLAG-83 assumptions" as physico-chemical assumptions. Secondly, the optimum response is superimposed upon a "weakened" BLAG-like response (weakened by a factor P_{Wabio}). This implies that the maximum weathering rate in the "bio-assumptions" model is achieved at a higher T than T_{Wopt} provided $T_{Wopt} > T_0$. This can be seen from figure 2. Thirdly, in our model we assume T_{Wopt} and r_W to be constant over time. In reality, as argued by Knoll and James (1987) and Volk (1989), evolutionary changes in the life strategies of vegetation affect the biogenic rates of weathering and hence T_{Wopt} and r_W. Furthermore, both biotic and abiotic weathering are affected by geological factors such as enhanced weatherability resulting from mountain uplift (Caldeira, 1992). These factors are not included in our model.

$CaCO_3$-Precipitation Fluxes ("Bio-Assumptions").——By analogy with the correction factor for the weathering fluxes, we defined a correction factor for the calculation of the $CaCO_3$-precipitation fluxes in the "bio-assumptions" model. This factor, f_{Pbio} is given by:

$$f_{P_{bio}}(T) = e^{-\left(\frac{T - T_{P_{opt}}}{\sigma_P}\right)^2} \quad \textbf{(22)}$$

We incorporated this optimum function into the model by substituting $k_{prep\,AC} \cdot K_{eq}$ in (11) by $1/\tau$ (12) and rewriting the result in such a format that the net $CaCO_3$-precipitation flux in the ocean can be understood as a carbonate formation term minus a term that represents the dissolution of carbonate in the ocean water:

$$F_{P_{ij}} = k_{prep\,j} \cdot M_{OC} \cdot M_{OB}^2 - \frac{1}{\tau} \cdot M_{AC} \quad \textbf{(23)}$$

In our assumptions, the biota influences the formation term only. The resulting expression for the precipitation fluxes in the "bio-assumptions" model is given by (24):

$$F_{P_{ij}} = k_{prep\,j} \cdot (p_{P_{abio}} + p_{P_{bio}} \cdot \frac{f_{P_{bio}}}{f_{P_{bio_0}}}) \cdot M_{OC} \cdot M_{OB}^2 - \frac{1}{\tau_{BLAG}} \cdot M_{AC} . \quad \textbf{(24)}$$

In our model we assumed p_{Pabio} = 0.25 and p_{Pbio} = 0.75, which can be interpreted as assumptions for the relative contributions to the total precipitation fluxes at t=0 of physico-chemical (abiogenic) and biogenic precipitation respectively. Note that the term between brackets on the right-hand side of (24) equals 1 at t=0. For τ_{BLAG} we assumed 0.001 my.

As we did for the weathering fluxes, we introduce an intrinsic rate parameter r_P for the biogenic component of the precipitation which is defined as:

$$r_P = \frac{1}{F_{P_{bio}}(T_0)} = \frac{1}{e^{-(\frac{T_0 - T_{P_{opt}}}{\sigma_P})^2}} . \quad \textbf{(25)}$$

As long as we choose optima different from T_0, we can use T_{Popt} and r_P (r_P > 1) instead of T_{Popt} and σ_P in order to parameterize the optimum curve.

Further aspects of the model structure.—The differential equations that constitute the dynamic part of the model with "bio-assumptions" are the same as the ones for the model with the BLAG assumptions (eqs 13,14,15,16,17), but with other expressions for $f(T)$ (eq (20) instead of (5)) and F_p (using eq (24) instead of (11)).

The differential equations were integrated over time using the software package ISIM (Interactive SIMulation system). The integration method was 5th order Runge Kutta with variable step size (but step size ≤ 0.01 my).

We defined an instability quotient (I) and a (relative) stability factor (SF) for comparing the model behaviour under different assumptions. The instability quotient of a run is defined as the quotient of the standard deviation of the time series of the measured quantity (in our case, the temperature T) and the standard deviation of the time series of the perturbed quantity (in our case the solar constant S). If the temperature is completely stable with respect to changes in solar luminosity, I equals zero. A high I implies that a perturbation of the solar luminosity leads to large average deviations from the mean temperature.

The stability factor (SF) is defined relative to a reference run in which no feedback can occur, that is, T is calculated from S simply by inserting $CO_2(0)$ and $H_2O(0)$ into eq (A 2) of the app. (constant atmospheric CO_2 and H_2O concentrations, in this case their steady state or t=0 values). For run n it is defined as the inverse of the quotient of I of run n and I of the no-feedback reference run: $SF_n = I_{ref}/I_n$. Remember that I_{ref} is for solar variability only (atmospheric CO_2 and H_2O

concentrations kept constant). An *SF* greater than 1 implies that the net effect of all feedbacks is stabilizing; an *SF* smaller than 1 implies that the net effect is destabilizing, both compared to the no-CO_2+H_2O-feedback reference configuration of the model.

4.4 Results

Climate stability.—We investigated the stability of the global climate (*T* in the model) as a function of the assumptions by calculating the *SF* for a sinusoidal perturbation of the solar constant *S* with a period of 1 my and a relative amplitude of 5 per cent (that is, $S(t)=S_0\cdot(1+0.05\cdot\sin(2\cdot\pi\cdot t))$, *t* in my, S_0=1360 W m^{-2}). The simulation time was 10 my for each case, and we obtained the time series for *S* and *T* from which *I* was calculated by sampling every 0.01 my. As a second indicator for the stability we determined the difference between the highest and lowest temperature in the time series, ΔT. In determining ΔT we cut off the "cold-start" effect (caused by the discontinuity in the first derivative of *S* at *t*=0) by skipping the first 1 my of each simulation. For the reference run (no-feedback by CO_2 and H_2O) we found: *I*=0.05296, and ΔT=7.20 K.

Table 1 gives the results for four different model assumptions for 9 different parameterizations of the optimum curves. The cases with *SF* greater than the *SF* for the run with BLAG assumptions for both weathering and precipitation are printed in bold type. The results in table 1 show that the "bio assumptions" for weathering have a stabilizing effect provided T_{opt} is higher than 288 K (the *t*=0 steady state temperature) and that the stability factor increases with *r* and with T_{opt}. With "bio assumptions" concerning precipitation the figure is the other way around: the effect is destabilizing provided T_{opt} is higher than 288 K.

Table 1 The simulated stability (SF) of the global climate with respect to perturbation of S (see text) for different values of the optimum curve parameters r and T_{opt} for 4 different model configurations. Between brackets: ΔT (K).

T_{opt} (K)	BLAG weathering & BLAG precipitation	Bio weathering & BLAG precipitation			BLAG weathering & Bio precipitation			Bio weathering & Bio precipitation		
		r=2	*r*=10	*r*=100	*r*=2	*r*=10	*r*=100	*r*=2	*r*=10	*r*=100
283		0.646 (12.1)	0.654 (11.5)	0.429 (16.4)	**1.02** (7.26)	**1.81** (4.11)	**2.93** (2.51)	0.654 (12.4)	0.645 (12.0)	0.637 (12.4)
293	0.718 (10.2)	**0.926** (8.27)	**1.61** (4.81)	**1.91** (3.92)	0.491 (13.9)	0.385 (17.0)	0.341 (19.7)	0.619 (12.3)	**0.944** (9.72)	**1.10** (9.89)
303		**0.720** (10.2)	**1.05** (7.33)	**1.40** (5.53)	0.611 (12.0)	0.454 (15.7)	0.362 (19.8)	0.612 (12.0)	**0.770** (10.7)	**0.892** (9.12)

In table 1 we assumed a value for τ_{BLAG} of 0.001 my. We have carried out a sensitivity analysis by reproducing table 1 using a value for τ_{BLAG} of 0.01 my as the maximum and 0.0005 my as the minimum estimate. For τ_{BLAG}=0.01 my, the calculated numbers for *SF* changed by a factor 1 to 3 compared to the runs with the default value τ_{BLAG}=0.001 my. For τ_{BLAG}=0.0005 my the *SF* numbers changed by a factor 0.8 to 1. However, the general pattern of enhanced and reduced stability relative to the run with BLAG assumptions for both weathering and precipitation in table 1 (visualized in the table with bold typeface for higher *SF* and normal typeface for lower *SF*) turned out not to be affected by τ_{BLAG}. There was one single exception: for "Bio weathering and Bio precipitation" with parameter values T_{opt}=293 K and r=2 yielded a higher *SF* than "BLAG weathering BLAG precipitation" with τ_{BLAG}=0.01 my, whereas it yields a lower *SF* then "BLAG weathering and BLAG precipitation" for the cases τ_{BLAG}=0.001 my τ_{BLAG}=0.0005 my.

Further we carried out a sensitivity analysis with regard to the period and amplitude of the perturbation, using a period of 2 my rather than 1 my and a relative amplitude of 1 per cent rather than 5 per cent. In this case the *SF* numbers changed by a factor 0.3 to 3.7. The general pattern of enhanced and reduced stability in table 1 turned out to be insensitive to the period and amplitude of the perturbation with two exceptions: for "Bio weathering and Bio precipitation": T_{opt}=293 K and r=2 yielded a higher *SF* than "BLAG weathering BLAG precipitation", whereas T_{opt}=293 K and r=10 yielded a lower *SF* than "BLAG weathering BLAG precipitation"

Stable limit cycles.—While investigating the model response to a linear increase in the solar luminosity (*S*), we were surprised to find that some parameterizations led to oscillations in the model with "bio assumptions" for both weathering and precipitation. These oscillations began whenever M_{AC} fell below a critical value. An example of such a simulation is given in figures 3 and 4.

In this simulation we imposed a linear increase of 0.5 per cent per my in *S* upon a model configuration with "bio assumptions" for both weathering and precipitation, T_{Wopt}=298 K, r_W=2, T_{Popt}=303, r_P=8, and τ=0.01 my. (note: We chose a higher optimum temperature for precipitation to compensate for the fact that (A) the ocean temperature is lower than the global mean temperature (*T*), and (B) the optimum temperature in our model refers to the optimum global mean atmospheric temperature at sea level.)

Figure 4 presents the mass of the atmospheric CO_2 reservoir for the same run, showing that the increase in *S* is offset by a decrease in atmospheric CO_2. The oscillations start below a critical CO_2 (equilibrium) concentration. A partial explanation is that the temperature effect of a variation dM_{AC} in the atmospheric CO_2 reservoir size increases if the CO_2 concentration decreases. This is due to the logarithmic character of radiation absorption: feedbacks in the climate system acting

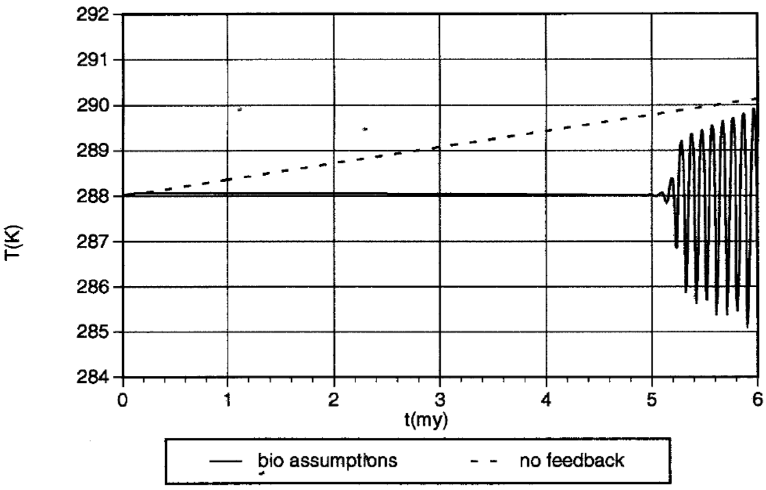

Fig. 3 The calculated temperature in response to a linear increase of 0.5 per cent/my in *S* for the model configuration described in the text.

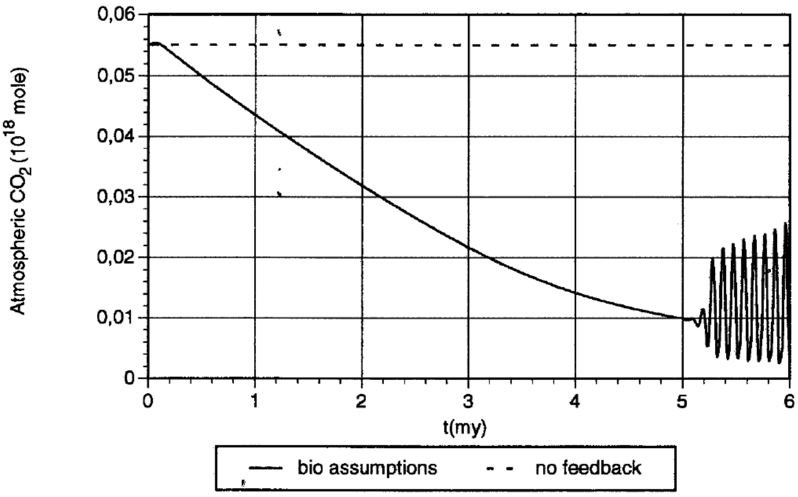

Fig. 4 M_{AC} as calculated in response to a linear increase of 0.5 per cent/my in *S* for the model configuration described in the text.

via the CO_2 concentration become stronger if the CO_2 concentration decreases. In figure 3 we see that the amplitude of the oscillations increases over time, but this is due only to the increase in *S* over time.

If we keep *S* constant, the oscillations turn out to be stable limit cycles (figure

5): if we plot the simulation results of figure 5 in the "phase space" log(M_{AC}) - M_{OB}, a closed curve, which acts as an attractor of the system (figure 6), is described. Experiments showed further that perturbations in the size of M_{AC} are counteracted, which leads to a return of the system to this closed curve: the oscillations are stable limit cycles. In the fields of theoretical ecology and systems theory it is well known that the tension between stabilizing and destabilizing feedbacks can give rise to stable limit cycles (May, 1973). In our case the stabilizing feedback is the feedback via weathering (higher T gives higher weathering rates and consequently higher CO_2 consumption), and the destabilizing feedback is the feedback via precipitation (higher T gives higher precipitation rates and consequently higher CO_2 release).

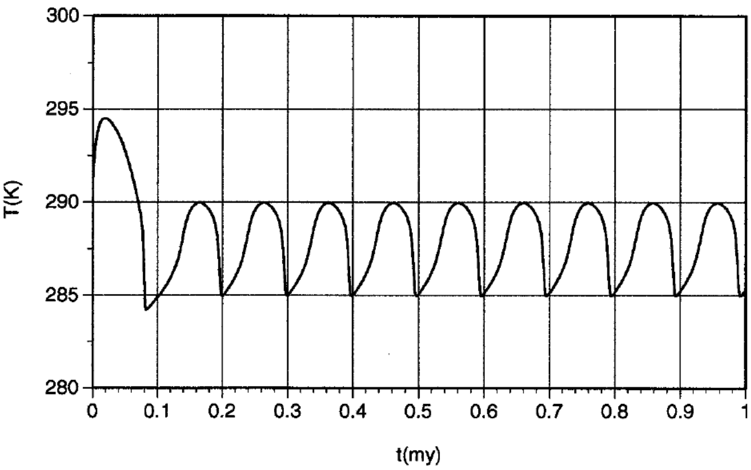

Fig. 5 Simulated temperature response to an instantaneous increase of 3 per cent in S at t=0 for the model configuration described in the text.

We investigated the sensitivity of the result given in figures 3 and 4 to the partition parameters $p_{Wabio} : p_{Wbio}$ and $p_{Pabio} : p_{Pbio}$, which define the relative contributions to the total weathering and precipitation fluxes at t=0 of physico-chemical (abiogenic) and biogenic processes. Note that the sum of the partition parameters for each process is 1 by definition. The results of the sensitivity analysis are given in table 2. The value of p_{Wbio} is not critical to the occurrence of oscillations. However, if p_{Pbio} is chosen smaller than (roughly) 0.5, the oscillations occur only if extremely high values for r_P are chosen. If p_{Pbio} equals zero, the oscillations do not occur any more.

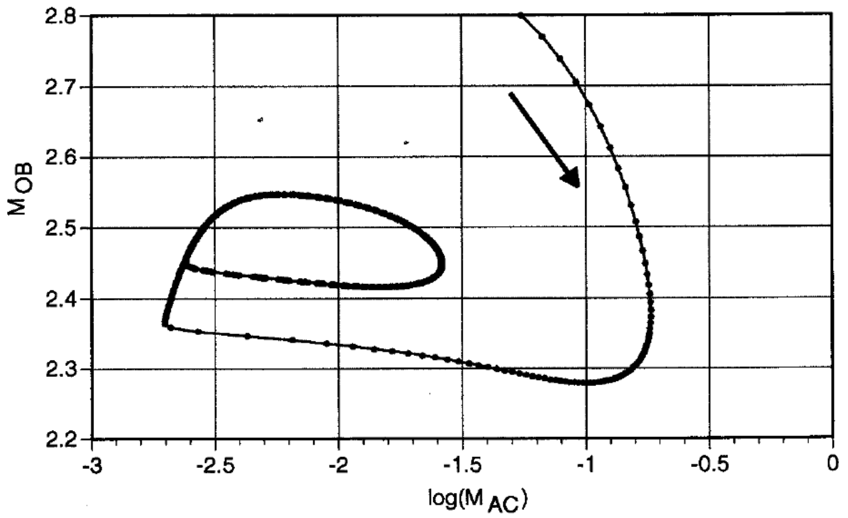

Fig. 6 The simulation results of figure 5 plotted in the "phase space" log(M_{AC}) - M_{OB}. Time increases in the direction of the arrow. The system returns to the closed (equilibrium) trajectory.

Table 2 The consequences of changes in the assumptions for the biogenic share in the weathering and precipitation fluxes at t=0, for the results in figures 3 and 4.

	consequence for		
change in the partition parameters:	critical value of M_{AC}	period of oscillations	amplitude of oscillations
increase of p_{Wbio}	lower	slightly lower	lower
decrease of p_{Wbio}	higher	slightly higher	higher
increase of p_{Pbio}	higher	slightly higher	higher
decrease of p_{Pbio}	lower	slightly lower	lower

We also investigated the sensitivity to τ and the optimum curve parameters. The period of the oscillations turns out to be very sensitive to τ, and also to the optimum curve parameters r_P and T_{Popt}. With τ = 0.001 my, T_{Popt} = 303 K, and *S* = 1400.8 W/m2, we simulated oscillations with periods in the range 54 ky (r_P = 14.7) to 82 ky (r_P = 20). For those who think *S* = 1400.8 W/m2 an unreasonable value: it can be seen from eq (A 2) in the app. that instead of changing *S* into 1400.8 W/m^2 we can also keep *S* at the default value of 1360 W/m^2 and instead change the albedo from *A*=0.350 to *A*=0.369 with the same result. With τ = 0.0005 my we did not manage to simulate oscillations any more (although it should be said that we have not completely scanned the parameter hyper space of the model).

In reality, the rate of increase in solar luminosity (+ 5 per cent per 750 my) is much lower than the rate used in figures 3 and 4 (+ 5 per cent per 10 my). To find out whether the rate of increase in solar luminosity is critical to the model behaviour, we repeated the simulation of figures 3 and 4 with lower rates of increase. The results are given in table 3. The qualitative behaviour turned out to be insensitive to the rate of increase in solar luminosity. The transition to the oscillatory regime became more gradual if the rate of increase was chosen lower. This is because the rate of increase in amplitude of the cycles gets lower. In the case of 5 per cent per 500 my, it takes 10 cycles (1.03 my) before the difference between the highest and lowest temperature in one cycle exceeds 1 K.

Table 3 The consequences of changes in the rate of increase in solar luminosity for the results of figures 3 and 4.

Rate of increase in *S*	Critical value of M_{AC} (10^{18} mole)	period of oscillations (my)
5 per cent/10 my	0.0102	0.095
5 per cent/100 my	0.0110	0.100
5 per cent/500 my	0.0111	0.103

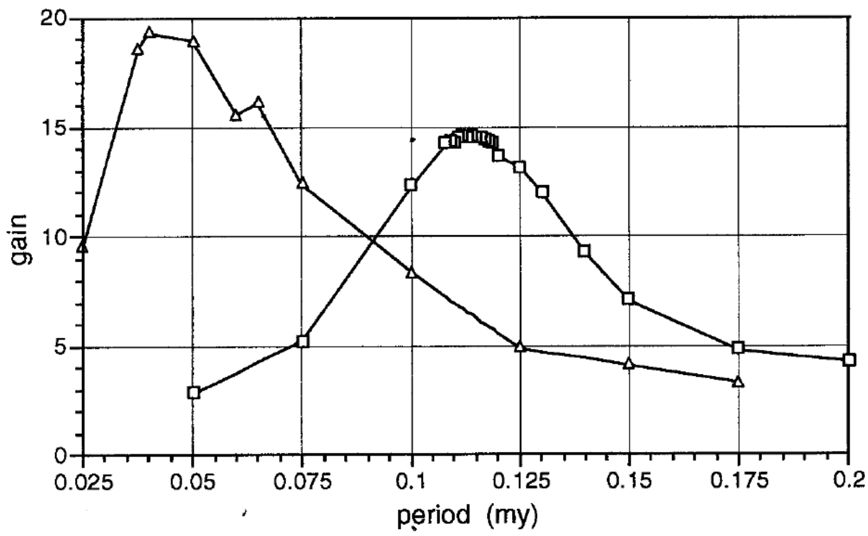

Fig.7 Two cases of frequency selective amplification, see text.

Frequency-dependent amplification.—In near critical conditions, the model amplified external perturbations. The magnitude of the amplification was a function of the period of the perturbation. Figure 7 shows the results of a series of simulations where for different periodicities a sinusoidal perturbation with a relative amplitude of 0.1 per cent (that is: $S(t)=S_0(1+0.001*\sin(2\pi t/p))$, where p is the period in million years) was imposed upon a model configuration with "bio-assumptions" for both weathering and precipitation. For the □-marked line in figure 7 the parameter values for the optimum curves were: T_{Wopt}=298 K, r_W=2, T_{Popt}=303 K, r_P=8 while we assumed τ = 0.01 my. We started with a value of 1390 W/m^2 for solar luminosity (S_0), which is just below the critical value that gives rise to spontaneous oscillations (for the given parameterization). For the △-marked line the parameter values were: T_{Wopt}=298 K, r_W=2, T_{Popt}=303 K, r_P=10, S_0=1410 W/m^2, and τ = 0.001 my. In each simulation we allowed the model for 1 my to settle down into a dynamic equilibrium. Then, during the second my of the simulation we measured the difference between the highest and lowest simulated temperature in each cycle. We defined gain as the quotient of the measured temperature difference in each cycle and the temperature difference in each cycle in a no-feedback run (0.15 K). The maximum gain (14.5) was obtained for a period of 0.112 my for the □-marked line, in which case the simulated temperature amplitude was 2.12 K. For the △-marked line the maximum gain (19.3) was obtained for a period of 0.04 my, in which case the simulated temperature amplitude was 2.81 K.

4.5 Conclusions and discussion

As the results presented in table 1 show, if biogenic feedbacks in the carbonate-silicate geochemical cycle are taken into account by assuming an optimum response for the weathering rate with respect to temperature, then the stability of the global climate (indicated by the stability factor *SF* as defined in this article) with respect to perturbations in solar luminosity is increased, provided optima are higher than T_0 (T_0 is the steady state temperature at the start of a simulation, here 288 K). This conclusion is in agreement with the findings of Kump and Volk (1991) who used an analytical rather than a numerical approach to investigate the role of the biota in climate regulation.

Biogenic feedback (temperature optimum response) for the carbonate precipitation rate decreases the stability of the global climate, provided the optima are higher than T_0. An optimum response for both the weathering rate and the precipitation rate tends to increase the stability of the global climate, provided the optima are higher than T_0.

Kump and Volk (1991) argued that "Gaian regulators" tend to stabilize environmental quantities below the optimum value. Our results confirm this conclusion. Note that if the system were to reach a temperature above the optimum temperature, the biogenic part of the rate-law expression would reach the downward slope of the optimum curve, implying a reversal of the feedback sign from negative to positive, see figure 2. In the cases in table 1 with a ΔT higher than roughly twice the absolute difference between T_{opt} and T_0 (note: 'twice' because ΔT stands for the top-dale temperature difference; 'roughly' because the response might be asymmetrical), this reversal of initially positive feedback into negative feedback actually occurred when T exceeded T_{opt}, which prevented the system from a complete run-away. In the cases with initially negative feedback, the negative feedback prevented the system from exceeding T_{opt} preventing the system from getting instable.

Under near critical conditions, frequency selective amplification of external perturbations occurs. In one of the examples given (□-marked line in figure 7), the main periodicity observed in the Pleistocenic glacial-interglacial cycles, that is, 100 ky, is reinforced by a factor of 12. In the same example the other Milankovitch-periodicities of 41 and 23 ky are reinforced by a factor 2.6 and 1.9 respectively. Such frequency-dependent amplification, caused by biogenic feedbacks, might explain the inversion of the relative importance of the 0.10, 0.041, and 0.022 my periodicities in the linear variance spectrum of the Milankovitch curve with respect to the same periodicities in the spectra of climate data (Pisias and Shackleton, 1984).

The problem of the absence of the 100 ky periodicity from the Milankovitch forcing curve.—The dominant frequency in all ice volume records (and in most other climate records) is a periodic component near 100 ky, which coincides with the period of changes in the eccentricity of the orbit (Hays, Imbrie, and Shackleton, 1976; Pisias and Shackleton, 1984). The presence of this dominant component is not expected from the simple Milankovitch hypothesis, because this period is not represented to a significant extent in the insolation record of any latitude or season (Berger, 1978). Milankovitch forcing is strongest at 23 ky, less strong at 41 ky, and almost absent at 100 ky. It is a widely held view that there should be an other forcing at 100 ky (Broecker, 1984; Pisias and Shackleton, 1984). However, in our model such a forcing is not required because a 100 ky periodicity can be generated internally.

Parameterizations of the optimum curves, which are not a priori unrealistic, led to stable limit cycles in the system, which begin when CO_2 falls below a critical value. CO_2 reduction occurs as the system's response to a gradual increase in solar luminosity. Depending on the values of the temperature optimum curve parameters for biogenic precipitation (T_{Popt} and r_P in the model) and the time constant for overturn of the oceans (τ), the oscillations have periodicities roughly in the range 50 to 150 ky.

Pisias and Shackleton (1984) suggested that the 100 ky Pleistocenic forcing may result from changes in atmospheric CO_2. By presenting variance spectra of the atmospheric CO_2 record derived from the geological record they showed that the relative importance of the 100, 41 and 23 ky periodicities in this spectrum was in agreement with the variance spectra of the climate record. Pisias and Shackleton did not present a mechanism to explain the CO_2 forcing. Although we realize that our model is tentative, the stable limit cycles in simulations under critical conditions do provide such a mechanism. An additional quality of our model is that it shows a sudden transition to an oscillatory state starting when the atmospheric CO_2 concentration falls below a critical level. This transition is due to the system's response to a gradual increase in solar luminosity. The pattern is insensitive to the rate of increase in solar luminosity. This pattern is in agreement with the geological record of the Pleistocene. It should be noted that any forcing (for example tectonic forcing or albedo increase) to which our model responds with CO_2 reduction could potentially yield the same phenomenon.

Our results show striking similarities with the simulation results obtained by Saltzman and Maasch (1988, 1991). Saltzman and Maasch developed a global dynamical model governing the evolution of ice mass, carbon dioxide, and deep ocean temperature over the late Cenozoic. Just like our model, theirs shows the typical transition to glacial interglacial oscillations, and it can bifurcate to a free oscillatory regime that is under the "pace maker" influence of Milankovitch forcing. Although modelled differently, in both models the long term carbon cycle is a crucial part of the oscillator. However, the causative mechanism differs. Although in both models the oscillations start as soon as the CO_2 concentration falls below a critical value, the CO_2 reduction is achieved differently. In our model the CO_2 reduction is due mainly to the systems response to a gradual increase in solar luminosity. In Saltzman and Maasch's model the CO_2 reduction is caused by variations in tectonic forcing. The mechanism could be increased weathering due to rapidly uplifted mountain ranges.

Saltzman and Maasch used their model to simulate the last 5 my of the Earth's climate. Our purpose was different. We explored the consequences of introducing temperature optimum responses for weathering and precipitation for the stability of the global thermostat system comprised by the coupled Urey and Chamberlain reactions, when exposed to perturbations in solar luminosity. We realize that the perturbations used in this model study are exaggerated. We deliberately opted for extreme forcing, however, since it highlights some fundamental consequences of the biological intervention in the long-term carbon cycle.

In conclusion we maintain that our simulations provide evidence that the investigated biogenic feedback mechanisms in the carbonate-silicate geochemical cycle might play a key role in the global thermostat system and the glacial-interglacial cycles.

4.6 Acknowledgments

The main part of this study was carried out in 1990 at the University of Leiden and resulted from collaboration between the Biology Department and the Biogeochemistry Department. We thank Wim Turkenburg, Jan Goudriaan, and Lee Kump for making useful suggestions and for encouraging us to publish these results. We thank Robert A. Berner and an anonymous reviewer for their most useful comments on our manuscript. We are grateful to Sheila McNab for making linguistic and stylistic improvements.

4.7 References

Berger, A., 1978, Long-Term variations of Daily Insolation and Quaternary Climatic Changes: Journal of the Atmospheric Sciences, v. 35, p. 2362-2367.

Berner, R.A., 1991, A Model for Atmospheric CO_2 over Phanerozoic Time: American Journal of Science, v. 291, p. 339-376.

Berner, R.A. 1994, GEOCARB II: A Revised Model of Atmospheric CO_2 over Phanerozoic Time: American Journal of Science, v.294, p. 56-91.

Berner, R.A., Lasaga, A.C., and Garrels, R. M., 1983, The Carbonate Silicate Geochemical Cycle and its Effect on Atmospheric Carbon Dioxide over the Past 100 Million Years: American Journal of Science, v. 283, p. 641-683.

Broecker W.S., 1984, Terminations, *in* Berger, A., Imbrie, J., Hays, J. [et al.], 1984, Milankovitch and Climate: understanding the response to astronomical forcing: Reidel, Dordrecht, p. 687-698.

Caldeira, K., 1992, Enhanced Cenozoic chemical weathering and the subduction of pelagic carbonate: Nature, v. 357, p. 578-581.

Cochran, M.F., and Berner, R.A., 1992, The quantitative role of plants in weathering, *in* Kharaka, Y. K. and Maest, A.S, editors, Water-Rock Interaction: Balkema, Rotterdam, p. 473-476.

Gough, D.O., 1981, Solar Interior Structure and Luminosity Variations: Solar Physics v. 74, p. 21-34.

Hays, J.D., Imbrie, J., and Shackleton, N.J., 1976, Variations in the Earth's Orbit: Pacemaker of the Ice Ages: Science, v. 194, p. 1121-1132.

Kasting, J.F., 1989, Long Term Stability of the Earth's Climate: Paleography, Paleo-climatology, Paleoecology (Global and Planetary Change Section) v. 75, p. 83-95.

Knoll, M.A., and James, W.C., 1987, Effect of the advent and diversification of vascular land plants on mineral weathering through geologic time: Geology, v. 15, p. 1099-1102.

Kondratjev, K.Ya., 1969, Radiation in the Atmosphere: Academic Press, New York, 912 p.
Kump, L.R., and Volk, T., 1992, Gaia's Garden and BLAG's Greenhouse: Global Biogeochemical Climate Regulation, *in* Schneider, S.H. and Boston, P.J., editors, Scientists on Gaia: Cambridge Massachusetts, MIT Press, p. 191-199.
Lasaga, A.C., Berner, R.A., and Garrels, R. M., 1985, An Improved Geochemical Model of Atmospheric CO_2 Fluctuations over the Past 100 Million Years, *in* Sundquist, E.T. and Broecker, W.S., editors, The Carbon Cycle and Atmospheric CO_2, Natural Variations Archean to Present (Geophysical monograph 32): American Geophysical Union, Washington, D.C., p. 397-411.
Lovelock, J., 1988, The Ages of Gaia, a Biography of our Living Earth: Oxford University Press, 252 p.
Lovelock, J.E., and Margulis, L., 1974a, Atmospheric homeostasis by and for the biosphere: the gaia hypothesis: Tellus v. 26, p. 2-9.
Lovelock, J.E., and Margulis, L. 1974b, Homeostatic tendencies of the earth's atmosphere: Origins of life, v. 5, p. 93-103.
May, R.M., 1973, Stability and Complexity in Model Ecosystems: New Jersey, Princeton University Press, 235 p.
Newman, M.J., and Rood, R.T., 1977, Implications of Solar Evolution for the Earth's Early Atmosphere: Science, v. 198, p. 1035-1037.
Pisias, N.G., and Shackleton, N.J., 1984, Modelling the global Climate Response to Orbital Forcing and Atmospheric Carbon Dioxide Changes: Nature, v. 310, p. 757-759.
Sagan, C., and Mullen, G., 1972, Earth and Mars: Evolution of Atmospheres and Surface Temperatures: Science, v. 177, p. 52-55.
Saltzman, B., and Maasch, K.A., 1988, Carbon cycle instability as a cause of the late Pleistocene ice age oscillations: Modeling the asymmetric response: Global Biogeochemical Cycles, v. 2, no. 2, p. 177-185.
Saltzman, B., and Maasch, K.A., 1991, A first-order global model of late Cenozoic climate change: Climate Dynamics, v.5, p. 201-210.
Volk, T., 1987, Feedbacks between weathering and Atmospheric CO2 over the last 100 million years: American Journal of Science, v. 287, p. 763-779.
Volk, T. 1989, Rise of angiosperms as a factor in long-term climatic cooling: Geology, v. 17, p. 107-110.
Walker, J.C.G., Hays, P.B., and Kasting, J.F., 1981, A Negative Feedback Mechanism for the Long-Term Stabilization of Earth's Surface Temperature: Journal of Geophysical Research, v. 86, p. 9776-9782.
Watson, A.J. and Lovelock, J.E., 1983, Biological homeostasis of the global environment: the parable of Daisyworld: Tellus, v. 35b, p. 284-289.
Westbroek, P., 1991, Life as a Geological Force, Dynamics of the Earth: W.W. Norton & Compagny, New York, 240 p.

Appendix A

The 0-dimensional Temperature Model.—The (implicitly used) temperature model in the BLAG'83 model has some shortcomings. The original BLAG'83 model uses an empirical relation between temperature and atmospheric CO_2 obtained from GCM results for $2xCO_2$ and $4xCO_2$ combined with observations of air bubbles in ice:

$$T = T_0 + \frac{\log\left(\frac{M_{AC}}{M_{AC0}}\right)}{0.347}. \qquad \textbf{(A 1)}$$

We have two objections to this greenhouse effect model:
1. If M_{AC} reaches zero, the term $\log(M_{AC}/M_{AC0})$ reaches minus infinity. This might even give rise to simulated temperatures far below 0 K. The real global mean temperature can never fall below the Stefan-Boltzmann equilibrium temperature that results from the radiation balance of the earth with a hypothetical completely transparent atmosphere, which is 256 K (assuming S=1360 W/m^2 and Albedo=0.35).
2. The greenhouse effect modelled as $T = T_0 + F_{gh}(M_{AC})$, in which $F_{gh}(M_{AC})$ stands for temperature increase as a function of M_{AC}, will only be valid in a small temperature domain. We prefer to calculate the greenhouse effect from the radiation balance rather than from empirical relations derived from limited empirical data.

Our model contains a simple 0-dimensional radiation balance climate model, which takes into account the greenhouse gases CO_2 and H_2O. In our model we calculate the temperature from:

$$T = \left[\frac{0.25 \cdot S \cdot (1 - A)}{\varepsilon \cdot \sigma \cdot P_I(H_2O) \cdot P_I(CO_2)}\right]^{0.25} \qquad \textbf{(A 2)}$$

where S stands for the solar luminosity in W m^{-2} (1360 W m^{-2} at t=0), A is the global albedo, which is assumed to be 0.35, ε is the mean emissivity of the earth-surface in the middle infrared and is assumed to be 0.9 (Sagan and Mullen, 1972), σ is the Boltzmann constant (= 5.66961 10^{-8} W m^{-2} K^{-4}). $P_I(H_2O)$ and $P_I(CO_2)$ are transmission functions of CO_2 and H_2O in the atmosphere respectively.
The transmission function for CO_2 is a Kuznetov function adopted from Kondratyev (1969), (we adjusted the constants to match our unit for M_{AC}):

$$P_I(CO_2) = 0.8 + 0.04 \cdot PC1 + 0.08 \cdot PC2 + 0.08 \cdot PC3 \qquad \textbf{(A 3)}$$

with

$$PC1 = 0.1 \cdot \exp(-191 \cdot M_{AC}) + 0.9 \cdot \exp(-1.64 \cdot M_{AC}) \qquad \textbf{(A 3a)}$$
$$PC2 = 0.5 \cdot \exp(-259 \cdot M_{AC}) + 0.5 \cdot \exp(-10.8 \cdot M_{AC}) \qquad \textbf{(A 3b)}$$

$$PC1 = 0.95 \cdot \exp(-1230 \cdot M_{AC}) + 0.05 \cdot \exp(-9.49 \cdot M_{AC}) \quad \textbf{(A 3c)}$$

The transmission function for water vapour is also adapted from Kondratyev:

$$P_I(H_2O) = 0.25 \cdot (P_I1 + P_I2 + P_I3 + P_I4) \quad \textbf{(A 4)}$$

with:

$$P_I1 = \exp(-0.353 * M_{AH2O}) \quad \textbf{(A 4a)}$$
$$P_I2 = \exp(-4.03 * M_{AH2O}) \quad \textbf{(A 4b)}$$
$$P_I3 = \exp(-69.2 * M_{AH2O}) \quad \textbf{(A 4c)}$$
$$P_I4 = \exp(-402.7 * M_{AH2O}) \quad \textbf{(A 4d)}$$

We modelled the relation between M_{AH2O} and temperature with a linear approximation obtained from a fit of (A 2) to the greenhouse function used in the BLAG'83 model (A 1) for the temperature values [286,287,288,289,290,291,293,295,297,299] K. For each temperature of this set the corresponding value for M_{AC} was calculated from (A 1). This value was filled in (A 2) and then the value of M_{AH2O} required to obtain the same temperature was determined. This resulted in a set of points (T, M_{AH2O}) through which we determined the best fit of a linear function T=a+b$\cdot M_{AH2O}$. The result was:

$$M_{AH2O} = -0.02765532 + 0.00011 \cdot T. \quad \textbf{(A 5)}$$

We realize that the values of M_{AH2O} obtained with this calculation method might not correspond to the real atmospheric water vapour content. It allows us to expand the temperature domain of the BLAG'83 temperature model in such a way that the qualitative aspects of the model are realistic, even at extremely low and high values for M_{AC}, whereby the quantitative aspects of the relation between M_{AC} and T (in the temperature domain between 285 and 300 K) are in reasonable agreement with the original BLAG'83 expression.

Chapter 5

Possibilities and Limitations of Integrated Assessment Models for the Climate Issue

5.1 Introduction
5.2 The emergence of IAMs as a science-policy interface
5.3 What are IAMs?
5.3.1 Definitions of IAMs
5.3.2 Variability in IAM-modelling practice
5.4 Key uncertainties and limitations faced by IAMs of the climate issue
5.4.1 Culture and Values
5.4.2 Demands for goods and services
5.4.3 Choice of technologies and practices
5.4.4 Fluxes of material in the environment
5.4.5 Valued Environmental Components
5.4.6 Exposure
5.4.7 Consequences
5.5 The usefulness and use of IAMs for the climate issue
5.5.1 The policy-usefulness of climate IAMs
5.5.2 The context of use of IAMs
5.6 Conclusions
5.7 Acknowledgements
5.8 References

Abstract

Integrated Assessment Models of climate change (IAMs) play an increasingly important role in climate risk assessment. In this chapter we analyse uncertainties and limits to predictability encountered in each stage of the causal chain of climate change which IAMs attempt to represent. In each stage of the causal chain we identify both potentially reducible and probably irreducible uncertainties affecting the outcomes of climate risk assessment. The state of science that backs the mono-disciplinary sub-models of IAMs varies from educated guesses to well-established knowledge. It is also uncertain to what extent the IAMs complete. We also explore the usefulness of IAMs to guide and inform the policy process. Although some experts maintain that we are not ready for integrated assessment, the models are currently used to address policy questions. It is highly questionable whether such use is justifiable, unless all actors that deal with IAMs and IAM results are fully aware of the limitations and caveats of IAM assessments.

5.1 Introduction

In the mid-eighties Integrated Assessment Models (IAMs) emerged as devices for integrating knowledge from different disciplines and for interfacing science with policy regarding environmental problems. A perfect IAM would model the complete so-called causal chain, including all the feedbacks within this chain. The causal chain starts with socio-economic drivers leading to economic activity and other practices, leading to emissions and other pressure on the environment leading to environmental changes, leading to physical impacts on societies and ecosystems, leading to socio-economic impacts, eventually returning to cause changes in the socio-economic drivers. The idea is that such an integrated model can be used as an instrument to evaluate and compare the consequences of (combinations) of policy-measures, or to select an optimal mix of policy-measures to meet a specified target. Examples of IAMs are the RAINS model (Regional Acidification INformation and Simulation) for acidification in Europe (Alcamo *et al.*, 1990) and the IMAGE model (Integrated Model to Assess the Greenhouse Effect) for the climate issue (Rotmans, 1990; Alcamo, 1994). In this chapter we explore the possibilities and limitations of climate IAMs in view of the tasks of model developers to model the complete cause-effect chain and to provide tools to guide and inform the policy process.

There are controversies among experts regarding the usefulness of IAMs for addressing climate change in the light of the huge uncertainties and unresolved scientific puzzles in this field. In their evaluation of energy models, Keepin and Wynne (1984) noted that the identification of ’objective policy truths’ or objective answers to policy questions is an unrealistic aim in science for policy. Similar notions are put forward by other authors. For instance, Giarini and Stahel (1993) observe "*...the starting of a new era of challenges and opportunities in the evolution of human society; an era in which an unrealistic quest for certainty will be replaced by an understanding of its limits.*".

As a contribution to a better understanding of the limits of science in relation to its policy use in IAMs, we seek answers to the questions: What are the possibilities and limitations of IAMs in relation to the tasks of the modellers to model the complete cause-effect chain and to guide and inform the policy process?

In the following, we first describe the rise of IAMs in the mid-eighties as devices for interfacing science with policy. We describe what IAMs are, in the detail that is necessary for a good understanding of the following sections. Then, we discuss the key uncertainties and limitations in each stage of the causal chain of the climate issue. Next, we discuss the controversy surrounding the (policy) usefulness of IAMs for the climate issue. Finally we consider the different contexts of use of IAMs.

5.2 The emergence of IAMs as a science-policy interface

Facilitated by developments in computer technology, *integrated modelling* emerged in the mid-eighties as a new paradigm for interfacing science and policy concerning complex environmental issues. In the second half of the eighties, it was believed that integrated modelling would be the optimal way to interface science with policy (Zoeteman, 1987). Parson (1994) claims that: *"To make rational, informed social decisions on such complex, long-term, uncertain issues as global climate change, the capacity to integrate, reconcile, organize, and communicate knowledge across domains - to do integrated assessment - is essential."* Integrated assessment models can produce insights that cannot be easily derived from the individual natural or social science component models that have been developed in the past (Weyant, 1994).

The first generation of these integrated models focused on acid rain. The RAINS model (Regional Acidification INformation and Simulation) is the most obvious example (Alcamo *et al.*, 1986; Alcamo *et al.*, 1990; Hordijk, 1991a). RAINS was developed in the eighties at the International Institute for Applied Systems Analysis (IIASA). The RAINS model played a major role in the international acid deposition negotiations in the framework of the United Nations Convention on Long-Range Transboundary Air Pollution and became an annex to the United Nations SO_2-protocol (Hordijk, 1991b, 1995). In the second half of the eighties, the Netherlands National Institute of Public Health and Environmental Protection (RIVM) developed the IMAGE model (Integrated Model to Assess the Greenhouse Effect), which was a pioneer in the field (De Boois and Rotmans, 1986; Rotmans, 1990). IMAGE has been used for scenario calculations in the influential Netherlands policy document "Zorgen voor Morgen" (Concern for Tomorrow; Langeweg, 1988) and for the development of emission scenarios for the assessments by the Intergovernmental Panel on Climate Change (IPCC), the latter in combination with the Atmospheric Stabilization Framework of the US Environmental Protection Agency (Swart, 1994a). The substantially revised version of the model, IMAGE 2 (Alcamo, 1994a), has been used by all three working groups of IPCC, mainly for developing reference and policy emission scenarios (Swart, 1994b; Alcamo, 1994b, Science and Policy Associates, 1995). Results produced by IMAGE 2.0 and IMAGE 2.1 were presented to the negotiators at the United Nations Conference of Parties to the Climate Convention (COP-1) in Berlin, March, 1995 (Alcamo *et al.*, 1995) and follow up (Alcamo and Kreileman, 1996), and at the second meeting of the COP in Geneva, July 1996 (Swart *et al.*, 1996).

Over the past few years, the number of integrated assessment models of the climate problem (hereafter referred to as *IAMs*) has grown significantly. In 1990, there were 3 IAMs: RIVM's IMAGE model (De Boois and Rotmans, 1986; Rotmans, 1990), WRI's Model of Warming Commitment (MWC, Mintzer, 1987), and Nordhaus' model (1989, 1990). Currently there are at least 40 IAMs addressing the climate issue (see the appendix to this chapter for a list).

At present, one of the main policy questions being addressed by IAMs concerns the operationalization of Article 2 of the United Nations Framework Convention on Climate Change (FCCC): *"The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner."*

Article 2 is operationalized in recent IMAGE modelling studies by means of defining 'safe landing zones' and corresponding 'safe emission corridors'. The 'safe landing concept' is a metaphor. If a plane goes down too slowly, it may miss the runway and crash beyond it. If a plane goes down too quickly it may crash before it reaches the runway. In climate terms: if policies are too few and too late, serious climate impacts may be unavoidable. If measures are too strict and too early, economic effects may be unacceptable (Swart *et al.*, 1996). Safe emission corridors are defined as the allowable lower and higher bounds of greenhouse gas emission scenarios, related to a set of criteria for climate policy whose purpose is to protect both the environment and the economy from disruption. The goals put ecological constraints on the upper limit and economic constraints (maximum allowable emission reduction) to the lower limit of the corridor. Such goals are based on scientific assessment but the selection of their level is a political process. Among the attempts to provide scientific rationales for the level of such goals are those by Krause (1989), Rijsberman and Swart (1990) and Jäger (1990). A typical set of such climate goals used in IMAGE 2 safe landing studies is (Swart *et al.*, 1996):

i. change in global surface temperature relative to 1990 ≤ 2.0°C;
ii. rate of temperature change ≤ 0.2°C/decade;
iii. sea level rise relative to 1990 ≤ 40 cm;
iv. maximum rate of emission reduction ≤ 2 %/yr.

In terms of Article 2, goals (i) and (iii) relate to the ’prevent dangerous anthropogenic interference with the climate system’, goal (ii) relates to the ’time frame sufficient to . . .’, whereas goal (iv) relates to the ’enable economic development to proceed in a sustainable manner’. The idea is that a higher rate of emission reduction would disrupt the economy. Using IMAGE 2.1, Alcamo and Kreileman (1996) calculate for the set of goals given above[1], that the ’safe emission corridor’ or allowable global emissions of all greenhouse gases together in 2010 range from 6.2 to 14.1 Gt C/yr CO_2 equivalents. The width of the corridor (that is: the spread in allowable emission) depends on the set of climate goals *i* up to and including *iv* chosen. When the limits are set twice as strictly (that is: divided by two), the allowable range becomes 6.9 to 9.1 Gt C/yr CO_2 equivalents. Current emissions are about 9.6 Gt C/yr CO_2 equivalents. Note that the higher low-end of the range (6.9 rather than 6.2) is due to the stricter limit for goal iv.

The typical set of safe landing criteria was the result of a series of workshops on "Using the IMAGE 2 model to support climate negotiations", attended by the IMAGE 2 modelling group and policy-makers of the Dutch Ministry of Housing Physical Planing and the Environment (the ’Delft workshops’ held in 1995 and 1996). The method of direct interaction between modellers and policy-makers was a lesson learned from the RAINS experience, where it turned out to be a key-ingredient of getting RAINS accepted as a tool for informing the negotiations on the UN Sulphur Protocol (Alcamo *et al.*, 1996). The ’safe landing’ criteria are in fact ’negotiation-constructs’ whose function is comparable to the ’critical-loads’ in the RAINS model: it provides a very simple computable rule to which the negotiators are committed, for distinguishing between good and bad

[1] with one exception, because in Alcamo and Kreileman’s study a different typical goal for the maximum rate of emission reduction was used, namely ≤ 4 %/yr. After this study was completed, the earlier mentioned Delft workshops took place, where it was concluded that a goal of ≤ 2 %/yr was more realistic, see also Swart *et al.* (1996). Further, it should be noted that for pragmatic reasons the safe emission corridor was compromised in the calculations, because the limit for rate of temperature change (goal ii) was allowed to be violated for one decade. This was done because relatively high rates of temperature change occur for a limited number of years in almost all emission profiles analyzed in their study. In other words, without compromising the safe landing criteria, safe landing zones wouldn’t exist.

in climate policies. This method allows the evaluation of policy scenarios and the testing of the extent to which the simulated climate stays within the safe landing zones defined by the four climate goals, given the total set of assumptions that constitute the model. Whether this really indicates that given a scenario we will stay in that zone depends on the validity of the model, and whether staying within in that zone really is safe depends on the validity of the operationalization of 'safe' with the four climate goals and the judgement of safety. Despite the practical success of such constructs in negotiations, unfortunately the answer to both questions is 'we don't know and we won't know' (see also Ravetz, 1986). Nevertheless, IAMs are used and will be used to address such questions.

5.3 What are IAMs?

With regard to the climate issue, a perfect IAM would analyse the full cycle of socio-economic drivers, economic activity, anthropogenic emissions of greenhouse gases, their concentrations in the atmosphere, the resulting climate forcing, climate change, sea level change etc., and finally the impacts of these on ecosystems, the economy, food production systems, water supply systems, as well as other human activities. On the one hand, 'integrated' refers to both the completeness that is aimed at and the inclusion of the feedback loops in and between the represented coupled cause effect chains (Rotmans, 1994). On the other hand, 'integrated' refers to the notion that these models bring together information and analysis from disparate disciplines (Parson, 1994, Weyant, 1994).

5.3.1 Definitions of IAMs

The literature shows a variety of definitions of IAMs, which we have summarized in Table 5.1.

Table 5.1 Definitions of Integrated Assessment and Integrated Assessment Models.

Author	Definition of IAM
Swart, 1994a p.128	"Integrated models are defined here as interdisciplinary models that capture the full cause-impact chain of the climate change problem and all sectors involved. Thus, in this definition, no integration across environmental themes is implied."
Parson, 1994	"The two defining characteristics are a) that it seek to provide information of use to some significant decision-maker rather than merely advancing understanding for its own sake, and b) that it bring together a broader set of areas, methods, styles of study, or degrees of confidence, than would typically characterize a study of the same issue within the bounds of a single research discipline."

Weyant, 1994 p.3	"One definition of an integrated assessment model is a model that projects future economic activity and the links between it and greenhouse gas emissions, between emissions and atmospheric concentrations and climate change, between climate change and the physical impacts on economies and ecosystems that result, between those physical impacts and their economic valuation. Following Parson (1994), however, for our purpose here we define any climate change focused model that links together information and analysis from disciplines that have not traditionally been combined as an "integrated assessment" model."
Rotmans and Dowlatabadi, 1995	"Integrated assessment can be defined as an interdisciplinary process of combining, interpreting and communicating knowledge from diverse scientific disciplines in such a way that the whole cause-effect chain of a problem can be evaluated from a synoptic perspective with two characteristics: (i) it should have added value compared to single disciplinary assessment; and (ii) it should provide useful information to decision makers"
Toth, 1995 p257	"In this paper, and throughout the collection that follows, the terms 'integrated model' and 'integrated assessment' refer to a set of formal models or studies without modelling support that are combined into a consistent framework to address one or more issues in the problem of global climate change."
Kasemir *et al.*, 1996; Baily *et al.*, 1996	"As integrated environmental assessments we understand procedures to arrive at an informed judgement on different courses of action with regard to environmental problems. The information required refers to physical, chemical, biological, psychological, socio-economic and institutional phenomena, including the relevant decision-making processes."

In this thesis we define an *integrated assessment model* as a mathematical representation of a coupled natural system and a socio-economic system, modelling one or more *cause-effect chains* including feedback loops, and explicitly designed to serve as a tool to analyse policies in order to guide and inform the policy process, mostly by means of scenario analysis. This explicit policy purpose defines the difference between IAMs and Earth System Models (ESMs) such as Atmosphere Ocean General Circulation Models (GCMs) and geochemical models, which are designed primarily for scientific purposes. It should however be noted that ESMs such as GCMs could also be used (and in fact they are) to look at policy questions.

The integration across disciplines, which is in Swart's, Parson's, Weyant's, Rotmans and Dowlatabadi's and Kasemir *et al.*'s definitions (see Table 5.1) is the consequence of including both the natural and the social system and representing in principle complete cause-effect chains. In the third row of Table 5.2, we give examples of disciplines that are competent to deal with elements of each of the stages of the causal chain. Multidisciplinarity is indeed a general characteristic of IAMs, but not unique to IAMs. In fact, many ESMs also are multidisciplinary. The two unique characteristics of IAMs are that they (1) integrate the natural system and the socio-economic system in one model and (2) have an explicit mission to perform policy analysis in order to guide and inform the policy debate.

A computer and a - user-friendly - software implementation of the IAM, including a user interface, are used to numerically integrate the model through time with a user-definable set of presumed future time series of input variables (e.g. population growth). Such a set of presumed future time series is called a *scenario*. Integrated models also permit the inclusion of a presumed portfolio of measures in a scenario, such as the introduction of carbon tax, a switch to renewables, reforestation, etc.

Janssen and Rotmans (1995) distinguish two different approaches in current IAM-practice: the macro-economic parameterized approach and the biosphere-climate process-oriented approach. The macro-economic models aim at deriving cost effective strategies to cope with the climate problem, whereas the biosphere-climate process oriented models aim at analysing the consequences of human activities. In this chapter we focus on the biosphere-climate process oriented IAMs. Most of our examples and illustrations from the current modelling practice come from the IMAGE model, which serves as a case study (see also Van der Sluijs, 1995).

Biosphere-climate process oriented IAMs have a modular structure of sub-models. For instance, the IMAGE 2 model consists of three sub-systems consisting of - in total - 13 sub-models. Each sub-model has its roots in a different scientific discipline. The individual sub-models are more or less radically simplified and aggregated input-output models that are usually derived from comprehensive models. While the comprehensive models usually have high process detail and consist of mathematical equations that directly reflect the processes as we think they occur in reality, the simplified modules in IAMs are more like calibrated black- or grey-boxes. This simplification is the inevitable consequence of computer limitations and the mission of IAMs to address policy questions: To be of use to the policy-making process, IAMs need to facilitate the comparison of many different user-definable scenarios and policy options in a reasonable time frame. If the most comprehensive model available were to be used for each sub-model, the calculation of one scenario would take several years of calculation time, even on the fastest super-computer. An ideal of IAM-modellers is to produce a model that can be used interactively by a policy-maker on her or his own desk-top PC and that gives results that do not differ significantly from the hypothetical IAM that would result from choosing the most comprehensive models available for each module.[1]

5.3.2 Variability in IAM-modelling practice

Differences in outcomes of IAM-assessments are partly due to differences in modelling approach and modelling techniques. In the following we discuss inter-IAM variability and its major sources. There are several techniques for designing aggregated simple input-output models from comprehensive high process-detail models. Meta-models can be generated by fitting simple mathematical equations to input-output data from the comprehensive model, or by aggregating

[1] It should be noted that there are hooks and eyes associated with this ideal, mainly related to the issues of quality control that emerge if IAMs are used outside the scientific community. See our discussion on quality control and the Sununu example in section 6.4 of the next chapter.

inputs and outputs by sensitivity analysis (Hordijk, 1994). The degree of simplification depends on the state of knowledge, the level of aggregation required and the complexities of the systems. All modules in an IAM needs to have about the same level of detail. Differences in aggregation level across modules are often handled by (statistical) interpolation and aggregation techniques. These techniques are based partly on somewhat arbitrary (though educated) assumptions.

Differences in the aggregation level of sub-models are not the only problems encountered when sub-models from different disciplines are coupled in an IAM. In the IIASA Integrated Assessment project, the geographical regions used in the population model were different from the geographical regions in the energy model. Other problems are that there may be differences in the basic assumptions underlying the submodels or the same parameter may have different values when this parameter is used in more than one sub-model. When coupling the models, one has to go back to the basic assumptions to make sure that they are consistent throughout the IAM. Sometimes, model-technical tricks are used to circumvent these problems rather than solving them.

Climate IAMs differ in a range of aspects. Weyant (1994) classifies the IAMs as being (1) either more or less complex in representing the natural science process associated with climate change, (2) either more or less complex in representing the economic process associated with climate change (3) either explicitly incorporating uncertainty or not explicitly incorporating uncertainty. Toth (1995) emphasizes other aspects: They can be partly or fully integrated, depending on how much of the cause-effect chain is covered. They differ in the level of integration. The most advanced models integrate equations in one single system. Other models use different techniques of hard and soft linking to transmit data between individual modules. In the case of hard linking, the sub-models exchange information while they run simultaneously in one framework, in the case of soft linking, results from one sub-model are used sequentially as input for the calculations of other sub-models. In that case the sub-models can even be run by different modelling groups on different computers in different buildings, as is the case with the IIASA integrated assessment project.

A closely related aspect concerns the completeness regarding the feedbacks accounted for. Especially the feedbacks between variables in different sub-models need to be taken into account in IAMs. IAMs differ in the extent to which such feedbacks between different stages are taken into account dynamically during the simulation. Dynamically means that all feedbacks are evaluated and take effect after each single time step of numerical integration of the differential equations that constitute the system (or after each discrete time step in discrete models), before the next numerical integration step (or discrete time step in discrete models) is performed (see e.g. Jacoby and Kowalik, 1980). According to Weyant (1994): *"very few of the operational models include interactions and feedbacks between modules other than a straight pass through of information from one module to the next. The IMAGE 2.0 and the AIM models are notable exceptions".* The TARGETS model and the MIT model also include complex interactions but were not yet operational when Weyant wrote his review.

IAMs also differ in comprehensiveness in terms of the degree to which they include sources and sinks of all greenhouse gases. Most of the climate IAMs look at CO_2 emissions and energy economy only. IMAGE is one of the exceptions that considers non-CO2 greenhouse gases and has a detailed representation of land-use changes and their effect on emissions and interaction with climate variables.

Further, IAMs differ in the level of aggregation and disaggregation. Aggregation is defined as the joining of more or less equivalent elements that exhibit mutual interaction (Goudriaan, 1993). Aggregation can take place across different scale-dimensions, leading to different resolutions on these scales. The most relevant scale dimensions in IAMs are: temporal scale (e.g. diurnal; seasonal; annual), spatial scale (e.g. local; regional; continental; global), systemic scales (e.g. individual plants; ecosystems; terrestrial biosphere), and conditional scales[1] (e.g. ecosystem internal variability; inter-ecosystem variability; global variability).

Sub-models of IAMs can be either deterministic or stochastic. In deterministic models all parameters and variables of the model have point values at any given time. In stochastic models the parameters and variables are represented by probability distribution functions. Monte Carlo Simulation is the most powerful technique for stochastic model-calculations. In its simplest form, Monte Carlo traces out the structure of the distributions of model output by calculating the deterministic results (realizations) for a large number of random draws from the distribution functions of input data and parameters. There are intermediate models in which only some of the parameters are stochastic, or in which parameters and variables are represented by a two-fold range (high estimate, low estimate) or a three-fold range (high estimate, best estimate, low estimate) rather than a probability distribution function.

When Rotmans started with IMAGE 1 in 1986, he chose a deterministic approach, for several reasons. Information available regarding the distribution functions of most parameters and variables was, and still is, insufficient. Rotmans (1994) did not see any advantage in assuming normal distributions or uniform distributions for unknown distributions. According to him, to do this would suggest exactness, which does not correspond to our current state of knowledge.

Rotmans also stresses that distribution functions based on the best available knowledge change over time due to the progress of research. *"The dynamics in perceived uncertainties is tremendous"* (Rotmans, 1994). Alcamo (1994b) is also sceptical about stochastic IAMs: *"It is hard enough to build deterministic models. Making the model stochastic does not solve your uncertainty problem. It makes it more explicit for better or for worse."* To him it is far from clear whether Monte Carlo or any kind of stochastic simulation will work when one analyses the uncertainty of a multi-component integrated model of the global environment. Further, he sees no reason why stochastical modelling should be *a priori* better than deterministic modelling followed by uncertainty analysis.

Weyant (1994) is more positive about stochastical modelling than are Alcamo and Rotmans: *"This is a formidable task, but one that can be immensely valuable if completed successfully. Successful completion of this enterprise will require model integration and management that has rarely been achieved in the past"*. Weyant further notes that: *"It is also possible that further disaggregation and more explicit treatment of uncertainty would not lead to different insights than produced by these simpler deterministic models."*

---

[1] By conditional scale we mean the variability in state conditions. This variability is minimal in the laboratory, where we can control all conditions and vary one variable, whereas it is maximal for the global averaged system. This scale level is for instance relevant for the calculation of the CO_2 fertilisation effect within a climate IAM. The CO_2 fertilisation effect depends on the type of vegetation and the extent to which CO_2 is a factor limiting plant growth. This, in turn, depends on state conditions such as nutrient and water availability. Hence, for a globally averaged terrestrial biosphere, this is much more difficult to determine than for a well defined ecosystem in a specific region where vegetation composition and soil-type are given.

Young and Parkinson (to be published) strongly criticize the current practice in which stochastic dynamic models are the exception rather than the rule in environmental science research. According to them: *"Until comparatively recently, MCS* [Monte Carlo Simulation] *was difficult to justify because it absorbed large amount of computing power and occupied unacceptable large computing time. Unless the model is very large, as in the case of the GCMs, this is no longer the case, however, since the rapid improvements in computer power have even made it possible to carry out MCS studies for most moderately sized simulation models on desk top personal computers. Larger models, (...), still presents problems for desktop machines or workstations but, (...), the analysis can be carried out quite straight forwardly on parallel processing computers which are ideally suited to the generation of the multiple realizations required by MCS."* It should however be noted that parallel processing computers are not yet widely disseminated.

Schimmelpfennig (1996) analysed the representation of uncertainty in economic models of climate impacts and reviewed the methodologies available for characterizing uncertainty. He found that uncertainty is poorly represented in existing studies of climate impacts. He criticizes this from the notion that *"when only mean values are presented as results, most of the information about the underlying distributions of random variables has been discarded."* For the economic models, he came to the same conclusion as Young and Parkinson for the natural system models: *"What is needed are Monte Carlo type simulations"*.

Regarding our own position in respect of the question of deterministic or stochastic integrated assessment modelling: We disagree with Rotmans' first argument. In our view its is better to have a first approximation of the uncertainty and its propagation through the model, based on imperfect assumptions regarding distribution functions, than the alternative which is the spurious exactitude suggested by single-value 'predictions' of deterministic models. We disagree with his Rotmans' third argument that there would be no reason why stochastical modelling should be *a priori* better than deterministic modelling followed by uncertainty analysis. The propagation of quantified uncertainty through a model can only fully be analysed by stochastical modelling. Hence, by choosing a deterministic approach, followed by sensitivity and uncertainty analysis, one discards a possibly essential part of the information on the propagation of uncertainty through the model. Regarding Weyant's second notion that it is also possible that more explicit treatment of uncertainty would not lead to different insights than produced by these simpler deterministic models, we would like to stress that the reverse is also possible. For all these reasons we strongly agree with Young and Parkinson's and with Schimmelpfennig's criticism: what is needed for uncertainty analysis are Monte Carlo type simulations.

Summarizing, process oriented IAMs differ in:

i) simplification techniques to draft sub-models;
ii) degree of simplification;
iii) consistency of basic assumptions throughout sub-models;
iv) process detail;
v) complexity in representation of the natural system;
vi) complexity in representation of the socio-economic system;
vii) level of integration;
viii) feedbacks accounted for;

ix) method and comprehensiveness of including feedbacks in the model;
x) comprehensiveness of greenhouse gases and their sources and sinks accounted for;
xi) level of aggregation across a range of scale dimensions;
xii) extent to which uncertainties are explicitly accounted for (stochastic model, deterministic model or intermediate model).

According to Parson (1994), there is no consensus regarding the best approach: *"Perhaps the most serious consequence of the immaturity of the field is that there is no shared body of knowledge and standards of 'best practice' for integrated assessment. Such knowledge is likely to develop with more thought and practice, but its present absence makes it ill-advised to pursue a single, authoritative vision of integrated assessment. On both intellectual and managerial dimensions, there are many plausible ways of addressing the most basic challenges of integrated assessment. There is no single right way to do it."*

5.4 Key uncertainties and limitations faced by IAMs of the climate issue

The task of the IAM community is modelling the entire cause-effect chain of anthropogenic climate change in one integrated model. In this section we explore the possibilities and limitations of IAMs in relation to this ambition. Therefore, we analyse the problems encountered in each stage of the causal chain, following the causal taxonomy developed by Norberg-Bohm *et al.* (1990) (first row in Table 5.2), to which we have added 'Culture and Values'[1]. In Table 5.2, we show key uncertainties and limitations in modelling future behaviour for each stage of the causal chain. The key uncertainties listed in the table are gathered mainly from a review of the literature and from personal communication with experts, combined with our own expertise. The table does not pretend to be complete, but rather provides illustrative examples of the various kinds of limitations and uncertainties that are encountered in the assessment of the possible future behaviour of key constituents in each stage of the causal chain.

We will discuss the uncertainties in each step of the causal chain in more detail. Because the process-oriented climate IAMs (which are the subject of this chapter) are more focused on the natural system than on the socio-economic system, the best represented parts of the causal chain in these IAMs are the *valued environmental components* and the (physical) *consequences*. Consequently, in the following analysis, these two stages of the chain will get the most attention.

[1] This modification of Norberg-Bohm's classification was inspired by the preliminary draft "ULYSSES Research Protocols GeMID: Generic Model Iterative Dialogue for Urban Lifestyles And Sustainablity" dd 16/08/1996 that was distributed via the ULYSSES E-mail mailing list.

Causal chain	Culture and values	Demands for goods and services	Choice of technologies and practices	Flux of materials	Valued Environmental Components	Exposure	Consequences
Examples of variables	-Values -Attribution of responsibility -Culture -Risk perception -Ethical attitude -Driving value -Myth of nature -Preferences -Religion -Laws/Legislation	-Population size -GNP -Consumption per capita	-Energy efficiency -Land use -Life style -Share of coal, oil, natural gas, nuclear, renewables, biomass -Energy price	-Emissions of greenhouse gases (CO_2, CH_4, N_2O, CFCs etc.) -Emissions of ozone precursors (CO, NO_x, VOCs etc.) -Emissions of SO_2, NO_x, aerosols, soot.	-Greenhouse gas concentrations -Temperature -Precipitation -Soil moisture -Sea level -Storm frequency -River runoff -Tidal amplitude -Ocean circulation patterns	-Welfare -Healthiness/fitness of population -Sensitivity -Adaptability -Vulnerability -Damage thresholds of buildings and infrastructure for storms and floodings	-Agricultural production -Biodiversity -Storm damage -Flood damage -Migration patterns -Loss of property -Land loss -Health -Loss of water supply
Corresponding sub-system or sub-model	-Society -Culture -Values -Laws	-Economy -Population -Mobility -Agricultural demand -Values -Culture -Behaviour	-Energy economy -Transport economy -Industrial production -Households -Agriculture -Land use -Life-style	-Industrial emissions -Energy emissions -Land use emissions -Global Carbon Cycle -Other Global Biogeochemical Cycles -Atmospheric chemistry	-Terrestrial biosphere -Marine biosphere -Atmosphere Ocean Climate system	-Vulnerability assessment models -Values -Culture	-Impact assessment -Cost effectiveness -Cost benefit analysis -Values -Culture
Examples of competent disciplines	-Cultural theorists -Psychologists -(Moral) Philosophers -Sociologists -Historians -Jurists	-Demographers -Economists -Statistical agencies -Mathematicians -Psychologists	-Energy-experts -Economists -Engineers -SSST-ers (Social Studies of Science and Technology) -Cultural theorists -Psychologists -Philosophers -Sociologists	-Statistical agencies -Ecologists -Geo(bio)chemists -Biologists -Atmospheric chemists -Engineers	-Climatologists -Meteorologists -Oceanographists -Atmospheric chemists -Ecologists -Geo(bio)chemists -Biologists -Social psychologists	-Engineers -Hydrologists -Geographers -Biogeographers -Epidemiologists -Philosophers	-Economists -Hydrologists -Epidemiologists -Mathematicians -Psychologists -Sociologists -Cultural theorists
Key uncertainties	-Incomplete understanding -Indeterministic elements	-Demographic uncertainties -Behavioral uncertainties -Indeterministic elements	-Unpredictability of technological innovations -Incomplete understanding of implementation barriers -Indeterministic elements	-Incomplete information	-Incomplete understanding -Biogenic feedbacks -Chaotic behaviour -Multiple equilibria/Non-smooth behaviour -Modelability of surprise -Linkages with other anthropogenic environmental changes	-Uncertainty about vulnerability of future societies subjected to climate change	-Fundamental limits to predictability of future regional climate -Fundamental uncertainties in the attribution of monetary values -Multiple economic equilibria -Non-equilibrium

Table 5.2 Examples of variables, sub-systems, competent disciplines and key uncertainties in each stage of the causal chain.

5.4.1 Culture and Values

Culture and values are at the basis of the causal chain. The development of key variables such as risk perception, attribution of responsibility, life-attitudes (e.g. soberness), ethical attitude (ecocentrism or anthropocentrism), driving value (growth, equity, stability), myth of nature perceived-most-plausible (robust, fragile, or robust within bounds), laws and legislation, valuation of consequences for future generations, and other factors that can influence how many and what goods and services we demand and what life-style we develop, is poorly understood and to a certain extent open ended. Hence, the development of these variables is - at least partly - unpredictable. Therefore, differences in assumptions made about these variables causes differences between the outcomes of IAM-assessments.

5.4.2 Demands for goods and services

A given ensemble of culture and values that constitutes a human society gives rise to demands for goods and services, such as energy, transport, housing, heating, food etc. The resulting total demands are further a function of population size and composition, behaviour, life-style etc. The future behaviour of these entities is very hard to forecast reasonably reliably on time scales longer than roughly 10 years. IAMs for the climate issue usually use a 100-year time horizon. For instance, demographic uncertainties concern uncertainties about what factors trigger structural change in fertility behaviour, which still is poorly understood (Van Asselt, Beusen and Hilderink, 1996; Rotmans and De Vries, 1997). Keilman (forthcoming) observed that errors in the prediction of fertility are much higher than those in the prediction of mortality and that behaviorally determined variables are difficult to forecast. In comparisons of past population projections with the actual data, large errors have been found for both the young and the old after a forecast period of 15 years (up to +30 per cent for the age group 0-4, and 15 per cent or lower for women aged 85+ are not uncommon). According to Keilman, this suggests that those old forecasts supplied useful information for perhaps up to 10-15 years ahead, but certainly not longer. He also concludes that detailed studies for a few countries have found only modest systematic improvements in the accuracy of forecasting over time (if at all) when series of population forecasts produced by statistical agencies over a long period were analysed.

In the models, the processes that determine demands for goods and services are usually not represented by a sub-model. In stead, the uncertainties in this part of the causal chain are generally operationalized by designing a range of input scenarios covering the uncertainty ranges in the key variables.

5.4.3 Choice of technologies and practices

There is a number of constraints on the prediction of the future choice and availability of technologies and practices to fulfil demands for goods and services. A fundamental constraint here is the unpredictability of technological innovation. We cannot predict if and when an inexpensive clean energy technology will be available in the future. And even if we could, the availability of such a technology does not mean that it will be used. Several studies have shown that many potentially paying energy conservation measures can be taken in different sectors of our economic system, but that they are not taken because there are partly unknown or poorly understood barriers to their implementation (e.g. Worrell, 1994; Gillissen *et al.*, 1995; Velthuizen, 1995).

Gillissen *et al.* (1995) reviewed the potential barriers for the implementation of energy efficiency technologies. They notice that in a world without uncertainty about future states of events and cash flows, with free and full information, with independence between technologies, and with unlimited access to capital markets, a profit maximizing firm would implement all available technologies that have a positive net present value. However, the discrepancy between these premises and the real situation gives rise to implementation barriers. They arrived at the following categorization:

a. *knowledge barriers*: i) too few information channels; ii) low information processing capacity of smaller firms;

b: *economic barriers*: i) low expected energy prices; ii) uncertainty due to expected fluctuations in energy prices; iii) low revenues expected due to low energy bill; iv) budgetary problems; v) too high return of investment required; vi) bounded rationality;

c: *physical/technology barriers*: i) reduction in production quality; ii) lock-in effects (see also Arthur, 1988); iii) technological uncertainty about performance; iv) uncertainty about speed of technological development;

d: *management barriers*: i) no specialized personnel; ii) no interest in energy conservation by management; iii) no priority given to conservation (high opportunity costs); iv) bounded rationality.

With regard to other technology changes, such as a shift to renewable energy sources and decarbonation of fuels and flue-gases, there are similar implementation barriers (Turkenburg, 1995). The future behaviour of factors that bring about implementation barriers is largely open-ended and hence hardly predictable.

Large uncertainties are also associated with the estimates of future costs of technologies; these are based partly on informal guess work regarding the quantification of poorly known cost-factors, as we know from *inter alia* our own experience in this field (Van der Sluijs *et al.*, 1992).

A way of endogenizing some aspects of technological change and learning-by-doing effects in the models is the use of learning-curves to represent trends in autonomous and price induced energy efficiency improvements. The basic idea is that as individual technologies improve, conversion processes and end-use devices progress along their learning curves, and as inefficient technologies are retired in favour of more efficient ones, the amount of primary energy needed per unit of GDP - the energy intensity - decreases (WEC/IIASA, 1995). This approach is taken *inter alia* in the IIASA-WEC scenarios and in the energy sub-model (TIME) of IMAGE and Targets (WEC/IIASA, 1995; De Vries and Van den Wijngaart, 1995).

5.4.4 Fluxes of material in the environment

The estimation of emissions is the next step in the causal chain. For a given future energy demand and a technology mix to fulfil this demand, one needs to know the emission characteristics of the technologies and practices in order to determine the associated emissions. A problem here can be incomplete information about e.g. the emission characteristics of the technologies and lifestyles.

Much more difficult to estimate are the changes in fluxes of greenhouse gases related to land-use changes and changes in practices, especially with regard to the non-CO_2 greenhouse gases. There are already significant uncertainties in estimating current emissions of all greenhouse gases due to incomplete information. (e.g. Ebert and Karmali, 1992). The sources of CH_4 and N_2O are not well quantified (Schimel *et al.*, 1996 in IPCC'95) and emissions of substances such as CH_3CCl_3 and CO that affect atmospheric chemistry pathways and the life-time of greenhouse gases are not well quantified either. The uncertainties in sources and sinks of CO_2 are still large and still allow for the view that there is a missing (unidentified) carbon sink. For instance, the combined CO_2 fertilization, N-fertilization and other climate effects on global plant growth for the period 1980-1988 is estimated by IPCC'95 to be 1.3 ± 1.5 GtC/yr (estimated 90% confidence interval), indicating an error in the numeral that is of the same order of magnitude as the numeral itself, and indicating that even the sign of that flux is unknown as it can also be negative. Man's knowledge about atmospheric chemistry of the non-CO_2 greenhouse gases is incomplete. The estimates for the atmospheric lifetimes of non-CO_2 greenhouse gases are surrounded by huge uncertainties. For instance, the uncertainties in estimated life time amount to 300% for $CHCl_3$, 200% for CH_2Cl_2, 25% for CH_4 whereas the uncertainties in the lifetimes of the various CFCs range from 20% to 300% (all figures from Schimel *et al.*, 1996 in IPCC'95). As a consequence, the resulting future greenhouse gas concentrations calculated with Global Carbon Cycle models and Atmospheric Chemistry models are surrounded by significant uncertainties. These uncertainties however can be reduced when further research succeeds in quantifying sources and sinks more accurately and in unravelling the complexities of interrelated atmospheric chemistry pathways.

5.4.5 Valued Environmental Components

Changes in material fluxes lead to changes in Valued Environmental Components (VECs), which are those attributes of the environment which humans value. In general, we value those components not in themselves (although this would be debated by *inter alia* the deep ecologists), but because changes in them may lead to undesired consequences. The environmental components valued by human societies are apt to change with changes in culture, values, perception, and technology; all are unpredictable processes. As an implicit working hypothesis, the current IAMs assume that the set of currently valued environmental components will be the same as the set of components valued in the future. However, the current record falsifies the validity of this assumption: Looking over the currently available time series of successive climate risk assessments from the early-seventies until the present, many new VECs have shown up over time that were not valued before; these have emerged due to innovations in climate research and due to the increased social awareness of the climate change issue (Jäger *et al.*, to be published, see for the valuation of non-CO_2 greenhouse gases also chapter 3 of this thesis). Examples are the concentrations of newly discovered anthropogenic greenhouse gases such as CFCs (1975), N_2O (1976), CH_4 (1976) and SF_6 (1994); growing interest in storm-frequencies and intensities triggered by the dramatic increase in storm damage in the USA caused by super storms in the past decade; growing interest in local soil-moisture, due to the *'ensure that food production is not threatened'* objective in Article 2 of the FCCC; growing interest in ocean circulation patterns since the discovery of the targe scale thermo-haline circulation in the world ocean systems (also kown as the 'conveyor belt') and the recent notion that it might switch to another regime via climate-change-induced changes in the hydrological cycle.

The future states and temporal and spatial distribution of Valued Environmental Components such as temperature, precipitation, soil moisture, sea level, tidal amplitude, ocean circulation patterns, and storm intensity, frequency and duration, are evaluated using Earth System Models such as coupled Atmosphere Ocean General Circulation Models (GCMs, also known as: Global Climate Models), ocean circulation models, models of ice-dynamics and sea-level, and biosphere models.

The most advanced models used for the assessment of climate change are the GCMs. The simplified climate models used in IAMs are calibrated to GCM-results that are supposed to back them scientifically. The usefulness of GCMs (and hence of the simpler climate models in IAMs) for formulating policy advice has been debated. As two well-known modellers put it, the common wisdom is that feedbacks can "*be predicted credibly only by physically based models that include the essential dynamics and thermodynamics of the feedback processes. Such physically based models are the general circulation models*" (Schlesinger and Mitchell, 1987). The IPCC takes the same position on the usefulness of Atmosphere Ocean GCMs: *"... it is generally believed that it is only through such models that we can gain a scientific understanding (and hence a reliable predictive capability) of climate and climate change."* and *"This faith in the fundamental soundness of the modelling approach does not deny the presence of significant errors in current models nor the utility of models known to be incomplete, but does provide confidence that these errors can and will be reduced through continuing modelling research."* (Gates *et al.*, 1996 in IPCC'95)

These quotations from the IPCC'95 report clearly illustrate the dominant modelling paradigm of deterministic reductionism. The basic assumption behind this paradigm is that physical systems can be reliably described by deterministic mathematical equations, based on physical laws, provided only that sufficient detail can be included in the model to describe all the physical processes that are perceived to be important by the scientists involved, and provided that the initial state of all variables and parameters can be determined with sufficient exactitude. This paradigm leads to ever complexer models, demanding more and more computer power. It has also led to a still continuing record of claims that uncertainties will be reduced if model resolution is increased and if more physics and fewer parameterizations are included in the GCMs.

The modelling paradigm of deterministic reductionism has been criticized by *inter alia* Young and Parkinson (forthcoming). The main criticism formulated by Young and Parkinson is that physical-law-based models are more extensions of our mental models and perceptions of the real world than necessarily accurate representations of the world itself. Further they argue that deterministic reductionism is a useful paradigm in experimental physics and in other fields where one can do well-planned experiments under well-controlled laboratory conditions. However, for the description of the global environment where virtually all conditions are beyond our control, it is doomed to fail. They make a case for a new modelling approach that combines stochastic simulation models with data-based mechanistic models obtained from time series of observations. They demonstrated the feasibility of this alternative modelling approach by applying it to the Enting-Lassey Global Carbon Cycle Model which is used by the IPCC.

This coin has another side, because data-based mechanistic modelling might well exclude important counter-intuitive aspects of the dynamics of the modelled system that may show up only under novel conditions. Non-linear dynamics often result from the complex interplay of rules such as physical laws and other conceptual models. Data-based modelling might be the best tool for short-term extrapolation, whereas for longer-term forecasts, the rule-based models can add additional insight into possible counter-intuitive future behaviour of the modelled system. In that sense, we think that both modelling approaches (data-based modelling and rule-based modelling) are complementary in providing insights into possible future system behaviour.

Shackley *et al.* (forthcoming) criticized the widely believed superiority of GCMs relative to other climate modelling approaches. Their criticism can be summarized in three points: i) GCMs actually do not succeed in representing the complexity of interaction of primary processes (physical laws) which is commonly claimed or assumed (see also chapter 2 of this thesis); ii) there is no *a priori* reason to assume that the complexity of interaction and the degree to which model behaviour is governed by physical laws is proportional to the probability of a model being valid; iii) there are alternative understandings of model complexity and interaction.

Shackley *et al.*'s point (i) is further supported by a quote from Ann Henderson-Sellers, a prominent GCM modeller involved in several GCM inter-comparison projects and co-author of the chapter by Gates *et al.*, from which we took the previous IPCC quotes on the usefulness of GCMs. In an other scientific publication she says about GCMs: *"Today's climate models are essentially useless for virtually all forms of policy advice related to climate change. They are useful for some forms of short-term forecasting and medium range climate advice (e.g. El Niño projections, ...), but for long-term advice related to the enhanced greenhouse effect the value is minimal at best. The key conclusions of the models are driven by the assumptions and the various structures and devices used to simplify the calculations to make the models computable with today's technology. This massive problem is an important feature of the difficulty in linking the science and the policy."* (Henderson-Sellers, 1996a, p.43/44).

Henderson-Sellers touches two important points here which hold for essentially all Earth System Models. The first is the notion that the key conclusions of computer models are driven by assumptions and the modellers' "trick-box" to make the models computable with today's technology. The second point is that ESMs are not designed for answering policy questions, but rather for gaining insight into the modelled system. The key-word for gaining insight is simplification. According to Leo Schrattenholzer (IIASA), a model is a simplified representation of a system, where simplification is the goal and not the restriction (IIASA Seminar on "Comparing different philosophies or practical approaches to modelling", 9 July 1996). What happens now is that models designed for scientific purposes are used directly in integrated assessment models to address policy questions. It might well be that addressing policy questions requires differently designed models in which simplified, idealized, smoothed deterministic representation of nature is not adequate. In addition (not instead), models are needed that explicitly treat the uncertainties and non-smoothness.

We will discuss some key problems currently encountered in the practice of modelling the natural system. These are (i) incomplete understanding, (ii) biogenic feedbacks, (iii) chaotic behaviour, (iv) multiple equilibria and (v) linkages with other global environmental changes. Another important problem arises from the difficulties of regional and local projections of changes in VECs, which we will discuss in the section on consequences (5.4.7).

i. Incomplete understanding of the modelled system

In a recent ranking exercise involving 16 prominent climate modellers from the US, the top five sources of incomplete-understanding-uncertainty in climate modelling were identified from a list of 25 candidates (Morgan and Keith, 1995). These are:

- Cloud distribution and optical properties (including the effect of aerosols on clouds);
- Convection-water vapour feedback (all processes transport water vertically, except in the planetary boundary layer);
- Carbon dioxide exchange with terrestrial biota
- Carbon dioxide exchange with the oceans (including the ocean biota)
- Oceanic convection (e.g. high latitude production of deep water that is believed to drive the so-called "conveyer belt")

The participating experts expect a very significant reduction of uncertainty regarding climate sensitivity if complete understanding of these 5 elements is achieved. Uncertainties about non-CO_2 greenhouse gases were ranked low. It should be noted that Morgan and Keith's ranking exercise is biased in at least two fashions. First, only US climate experts were involved. Second, the experts involved are mainly climatologists, whereas we indicated in Table 5.2 that experts from other disciplines have competence to deal with these issues as well. We speculate that uncertainties associated with biogenic feedbacks and geochemical cycles other than the carbon cycle would rank higher if a more representative cross-cut of the competent disciplines had been selected for the ranking exercise.

However, Morgan and Keith's findings are consistent with MIT's claim that "*the most gain in reducing overall uncertainty in climate behaviour would be achieved by better understanding three processes: convection, cloud formation, and ocean circulation. They also conclude that progress in these fields will be slow during the next decade.*" (E-lab January-March 1995).

ii. Biogenic feedbacks

A number of feedback mechanisms, especially those in which the biota play a key role, are left out of the models because of a reluctance on the part of meteorologists and oceanographers to quantify these processes (Margulis and Lovelock, 1974; Leggett, 1990; Schneider, 1991; Turkenburg and van Wijk, 1991; *Ambio*, Febr. 1994). Until recently, very few attempts had been made to include biological processes in GCMs, beyond highly simplistic representations of the land-surface. Examples of biogenic mechanisms that might play a significant role in climate feedback loops are:

- The role of vegetation in surface properties (Melillo *et al.*, 1996, in IPCC'95);
- CO_2-induced reductions in stomatal conductance, resulting in lower evapotranspiration which affects both soil moisture and latent heat transport (Melillo *et al.*, 1996, in IPCC'95);
- The formation of biogenic substances that form a molecular top-layer upon ocean water which inhibits ocean evaporation (Personal communication with P. Westbroek, 1993);
- Enhanced aerobic respiration and large-scale oxidation by erosion and fire of high latitude peats (these peats are estimated to contain 450 GtC) caused by eventual drying and warming of these regions. This has also consequences for the CH_4 balance, because if the aerobic top layer of the soil becomes thicker by drying, its CH_4 uptake will increase. (Melillo *et al.*, 1996, in IPCC'95);
- The role of biota in the carbonate-silicate geochemical cycle: In a CO_2 rich world the

weathering rates of silicate might increase by enhanced vascular plant growth and enhanced CO_2 concentrations in soils, and by the intensification of the hydrological cycle via increased wash-out of carbonate. Carbonate is the product of the silicate weathering reaction, so increased wash-out will speed up the weathering rate. This feedback can be inferred from the model presented in chapter 4 of thesis. A possible consequence is that the carbon sink caused by silicate weathering will no longer be negligible for the shorter time scales in a CO_2 rich world. This hypothesis is supported by tentative unpublished work by Jan Goudriaan, who did experiments with a modification of his carbon cycle model that included the weathering equations from the Van der Sluijs *et al.* (1996) model of chapter 4 (personal communication with Jan Goudriaan, 1992);

- Albedo changes due to land-use change and vegetation changes;
- The influence of climate change on blooms of calcifying-algae such as Emiliania Huxley and the influence of these blooms on the albedo. These blooms form white clouds in the ocean surface water. (personal communication with P. Westbroek, 1990);
- The role of Di-Methyl Sulphide (DMS) emitted by marine algae in modulating cloud formation and cloud optical properties (Charlson *et al.*, 1987). The gas DMS oxidizes in the atmosphere to form sulphate aerosols. The sulphate aerosols act as condensation-nuclei for clouds. Given a same amount of water vapour available for cloud formation, increases in condensation nuclei concentration would create clouds with more (but smaller) droplets, and therefore a larger total reflection surface. The blooms of such algae and hence their DMS emission can depend on many different climate variables (not only temperature and direct sunlight, but also ocean-circulation and wind-stress for nutrient supply), making the climate and the algae a closely coupled system.

For a more comprehensive discussion of the biospheric modulation of the climate we refer to Margulis and Lovelock (1974), Lovelock (1988), Schneider (1991), and Westbroek (1991). For a recent review of biogenic feedbacks we refer to Woodwell and Mackenzie (1995).

More detailed investigation of biogenic climate-feedbacks requires the coupling of biosphere models to GCMs. This task is compounded because this means that the GCMs will have to resolve the vertical structure of the planetary boundary layer, which present GCMs don’t do. Further it requires a physiologically based representation of the processes controlling canopy conductance. As long as the biophysical key processes controlling soil moisture are not taken into account in GCMs, their simulations of soil moisture in a high CO_2 world are highly questionable (Melillo *et al.*, 1996 in IPCC’95). Further, given that the importance of these, and other feedbacks, is currently not known, but could conceivably be significant, the estimates of climate sensitivity using current GCMs might well be inaccurate.

iii. Chaotic behaviour

Chaotic behaviour usually refers to the phenomenon that very small changes in the system parameters or initial state can have a disproportionally large impact on the system behaviour of non-linear systems, which makes the system practically unpredictable. Since Lorenz, it has been widely believed that weather is chaotic, with a loss of coherence (for neighbouring initial conditions) in one or two weeks (Abarbanel *et al.*, 1991). This notion puts limits to the long-term predictability of weather and climate (see also Tennekes, 1992, 1994). The IPCC assumes that elements of the climate system are chaotic, while other elements are stable. *"The existence of these stable components allows prediction of global change despite the existence of the chaotic elements"* (Houghton *et al.*, 1990 p.80). IPCC calls their own assumption of smooth response of the climate system to forcing a *"reasonable working hypothesis, which receives some support from the smooth transient response simulated by coupled ocean atmosphere models."* (Houghton *et al.*, 1990, p.80).

The basic assumption behind the IPCC view is that there is a sharp distinction between fast elements of the atmosphere ocean systems and the slow elements (Hasselmann, 1976). The fast components are partly chaotic, but the slow ones are assumed to be non-chaotic. This view has been criticized *inter alia* by Abarbanel *et al.* (1991) on the grounds that such a strict separation between fast and slow elements is, in principle, invalid because the atmosphere ocean system should be treated as a coupled system. They claim that the real issue in the predictability of climate is whether the atmosphere-ocean systems constitute a chaotic dynamical system on all time scales, and provide evidence (but no proof) from a 134 year global mean temperature record that chaotic behaviour exists on all time scales.

Recent findings of the IGBP-PAGES (International Geosphere Biosphere Programme, PAst Global changES) also indicate that the climate system on long time scales is not as smooth as was assumed hitherto: *"High resolution records (from ice core and lake sediments) reveal rapid climate changes by several degrees within a decade or so"* (Lorius and Oeschger, 1994). This makes them conclude that *"global change science faces a new great question: can climate ever be predictable?"*

iv. Multiple equilibria/non-smooth behaviour

The issue of transitivity is another key uncertainty in modelling future states of valued environmental components. A transitive system is one which has only one equilibrium state; an intransitive system has at least two equally acceptable states. In an almost intransitive system, on the other hand, it is impossible to determine what is the 'normal state', *"since either of the two states can continue for a long period of time, to be followed by a quite rapid and unpredictable change to the other"* (Henderson-Sellers and Robinson, 1986).

The notion of multiple equilibria in the earth's climate is not new. Already in 1978 Oerlemans and Van den Dool showed, with a zonally averaged climate model of the energy balance and satellite measurements from that time, that using the actual solar constant, both the present climate and an ice-covered earth are stable solutions of the model. They investigated the effect of variation in the solar constant in detail, and found that if the solar-constant is decreased by 9-10% the warm solution jumps to the cold one. Transition from the cold to the warm solution requires an increase of the solar constant to 109-110 % of its present value. They conclude that our climate is more stable with respect to solar variations than was previously assumed, and that the model is more sensitive to changes in the greenhouse effect than to solar variations.

Clearly, the 'do-ability' of climate prediction depends on the system's transitivity. Yet, at present, geological and historical data are not detailed enough to determine which of these system types is typical for several sub-systems of the geosphere-biosphere system and the resulting coupled earth system. It is easy to see that should the climate turn out to be almost intransitive it will be extremely difficult to model (Henderson-Sellers and Robinson, 1986). Recently, Rahmstorf

(1995) showed intransitivity in a global ocean circulation model coupled to a simplified climate model: moderate changes in fresh-water input in the North Atlantic thermo-haline circulation (the so-called "conveyer belt", of which the Gulf Stream is a component) can induce transitions between different equilibrium states, leading to substantial changes in regional climate of several degrees on time scales of only a few years.

In the earlier mentioned survey among 16 prominent US climate experts, 14 of them gave as their expert judgement on the question *"Are there multiple stable climate states?"* the answer *"yes"*, one answered *"no"*, and one answered that he/she views the climate system as a non-equilibrium system wandering through phase space. The latter view is supported by our findings in chapter 4 of this thesis, where we speculate that the Pleistocenic glacial interglacial cycles, through which the current climate is believed still to be looping, might be understood as stable limit cycles in the long-term carbon cycle.

A closely related issue is non-smooth behaviour. Non-smoothness means that there are discontinuities (jumps) or discontinuities in the first derivate (sudden changes in trends) in the behaviour of a system over time. Non-smoothness introduces the problem that trends identified from the past trajectory are bad predictors for the future behaviour in the immediate environment of a discontinuity in the modelled phenomenon. The geological record of the earth system suggests the existence of non-smoothness in the natural system. We will discuss the question whether surprise can be modelled later in this thesis in chapter 6.

v. Linkages with other anthropogenic environmental changes

There are links between acidification and climate change. Sulphate aerosols affect the radiation balance by reflecting incoming solar radiation. They also have an indirect effect via their role in cloud formation (Tailor and Penner, 1994; Houghton *et al.*, 1994; 1995). A further link between acidification and climate change is nitrogen fertilization of the terrestrial biosphere (Melillo *et al.*, 1996 in IPCC'95). There also are links between stratospheric ozone depletion and climate change. The halocarbons that cause ozone depletion also are strong greenhouse gases. The substances that have been developed to replace CFC-11 and CFC-12 since the Montreal Protocol, are much less effective in depleting the ozone layer, but they still are strong greenhouse gases. Further, ozone is by itself a greenhouse gas. Another link is the fact that the enhanced greenhouse effect cools the stratosphere, leading to increased formation of stratospheric clouds that catalyse ozone depletion (Austin *et al.*, 1992). Finally, increased UV-B radiation (caused by ozone depletion) has an influence on the marine biota (Denman *et al.*, 1996 in IPCC-I, 1995).

There are several other global and local environmental changes going on that are not yet considered in the IAMs, although they do have indirect links with the climate system, the carbon cycle and the atmospheric chemistry of the other greenhouse gases. Examples are pollution of river-systems and oceans which might affect shelf sea and ocean biota, which in turn affect carbon fluxes in these systems. Further, changes in the geochemical cycles of phosphate and nitrogen might be significant by their eutrophication of continental shelf areas and also via the fertilization effects on the terrestrial eco-systems, which in turn affect the carbon cycle (e.g Denman *et al.*, 1996; Melillo *et al.*, 1996 both in IPCC'95).

All these linkages have to be considered in assessing the risks of climate change. The current climate IAMs consider only part of these links, meaning that potentially important interactions are ignored, which constitutes an additional limitation to the practical value of climate IAMs.

In summary, the Earth System Models used to evaluate future states of VECs encounter a number of limitations:

a) The set of environmental components being valued by human societies has changed over time and is likely to keep changing. It is unpredictable which environmental components will be valued by future societies;
b) the physical laws and other concepts on which ESMs are based can be viewed better as extensions of our mental models and perceptions of the real world than necessarily accurate representations of the world itself;
c) in the global environment, virtually all conditions are beyond our control. Consequently, a deterministic approach as chosen in IAMs might be doomed to fail;
d) GCMs are not as physics-based as is commonly claimed or supposed, key conclusions of GCMs and other ESMs are driven by assumptions and the modellers' "trick-box" to make the models computable with today's technology;
e) there is no *a priori* reason to assume that the complexity of interaction and the degree to which model behaviour is governed by physical laws is proportional to the probability of a model being valid;
f) there are alternative understandings of model complexity and interaction;
g) ESMs are not designed for answering policy questions, but rather for gaining insight into the modelled system. In addition, models that explicitly treat the uncertainties and non-smoothness are needed;
h) our understanding of a range of important climate processes is incomplete;
i) a range of biogenic-feedbacks is omitted in the models, because these feedbacks are not quantifiable with present-day knowledge;
j) chaotic behaviour of system components decreases the practical predictability of the climate, because we cannot know initial conditions and parameter values with sufficient precision;
k) the theoretical possibility of multiple equilibria and the resulting intransitivity compounds the task of prediction;
l) potentially significant linkages with other global environmental changes are not yet considered in the models.

5.4.6 Exposure

Exposure is the next step in the causal chain. The changes in valued environmental components are linked to consequences via different exposure pathways. In their contribution to the IPCC Second Assessment Report, IPCC Working Group II distinguishes between sensitivity, adaptability, and vulnerability (Watson *et al.*, 1996). Following their definitions: Sensitivity is the degree to which a system will respond to a change in climate conditions. Adaptability refers to the degree to which adjustments are possible in practices, processes or structures of systems, to projected or actual changes in climate. Adaptation can be spontaneous or planned and can be carried out in response to or in anticipation of changes in conditions. Vulnerability defines the extent to which climate change may damage or harm a system. It depends not only on a system's sensitivity, but also on its ability to adapt to new climatic conditions. For instance, the consequences of sea-level rise or changes in the distribution of malaria vectors or other climate-zone related diseases are highly determined by the sensitivity, adaptability, and vulnerability of the local systems. These are in turn affected by parameters such as welfare of a local society or fitness of a local population. An industrialized country that has enough money for a good coastal defence system will have less exposure to future sea level rise than a developing country that has no money for adequate coastal defence. Regarding increased storm intensities, it may well be that future building technology results in super-storm-resistant houses, so that future storm damage stays within proportions. Future breeding (or genetic engineering) of drought-resistant crops may change the consequences of droughts on agriculture. Outdoor agriculture might be rare in a hundred years from now; so the vulnerability of future food production to climate change might change dramatically.

A serious omission in current Integrated Assessment practice is that the question of what the future world on which the climate change will be imposed might look like has not yet been addressed (personal communication Thomas Schelling, July 1996). Instead, the modelled climate change is imposed upon the current world. Examples of this are the RIVM studies on the possible future distributions of malaria (Martens, Rotmans and Niessen, 1994; Janssen and Martens, 1995) and of schistosomiasis (Martens, 1995). These authors themselves recognize major shortcomings of this approach: *"The extent of an increase in malaria risk will be superimposed upon change in malaria transmission associated with socio-economic development and the (in)effectiveness of control measures."* (Martens, Rotmans and Niessen, 1994) and *"among others, two of the limitations of the present model version are the non-inclusion of the impacts of socio-economic developments and land-use changes on the occurrence of malaria."* (Janssen and Martens, 1995). Martens (1995) adds another important constraint, namely "*in this study, one specific health impact of an anthropogenic-induced climate change is being investigated separately, although in many instances interactions between the various health effects of a climate change are possible if not probable (e.g. synergism between infectious diseases and levels of undernutrition) and they may accumulate in vulnerable populations."* To this, we can add uncertainties about future methods of coping with malaria as a result of technological and medical innovations. It cannot be ruled out that in a hundred years' time malaria will have ceased to exist.

5.4.7 Consequences

The last link in the causal chain is formed by the consequences such as floodings, shifting climate zones, changes in agricultural production, extinction of species, changes in ecosystems, changes in migration patterns, storm damage, effects on water supply, etc. Just as we discussed above with regard to exposure, it is uncertain whether (or even: unlikely that) the set of consequences that is being valued to day will be the same in the future. Again, because of the fundamental unpredictability regarding what consequences will be valued by future societies, the implicit - but probably invalid - working hypothesis of current IAMs is that this set remains unchanged.

A more serious problem is that the prediction of the consequences of exposure to future changes in valued environmental components requires regional-specific information on future states of valued environmental components. However, regional prediction is not yet possible with current state-of-the-art of climate models, and the inherent predictability of climate diminishes as geographical scale is reduced, as can be seen from the following quotes from IPCC: *"Confidence is higher in the hemispheric-to-continental scale projections of coupled atmospheric ocean climate models than in the regional projections, where confidence remains low. There is more confidence in temperature projections than hydrological changes".* and *"Considering all models, at the* 10^4 *-* 10^6 km^2 *scale, temperature changes due to* CO_2 *doubling varied between +0.6°C and +7°C and precipitation changes varied between -35% and +50% of control run values, with a marked interregional variability. Thus, the inherent predictability of climate diminishes with reduction in geographical scale."* (Kattenberg *et al.*, 1996, in IPCC'95).

We further illustrate this massive problem with a few quotes from Henderson-Sellers (1996b): *"The urgent issue is the mismatch between the predictions of global climate change and the need for information on local to regional change, in order to develop adaptation strategies.*"; "*The dilemma facing policymakers is that scientists have considerable confidence in likely global climatic changes but virtually zero confidence in regional changes.*"; "*Unfortunately, climate models cannot yet deliver this type of regionally and locationally specific prediction and some aspects of current research even seem to indicate increased uncertainty.*"

Regional prediction of future soil moisture changes is essential for assessing the risks that climate change will bring to the food production system. This issue is topical in the light of Article 2 of the UNFCCC and the current climate negotiations on that convention. Several inter-comparison projects of GCMs show that the uncertainties in geographical distributions of future soil moisture are huge. In fact, these GCM inter-comparison exercises showed that the uncertainties were much higher than was assumed in the IPCC 1990 report (Henderson-Sellers, 1996b).

Impacts in economic terms are modelled by economic models and cost-benefit analyses. Such models are usually based on the concept of economic equilibrium. However, linked to the proof of economic equilibrium is the result that there are usually multiple equilibria (Sonneschein, 1974). Jaeger and Kasemir (1996) have argued that the existence of multiple equilibria casts severe doubts on the possibility of meaningful cost-benefit analysis concerning different climate policies. Also, the insight is growing that real economies are essentially in a far-from-equilibrium state (Giarini and Stahel, 1993). This adds to the perceived uncertainty since it makes our previously gained understanding of the dynamics of equilibrium economic systems inadequate.

Pearce *et al.* (1996) list four major sources of uncertainties in estimating the social costs of climate change: i) limited knowledge about regional and local impacts; ii) difficulties in measuring the economic value of impacts, even where the impacts are known (particularly for non-market impacts and impacts on developing countries); iii) difficulties in predicting future technological and socio-economic developments and iv) the possibility of catastrophic events and surprises.

It is generally perceived that consequences matter only in terms of what people value. A widely acknowledged limitation of utility theory and welfare theory is the inherent impossibility to rank and aggregate utility and preferences in an objective way and the impossibility to objectively attribute value to consequences. Still, working group III of IPCC has attached monetary values to the costs and benefits of human intervention in the system (Pearce *et al.*, 1996). The issue of monetary valuation using methods such as "willingness to pay" (WTP) and "value of a statistical life" (VOSL) is currently subject to vehement controversy. Monetary valuation has been criticized for its unfairness (the willingness to pay to prevent loss of a statistical life of one US citizen is much higher than the willingness to pay to prevent loss of a statistical life of one Bangladesh citizen, in other words, willingness to pay biases its conclusions in favour of projects that harm the poor); for the irrelevance of values based on preferences (willingness to pay cannot fully address the importance to human society of large scale ecosystem integrity); for the existence of better measures (e.g. indicators of ecological integrity); for its encouragement to exclude effects that are hard to measure; and for its distortion of non-market consequences to which it draws attention. For a more comprehensive discussion on the debate on monetary valuation and its uncertainties we refer to Adams (1995a, 1995b) and Toman (1997).

In conclusion, a major problem with climate IAMs is that our present-day knowledge and understanding of the modelled system of cause-effect chains and the feedbacks in between is incomplete and is characterized by large uncertainties and limits to predictability. In each stage of the causal chain there are both potentially reducible and probably irreducible uncertainties affecting the estimates of future states of key variables and the future behaviour of system constituents. The potentially reducible parts stem from incomplete information, incomplete understanding, lack of quality in data and model assumptions and disagreement between experts. The probably irreducible parts stem from ignorance, epistemological limits of science, indeterministic system elements, practical unpredictability of chaotic system components, limits to our ability to know and understand, limits to our ability to handle complexity, the 'unmodelability' of surprise, non-smooth phenomena, and from intransitive system components due to multiple equilibria.

A closely related problem is that the state of science that backs the mono-disciplinary sub-models differs across sub-models. This implies that given the present state of knowledge, climate IAMs consist of a mixture of elements covering the whole spectrum ranging from educated guesses to well-established knowledge. It is also uncertain to what extent IAMs are complete.

Toth (1995) signalled two other problems in uncertainty management faced by the IAM modellers' community. First, he points out the danger that the more detailed and the more specific IAMs become, the more unreliable the modelling results will be. Toth's notion was put forward earlier by Environmental Resources Limited (1985), who claim that there is an optimum in the complexity of models, because uncertainty in model structure decreases with complexity, whereas data error increases. The curve describing the total uncertainty as a function of complexity has an optimum, beyond which further complexification results in an increase of total error. Second, Toth notices that the diversity of links makes uncertainty analysis an increasingly difficult task in practice.

The implications of all these problems depend strongly on the application of the IAM. A policy-maker who uses the model as a tool to help choose policy options suitable for achieving a given goal definitely needs good insight into the reliability and scientific status of the outcomes. A scientist who uses the model to assess the relative importance of the uncertainties is less dependent on the overall reliability of the outcomes. The importance of uncertainty management is a function of the context of use and the objective of use of IAMs (compare Clark and Majone, 1985; Mermet and Hordijk, 1989; Beck *et al.*, 1996a, 1996b). In practice, IAMs are used with a mix of policy-oriented and knowledge-acquisition-oriented objectives and in a variety of contexts. We will discuss the uses of IAMs in the following sections.

5.5 The usefulness and use of IAMs for the climate issue

In this section, we investigate the possibilities and limitations of climate IAMs to guide and inform the policy process. In the first part of this section we sketch the range of opinion on the (policy) usefulness, obtained from the literature and from interviews with two modellers. Within the controversy on the policy-usefulness of climate IAMs, we seek to identify what applications have been agreed upon as valid. In the second part of this section we discuss the context in which IAMs are used.

5.5.1 The policy-usefulness of climate IAMs

In the previous section we found that climate IAMs are currently based on a mixture of knowledge which covers a wide spectrum ranging from educated guesses to well-established knowledge. The uncertainties are large at every stage of the causal chain, and many scientific puzzles still have to be solved. Precisely because of these circumstances, some members of the international climate community feel that we might not be ready to link different aspects of the climate change issue together. The concern is that given the uncertainties in each of the individual components, linking them together would multiply uncertainties. This concern is widely heard in the debate on integrated assessment. The expression used by Henderson-Sellers (1996a) is "uncertainties explode". Swart (1994a) observed a similar concern among the social science community: "*Because of the large gaps in knowledge in the social sciences, in a recent report by social scientists of the US National Research Council, a strong warning is given against too much emphasis on the development and application of integrative models, encompassing both natural sciences and the human dimensions of global change (Stern et al., 1992). Climatologists dispute the inclusion of impacts in integrated models. This would misleadingly suggest a deterministic linkage between causes, physical effects and socio-economic impacts.*" Swart adds that there is an increasing desire to integrate as many aspects of global change as possible and to do collaborative research, even if pertinent uncertainties continue to exist.

Leen Hordijk[1] (1994) regards it as dangerous that parts of the IMAGE model anticipate scientific developments, instead of running parallel to or lagging behind scientific developments. *"For the climate case, the uncertainties are more numerous, bigger and more complex than with acidification. This can make climate IAMs ineffective in the policy process. With the RAINS model the situation was different. Acidification was an acknowledged problem in a large part of Europe and the major scientific puzzles concerning trans-boundary transport had been solved. The effects were already visible. So, you had a good starting point. Good models did not yet exist, but they*

[1] In the eighties Leen Hordijk was project leader of the RAINS model at IIASA. In 1987 Hordijk moved to RIVM and from the sidelines became involved in the IMAGE project. Later Hordijk was appointed as a professor at Wageningen Agricultural University where he leads a research group on Environment and Climate (WIMEK). In 1992 he set up the IMAGE advisory board.

were being developed in parallel with the scientific developments."

Hordijk sees three different strategies for doing integrated modelling: *"One strategy is to be ahead of the scientific development, as was the IMAGE model. IMAGE 1 was used with a signalling function, and that worked. IMAGE 2 is getting more and more detailed, and then you have to be extremely careful not to be too far ahead of the scientific developments. A second strategy is to run parallel to scientific development. That is what RAINS did. The third strategy is to wait until science has matured and then develop an IAM. That is what happened in the US national acidification program (NAPAP). There, only after more than ten years of research, and that was done deliberately, they did start to construct an IAM. One of the directors of NAPAP absolutely disagreed with the way the RAINS model was realized and was used. He saw it as a mixing up of policy and research. The approach that he advocated was to wait until the science is complete, and only then do integrated modelling"* (Hordijk, 1994; see also Hordijk, 1995).

Joe Alcamo[1] is a proponent of the use of climate IAMs to inform the policy process. He argues: *"We have global agreements to act on climate change and other environmental problems. While there is a great uncertainty regarding our future, we have a certain responsibility to take our best scientific understanding and use that to develop reasonable policies. Our best understanding can be expressed in an IAM like IMAGE 2, and it can then be used to analyse policies. We recognize that our best current scientific knowledge may not be the best knowledge in the future."* (Alcamo, 1994b).

Alcamo thinks there is nothing wrong with an IAM being ahead of the scientific developments. He thinks it can stimulate discussion and speed up the process of model-improvement. Alcamo does not completely agree with Hordijks' claim that RAINS ran parallel to scientific development: he thinks that both RAINS and IMAGE 2 were ahead of the accepted science. He mentiones the inclusion of nitrogen-transport in the RAINS model as an example: *"when hydrologists and soil scientists told us that it was imperative to include nitrogen transport and deposition in RAINS, atmospheric scientists told us that these calculations "were not ready". Nevertheless, a Polish scientist (Jerzy Bartnicki) and I went ahead and built a simple European-scale model of nitrogen deposition in Europe, published it in a journal, and included it as a submodel in RAINS. In my opinion this action stimulated other researchers with a better model to give us the nitrogen calculations we needed in RAINS."* (E-mail message from Joe Alcamo to Jeroen van der Sluijs, 16-10-1996).

Alcamo's response to the concerns about the multiplication of uncertainties is: *"I argue that it is not always the case that uncertainty from one component propagates to the next. It is also possible that uncertainty is dampened from one component to the next. I can give you an example. In the literature, CO_2 emission estimates for the year 2100 have a range of a factor of 50. But if a global carbon cycle model is run with a factor of 50 difference in CO_2 emissions, it only produces a factor of 3 or 4 difference in atmospheric CO_2 concentrations. Furthermore, this factor of 3 or 4 variation in atmospheric concentration produces a difference of a factor of two or less in computed global temperature increase. So to a degree there is theoretical evidence that the global*

[1] Joe Alcamo was prominently involved in the development of both the RAINS model and the IMAGE 2 model. He used to be deputy project leader of the RAINS model at IIASA. From 1 January 1992 to 29 February 1996 he was project leader of IMAGE 2. Now, Alcamo is a professor at the Center for Environmental Systems Research at the University of Kassel.

system integrates the variations found in its different components, and can actually dampen out fluctuations in different parts of the global system. In the end, this dampening process allows us to build integrated, multi-component models without having them suffer from unacceptably high uncertainties." This view is however not widely accepted in the scientific community, because there is no *a priori* reason to assume that it would more likely for uncertainties to cancel out than to accumulate.

Parson (1994) stresses how useful IAMs can be for making rational informed social decisions, while he further claims that IAMs assist in the structuring of uncertainties: *"First, integrated assessment can help (indeed is necessary) to answer the broadest bounding question, how important is climate change. Second, IA can help assess potential responses to climate change, either with a benefit-cost framing that compares costs of responses to the impacts they prevent, or with a cost-effectiveness framing that assesses relative effectiveness and cost of different response measures to meet a specified target. Third, IA can provide a framework in which to structure present knowledge, providing several benefits. Perhaps the most important contribution is structuring of uncertainty and sensitivity: how well quantities and relationships are known, and how strongly valued outputs depend on them. Finally, integrated assessment can serve the longer-term goal of capacity building."* (Parson, 1994)

The usefulness of integrated modelling for assessing uncertainties and for guiding research is also stressed in the evaluation of the Dutch National Research Programme on Global Air Pollution and Climate Change (NRP): *"A second general issue is the role of this theme* [that is integration] *in guiding the research area, and the role of IMAGE in particular. IMAGE (through uncertainty assessment) can provide information on the relative importance of uncertainties on different areas which may be useful (used in conjunction with other information) in guiding the programme. However, issues like the scope for and research cost of uncertainty reduction and the policy-relevance of uncertainties (at different levels, national, international) need to be addressed. Notwithstanding this, the importance of IMAGE should not be overestimated."* (Science Policy Associates, Inc., 1995)

Having evaluated the state of the art of IAMs at an IIASA workshop, Toth (1995, p265) says on the subject of their usefulness *"If the building blocks are so shabby, is it worthwhile building integrated models at all? The answer is clearly yes, despite the present weaknesses of the models. The reason is that modelling forces us to reveal our assumptions and changing those assumptions shows how important they are with respect to the outcome."*

Morgan and Henrion (1990) also support integrated modelling: *"There are legitimate reasons for building large and complex models. Such models are justified when the details of the system are well understood* ***and*** *the inclusion of these details in the model is essential to the insight or answer that is sought."* Then, they notice that these criteria are not met for problems such as climatic change and conclude: *"Modelling any of these problems involves complex systems of coupled differential equations and large amounts of data to establish initial conditions. These models cannot be used to produce precise predictions like those of engineering design models. Rather they provide a vehicle for research on systems we do not yet fully understand."*. At present Morgan is deeply involved in a major climate IAM effort at The Center for Integrated Study of the Human Dimensions of Global Change at Carnegie Mellon University (NSF, 1996).

Hellström (1996) arrives at almost the same conclusion: *"The primary significance of models* *seems to be one of heuristics; once we dispense with the assumption that models are true depictors of the world 'out there', their value becomes that of guidance for researchers and policy-makers. They become more of a policy instrument useful for the furthering of a science-policy dialogue than traditional scientific artefacts."*

Janssen and Rotmans[1] (1995) say something similar about the usefulness of climate IAMs: "*The models are meant to have an interpretive and instructive value rather than being prediction or "truth" machines.*". To stress the latter point, Rotmans and De Vries (1996) have chosen "*Insights, no answers*" as the title for their book on IAMs.

In summary, the positions in the debate vary from "We are not ready to do integrated modelling, we have to wait until the science used in the model has the status of well-established knowledge" to "We have the responsibility to use our best scientific understanding to develop reasonable policies. Integrated modelling is the optimal way to combine our knowledge in such a way that we can evaluate the consequences of different policy scenarios, do cost-benefit framing or optimize cost effectiveness to reach a target." Apparently there is agreement that IAMs are not truth-machines and cannot predict the future, but rather they are heuristic tools. IAMs are capable of testing sensitivity, answering 'what if' questions (although each answer has to be followed by ", given the total set of assumptions of this model'), ranking uncertainties, ranking policy options, assessing the relative importance of uncertainties, identifying research priorities and providing insights that cannot easily be derived from the individual natural or social science component models that have been developed in the past.

Despite the fact that some experts maintain that we are not ready for integrated assessment, the models are being used at present to directly address policy questions, for instance by identifying 'safe emission corridors', which are presented to negotiators as answers rather than as insights. It is highly questionable whether such use is justifiable, unless all actors that deal with IAMs and IAM results are fully aware of their limitations and caveats. These circumstances imply an urgent need for uncertainty management, quality assurance, high standards of IAM practice, and a high awareness of the limitations of models.

5.5.2 The context of use of IAMs

Mermet and Hordijk (1989) have presented a framework that correlates the role of assessment models to different kinds of policy contexts in which they are used and to the level of use. Although they inferred the framework as a result of a debriefing exercise concerning the use of the RAINS model in international negotiations, the framework is applicable to assessment in general.

---

[1] Jan Rotmans from RIVM developed the IMAGE 1 model in the period 1986-1990; he was prominently involved in the development of the ESCAPE model for the Commission of European Communities, which was the precursor of IMAGE 2. Further, he used to be project leader of the TARGETS model at RIVM.

Role of model	User level	
	Individual	*Collective (joint use by all parties)*
Model as motor of the process	A party promotes the model as an active basis for its position	All parties agree to use the model as reference framework for the process
Model as a source of information	A party uses the model to complement the argumentation of its position	The model is considered by all parties as one source of information used in the process
Model indifferent because marginal or useless	A party is reluctant to move from the political to a more technical ground	The negotiation is so adversarial that "rational" analysis of the problem plays little role
Model undesirable	A party disagrees on the science or fights the model as a tactic in the negotiations politics	Prospects for the use of the models are terrible

Table 5.3 Types and levels of use of assessment models in the negotiation process (Mermet and Hordijk, 1989).

The position of models in integrated assessment is subject to debate. As we said before, in the second half of the eighties RIVM promoted integrated modelling as the optimal way of interfacing science with policy (Zoeteman, 1987). The many limitations of climate IAMs are one of the reasons why Kasemir *et al.* (1996) and Bailey *et al.* (1996) have recently redefined Integrated Environmental Assessment, incorporating a much wider coverage of activities than modelling only (see their definition in Table 5.1). They argue that *"IAM is not a complete IEA methodology. Integrated Assessment Modelling is an important activity within the boundaries of IEA but it is only part of the assessment, not the whole."* Parson (1996) makes a comparable case for broadening the toolbox with unconventional assessment methodologies. The Netherlands NRP also has a broader interpretation of integrated assessment. They define it as a process in which a cluster of activities aims at optimizing the use of scientific knowledge for policy purposes. The activities include specific research, integration, risk analysis, policy analysis, and dialogue with policy and society (NOP-MLK, 1994). These notions marginalize the role of IAMs in integrated assessment.

A realistic image of the potential role of IAMs and - more general the role of scientific expertise - in the policy-making process needs to take into account its limitations and intrinsic uncertainties and the notion that scientific data do not necessarily correspond intrinsically to expert interpretation and policy conclusions, because these are ’underdetermined’ by any scientific knowledge because of the repertoire of interpretive possibilities existing at each link in the argumentative chains (Van Eijndhoven and Groenewegen, 1991; see also chapters 2 and 3 of this thesis). This means that we have to go beyond the technocratic view in which IAMs and other forms of assessment can provide an optimum scientific foundation for the policy process, and is both sufficient and conclusive in determining the best abatement strategy for the climate problem. Such a technocratic position would correspond to the upper right corner of Table 5.3. On the other hand, the lower right corner of Table 5.3 ends at relativism in which scientific expertise is of no use in decision making. This position is not compatible with what present day science **can** do, namely provide rational (though tentative) inter-subjective guidance for the ranking of plausibility and validity of theories on ’the world out there’. The position that is most compatible with both the potential and the limitations of IAMs is in our view somewhere between ’the IAM serves as **one** source of information used in the process’ and ’the IAM serves as **the** reference framework for the process’.

5.6 Conclusions

In this chapter we have explored the possibilities and limitations of Integrated Assessment Models (IAMs) of Climate Change in relation to their mission to model the entire causal chain and to guide and inform the climate policy debate and the negotiations on the climate convention.

We conclude that:

i. Man’s knowledge and understanding of the modelled causal chain of climate change (see Table 5.2) is incomplete and characterized by large uncertainties and limits to predictability. At each stage of the causal chain there are both potentially reducible and probably irreducible uncertainties that affect the estimates of future states of key variables and the future behaviour of system constituents. The potentially reducible parts stem from incomplete information, incomplete understanding, lack of quality in data and model assumptions and disagreement between experts. The probably irreducible parts stem from ignorance, epistemological limits of science, indeterministic system elements, practical unpredictability of chaotic system components, limits to our ability to know and understand, limits to our ability to handle complexity, unmodelability of surprise, non-smooth phenomena, intransitive system components and multiple equilibria.

ii. The IAMs currently available do not really integrate the entire causal chain, nor do IAMs take dynamically into account all feedbacks and linkages between the different stages of the causal chain.

iii. The state of science that backs the (mono-disciplinary) sub-models of IAMs differs across sub-models. In other words, the current climate IAMs consist of a mixture of constituents which covers a wide spectrum ranging from educated guesses to well-established knowledge. Further, we know that the models are incomplete, but it is uncertain to what extent.

iv There is a controversy about the usefulness of IAMs for the assessment of climate change. The positions in the debate vary from "We are not ready to do integrated modelling, we must wait until all science used in the model has the status of well-established knowledge" to "We have the responsibility to use our best scientific understanding to develop reasonable policies. Integrated modelling is the best way of combining our knowledge in such a way that we can evaluate the consequences of different policy scenarios, do cost-benefit framing or optimize cost effectiveness to reach a target."

There is however agreement that IAMs are not truth-machines and cannot reliably predict the future, but are heuristic tools. IAMs are capable of testing sensitivity, of answering 'what if' questions (although each answer has to be followed by ", given the total set of assumptions of this model"), of ranking policy options, of assessing the relative importance of uncertainties, of identifying research priorities and of providing insights that cannot easily be derived from the individual natural or social science component models that have been developed in the past.

v Despite the fact that some experts maintain that we are not ready for integrated assessment, the models are being used at present to directly address policy questions. For instance, they are being used to identify 'safe emission corridors', which are presented to negotiators as answers rather than as insights. It is highly questionable whether such use is justifiable, unless all actors that deal with IAMs and IAM results are fully aware of the limitations and caveats of IAM assessments. These circumstances imply that there is an urgent need for uncertainty management, quality assurance, high standards of IAM practice, and a high awareness of the limitations of models.

5.7 Acknowledgments

An earlier version of this chapter was published as a part of an IIASA (International Institute for Applied Systems Analysis) working paper (Van der Sluijs, 1996). We thank Jill Jäger, Joanne Linnerooth-Bayer, Jerry Ravetz, Josee van Eijndhoven, Wim Turkenburg, Rob Swart, Joe Alcamo, Anne Stijkel and Stephen Healy for making useful comments on the IIASA working paper and/or on earlier versions of this chapter. We are grateful to Lauren Hale and Sheila McNab for linguistic and stylistic improvements.

5.8 References

H. Abarbanel, S. Koonin, H. Levine, G. MacDonald, and O. Rothaus, *Issues in Predictability*, JSR-90-320, JASON, The MITRE Corporation, McLean, VA, 1991.

J. Adams, *Risk*, UCL Press, London, 1995a.

J. Adams, *Cost Benefit Analysis: Part of the Problem, Not the Solution*, Green College Centre for Environmental Policy & Understanding, University College London, 1995b.

J. Alcamo, L. Hordijk, J. Kämäri, P. Kauppi, M. Pösch, and E. Runca, Integrated Analysis of Acidification in Europe, in: *Journal of Environmental Management*, **21**, 1986.

J. Alcamo, R. Shaw, and L. Hordijk (eds.), *The RAINS Model of Acidification: Science and Strategies in Europe*, Kluwer Academic Press, Dordrecht, Boston, 1990, 402 pp.

J. Alcamo (ed), IMAGE 2.0: Integrated Modeling of Global Climate Change, in: *Water, Air, and Soil Pollution*, **76**, Nos. 1/2, 1994a.

J. Alcamo, *personal communication* (interview) 21 November 1994b.

J. Alcamo, M. Krol, and R. Leemans, *Stabilizing Greenhouse Gases: Global and Regional Consequences, Results from the IMAGE 2.0 Model*, Prepared as background report for the Dutch delegation to the first session of the Conference of Parties to the UN Framework Convention on Climate Change Berlin, Germany 28 March - 4 April, 1995, RIVM, Bilthoven, 1995.

J. Alcamo and E. Kreileman, *The Global Climate System: Near Term Action for Long Term Protection*, RIVM, Bilthoven, 1996.

J. Alcamo, E. Kreileman and R. Leemans, Global Models meet Global Policy, *Global Environmental Change*, 1996, **6** (forthcoming).

W.B. Arthur, Competing Technologies: an overview in: G. Dosi, Ch. Freeman, R. Nelson, G. Silverberg, and L. Soete, *Technical Change and Economic Theory*, Pinter Publishers, London, 1988 p.590-607.

J. Austin, N. Butchart and K.P. Shine, Possibility of an Arctic Ozone Hole in a Doubled-CO_2 climate, *Nature*, **360**, 1992, p. 221.

P. Bailey, C. Gough, M. Chadwick, and G. McGranahan, *Methods for Integrated Environmental Assessment: Research Directions for the European Union*, Stockholm Environmental Institute, 1996.

M.B. Beck, J.R. Ravetz, L.A. Mulkey, and T.O. Barnwell, *On The Problem of Model Validation for Predictive Exposure Assessments*, 1996a.

M.B. Beck, L.A. Mulkey, T.O. Barnwell, and J.R. Ravetz, *Model Validation for Predictive Exposure Assessments*, EPA, 1996b.

J.P. Bruce, H. Lee and E.F. Haites (eds.), *Climate Change 1995 : Economic and Social Dimensions of Climate Change*, contribution of Working Group III to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996, 448 p.

R.J. Charlson, J.E. Lovelock, M.O. Andreae and S.G. Warren, Oceanic Phytoplankton, Atmospheric Sulphur, Cloud Albedo and Climate, *Nature*, **326**, 1987, 655-661.

W.C. Clark and G. Majone, The Critical Appraisal of Scientific Inquiries with Policy Implications, in: *Science Technology and Human Values*, **10** (3), 6-19, 1985.

H. de Boois and J. Rotmans, Overzichtsmodel van de CO_2-Problematiek, in: *Berichten uit het RIVM 1985*, Bilthoven, 1986.

K. Denman, E. Hofmann and H, Marchant, Marine Biotic Responses to Environmental Change and Feedbacks to Climate, in: J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

B. de Vries and R. van den Wijngaart, *The Targets/IMage Energy (TIME) 1.0 Model*, RIVM report no. 461502016, Bilthoven, 1995.

H. Dowlatabadi, Integrated Assessment Models of Climate Change, An Incomplete Overview, in: *Energy Policy*, **23** (4/5), pp 289-296, 1995

C. Ebert and A. Karmali, Uncertainties in Estimating Greenhouse Gas Emissions, Environment Working Paper No. 52, World Bank, April 1992, 44 pp.

Energy Modeling Forum, *Final First Round Study Design for EMF 14: Integrated Assessment of Climate Change*, Energy Modeling Forum, Stanford University, Stanford, 1995a.

Energy Modeling Forum, *Model Results*, Stanford University, Stanford, 1995b.

Energy Modeling Forum, *Second Round Study Design for EMF 14: Integrated Assessment of Climate Change*, Energy Modeling Forum, Stanford University, Stanford, 1996.

Energy Modeling Forum, *Second Round Scenario Results*, Energy Modeling Forum, Stanford University, Stanford, 1996b.

Environmental Resources Limited, *Handling Uncertainty in Environmental Impact Assessment*, Environmental Resources

Limited, London, 1985.

S.O. Funtowicz and J.R. Ravetz, Three Types of Risk Assessment and the Emergence of Post- Normal Science, in S. Krimsky and D. Golding (eds), *Social Theories of Risk*, Westport CT, Greenwood, 1992, 251-273.

W.L. Gates, A. Henderson-Sellers, G.J. de Boer, C.K. Folland, A. Kitoh, B.J. McAvaney, F. Semazzi, N. Smith, A.J. Weaver, and Q.-C. Zeng, Climate Models - Evaluation, in: J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

O. Giarini and W.R. Stahel, *The Limits to Certainty, Facing Risks in the New Service Economy*, 2nd revised edition, Kluwer Academic Publishers, Dordrecht, 1993.

M. Gillissen, H. Opschoor, J. Farla and K. Blok, *Energy Conservation and Investment Behaviour of firms*. Department of Environmental Economics, Free University Amsterdam, and Depertment of Science Technology and Society, Utrecht University (NRP Report no. 410 100 102), 1995.

J. Goudriaan, *Model, Schaal en Aggregatieniveau*, (Model, Scale and Aggregation Level) inaugural oration, 30 September 1993.

K. Hasselmann, Stochastic Climate Models, Part 1: Theory, *Tellus*, **28**, 1976, p. 473-485.

T. Hellström, The Science-Policy dialogue in transformation: model-uncertainty and environmental policy, in: *Science and Public Policy*, **23** (2), 1996, p.91-97.

A. Henderson-Sellers, Bridging the Climate Gaps, in: T.W. Giambelluca and A. Henderson-Sellers, *Climate Change: Developing Southern Hemisphere Perspectives*, John Wiley & Sons, New York, 1996a.

A. Henderson-Sellers, Climate Modeling, Uncertainty and Responses to Predictions of Change, in: *Mitigation and Adaptation Strategies for Global Change*, **1** (1), 1996b.

A. Henderson-Sellers and R. Robinson, *Contemporary Climatology*, Longman, Harlow, 1986.

L. Hordijk, *An Integrated Assessment Model for Acidification in Europe*, Ph.D. thesis, Free University of Amsterdam, 1991a, 270 pp.

L. Hordijk, "Use of the RAINS Model in Acid Rain Negotiations in Europe." *Environmental Science and Technology* **25** (4), 1991b, p. 596-602.

L. Hordijk (ed.), *Report International Review Meeting IMAGE 2.0 11-13 January 1993 Amsterdam*, NRP report 00-09, Bilthoven, 1993.

L. Hordijk, *personal communication* (interview), 15 November 1994.

L. Hordijk, Integrated Assessment Models as a basis for Air Pollution Negotiations, *Water Air and Soil Pollution*, **85**, 1995, p. 249-260.

J.T. Houghton, G.J. Jenkins, and J.J. Ephraums (eds.), *Climate Change, The IPCC Scientific Assessment*, Cambridge University Press, 1990, 365 pp.

J.T. Houghton, B.A. Callander and S.K. Varney (eds.), *Climate Change 1992 The Supplementary Report to the IPCC Scientific Assessment*, Cambridge University Press, 1992, 200 pp.

J.T. Houghton, L.G. Meira Filho, J. Bruce, Hoesung Lee, B.A. Callander, E. Haites, N. Harris, and K. Maskell (eds.), *Climate Change 1994 Radiative Forcing of Climate Change and An Evaluation of the IPCC IS92 Emission Scenarios*, Cambridge University Press, 1994, 339 pp.

J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

S.L.S. Jacoby and J.S. Kowalik, *Mathematical Modeling with Computers*, Prentice-Hall, Inc., Englewood Cliffs, N.J., 1980

C.C. Jaeger and B. Kasemir, Climatic Risks and Rational Actors, in: *Global Environmental Change*, 1996 (forthcoming).

J. Jäger (ed.), *Responding to climate change: Tools for Policy Development*, Stockholm Environmental Institute, Stockholm, 1990.

J. Jäger, J. Cavender Bares, N. Dickson, A. Fenech, P. Haas, F. Hampson, P. Hughes, A. Liberatore, E.A. Parson, V. Sokolov, F. Toth, C. Waterton, J.P. van der Sluijs, J.C.M. van Eijndhoven, and B. Wynne 1997. Risk Assessment, in: The Social Learning Group, *Learning to Manage Global Environmental Risks: A Comparative*

History of Social Responses to Climate Change, Ozone Depletion and Acid Rain. MIT Press, forthcoming (1997?).

M.A. Janssen and W.J.M. Martens, *Sex, Drugs and Climate Change: Modeling Malaria as a Complex Adaptive System*, Report no. GWP95-2, RIVM, Bilthoven, 1995.

M. Janssen and J. Rotmans, *The Evolution of Integrated Assessment Models for Global Climate Change: Towards a new Generation*, paper prepared for the Energy Modeling Forum Working Group Meeting at Stanford University, California, 24-26 May 1995.

B. Kasemir, C.C. Jaeger, and G. Dürrenberger, Embedding Integrated Assessment Models in Social Discourse, in: *Science and Public Policy,* **23** (2), 1996, p. 124-125.

A. Kattenberg, F. Giorgi, H. Grassl, G.A. Meehl, J.F.B. Mitchell, R.J. Stouffer, T. Tokioka, A.J. Weaver, and T.M.L. Wigley, Climate Models - Projections of Future Climate, in: J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

B. Keepin and B. Wynne, Technical Analysis of IIASA energy scenarios, in: *Nature*, **312**, 1984, p. 691-695.

N. Keilman, Ex-Post Errors in Official Population Forecasts in Industrialized Countries, *Journal of Official Statistics*, (accepted for publication; forthcoming).

F. Krause, *From warming fate to warming limit: benchmarks for a global climate convention*, El Cerrito, California, 1989.

F. Langeweg, *Zorgen voor Morgen, Nationale Milieuverkenningen 1985-2010*, Samson H.D. Tjeenk Willink, Alphen aan den Rijn, 1988.

J. Leggett, J. (eds.) *Global Warming; the Greenpeace Report*, Oxford University Press, Oxford, 1990.

C. Lorius and H. Oescher, Palaeo-perspectives: Reducing Uncertainties in Global Change?, *Ambio,* **23** (1), 1994, p. 30-36.

J. Lovelock, The Ages of Gaia, A Biography of Our Living Earth, Oxford University Press, Oxford, 1988, 252 pp.

L. Margulis and J.E. Lovelock, Biological Modulation of the Earth’s Atmosphere, in: *Icarus*, **21**, 1974, p. 471-489.

W.J.M. Martens, *Modeling the Effect of Global Warming on the Prevalence of Schistomomiasis,* Report no. 461502010, RIVM, Bilthoven, 1995.

W.J.M. Martens, J. Rotmans, and L.W. Niessen, *Climate Change and Malaria Risk,* Report no. 461502003, RIVM, Bilthoven, 1994.

J.M. Melillo, I.C. Prentice, G.D. Farquhar, E.-D. Schulze, and O.E. Sala, Terrestrial Biotic Responses to Environmental Change and Feedbacks to Climate, in: J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

L. Mermet and L. Hordijk, On Getting Simulation Models Used in International Negotiations: a Debriefing Exercise, in: F. Mautner-Markhof (ed), *Processes of International Negotiations*, Westview Press, Boulder CO, 205-237, 1989.

I.M. Mintzer, *A Matter of Degrees: The Potential for Controlling the Greenhouse Effect*, World Resources Institute, Washington D.C., 1987

M.G. Morgan and M. Henrion, *Uncertainty, A Guide to Dealing with Uncertainty in Quantitative Risk and Policy Analysis*, Cambridge University Press, 1990.

M.G. Morgan and D.W. Keith, Subjective Judgements by Climate Experts’, in: *Environmental Science and Technology*, **29**, 1995, p.468A-476A.

NOP-MLK, *Programming Memorandum - Second Phase 1995-2000*, Internal report II-4, Programmabureau NOP, RIVM, Bilthoven, 1994

V. Norberg-Bohm, W.C. Clark, M. Koehler, and J. Marrs, Comparing Environmental Hazards: The Development and Evaluation of a Method based on a Causal Taxonomy of Environmental Hazards. In: W.C. Clark (Ed.), *Usable Knowledge for Managing Global Climatic Change*, Stockholm Environmental Institute, 1990.

W.D. Nordhaus, *The Economics of the Greenhouse Effect*, paper presented at the June 1989 meeting of the International Energy Workshop, Laxenburg, 1989

W.D. Nordhaus, To Slow or not to Slow: The Economics of the Greenhouse Effect, in: *The Economic Journal*, **101**, 1990, pp 920-937.

NSF (U.S. National Science Foundation) *NSF Awards $17.8 Million to Study*
Human Dimensions of Global Change, press release NSF PR 96-40, August 6, 1996.

J. Oerlemans and H.M. van den Dool, Energy Balance Climate Models: Stability Experiments with a Refined Albedo and Updated Coefficients for Infrared Emission, in: *Journal of the atmospheric sciences*, **35**, 1978, p. 371-381

E.A. Parson, *Searching for Integrated Assessment: A Preliminary Investigation of Methods and Projects in the Integrated Assessment of Global Climatic Change*, Paper presented at the 3rd meeting of the CIESIN-Harvard Commission on Global Environmental Change Information Policy, NASA Headquarters, Washington DC, February 17-18, 1994.

E.A. Parson, Integrated Assessment and Environmental Policy Making, In Pursuit of Usefulness, in: *Energy Policy*, **23** (4/5), 1995, p. 463-476.

E.A. Parson, *How Should we Study Global Environmental Problems? A Plea for Unconventional Methods of Assessment and Synthesis*, 1996.

D.W. Pearce, W.R. Cline, A.N. Achanta, S. Frankhauser, R.K. Pachauri, R.S.J. Tol and P. Vellinga, The Social Costs of Climate Change: Greenhouse Damage and the Benefits of Control, in: *Climate Change 1995, Economic and Social Dimensions of Climate Change*, Contribution to Working Group III to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996, 183-224.

S. Rahmstorf, Bifurcations of the Atlantic Thermohaline Circulation in Response to Changes in the Hydrological Cycle, *Nature*, **378**, 1995, p. 145-149.

J. Ravetz, Usable Knowledge, Usable Ignorance: Incomplete Science with Policy Implications, in: W.C. Clark (ed.), *Sustainable Development of the Biosphere*, Cambridge University Press, New York, 1986, p.415-432.

F.R. Rijsberman and R.J. Swart, *Targets and Indicators of Climatic Change*, Stockholm Environmental Institute, Stockholm, 1990.

J.S. Risbey and P.H. Stone, Sununu's Optimism, *Technology Review*, October 1992, p. 9 and 79.

J. Rotmans, *IMAGE An Integrated Model to Assess the Greenhouse Effect*, (Thesis), Rijksuniversiteit Limburg, 1990, 295 pp.

J. Rotmans, *personal communication* (interview), 9 November 1994.

J. Rotmans and H. Dowlatabadi, Integrated Assessment of Climate Change: Evaluation of Methods and Strategies, in: S. Rayner and J. Emonds (eds.) State of the Art report. (referred to in Van Asselt and Rotmans, 1995)

J. Rotmans and H.J.M. de Vries (ed.), *Insights, no Answers*, Cambridge University Press, Cambridge, 1996.

D. Schimel, D. Alves, I. Enting, M. Heimann, F. Joos, D. Raynaud, T. Wigley, M. Prather, R. Derwent, D. Ehhalt, P. Fraser, E. Sanhueza, X. Zhou, P. Jonas, R. Charlson, H. Rodhe, S. Sadasivan, K.P. Shine, Y. Fouquart, V. Ramaswamy, S. Solomon, J. Srinivasan, D. Albritton, R. Derwent, I. Isaksen, M.Lal, and D. Wuebbles, Radiative Forcing of Climate Change, in: J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

D. Schimmelpfennig, Uncertainty in Economic Models of Climate-Change Impacts, in: *Climatic Change*, **33** (2), 1996, p. 213-234.

M. Schlesinger and J. Mitchell, 'Climate Model Simulations of the Equilibrium Climate Response to Increased Carbon Dioxide', *Review of Geophysics*, **25**, 1987, 760-98.

S.H. Schneider (ed.), *Scientists on Gaia*, MIT Press, Cambridge, 1991, 433 pp.

U. Schotterer and H. Oeschger, The Stability of the Climate System in the light of Recent Ice Core Measurements, in: N. Nakićenović, W.D. Nordhaus, R. Richels, and F.L. Toth (eds.) *Integrative Assessment of Mitigation, Impacts, and Adaptation to Climate Change*, CP-94-9, International Institute for Applied Systems Analysis, Laxenburg, 1994.

Science and Policy Associates, inc. and Holland Consulting Group, *Evaluation of the Technical Emphasis, Policy Relevance and Management Performance of the Dutch National Research Program on Global Air Pollution and Climate Change, Executive Summary*, Science and Policy Associates, inc., Washington DC and Holland Consulting Group, Amsterdam, October 1992.

Science and Policy Associates, inc., *Dutch National Research Programme On Global Air Pollution and Climate Change, NRP Phase I Final Review Report*, Science and Policy Associates, inc., Washington D.C., March 1995.

H. Sonnenschein, Market Excess Demand Functions, in: *Econometrica*, 40, 1974, p. 549-562.

P.C. Stern, O.R. Young and D. Druckman (eds.), *Global Environmental Change: Understanding the Human Dimensions, National Research Council*, National Academy Press, Washington, 1992 (cited in Swart, 1994a).

R. Swart, *Climate Change: Managing the Risks*, (Thesis), Free University Amsterdam, 1994a.

R. Swart, *personal communication* (interview), 23 November 1994b.

R. Swart, M. Berk, E. Kreileman, M. Janssen, J. Bollen, R. Leemans, and B. de Vries, *The Safe Landing Approach: Risks and Trade-offs in Climate Change*, RIVM, Bilthoven, 1996.

K.E. Taylor and J.E. Penner, Response of the Climate System to Atmospheric Aerosols and Greenhouse Gases, *Nature*, **369**, 1994, p. 734-737.

Technology Review, The Political Pleasures of Engineering: An Interview with John Sununu, *Technology Review*, August/September 1992, p. 22-28.

H. Tennekes, Karl Popper and the Accountability of Numerical Weather Forecasting, *Weather*, **47** (9), 1992, p. 343-346.

H. Tennekes, The Limits of Science, The Conversion of a Meteorologist to Ecology, in: Zweers and Boersma (eds.), Ecology, Technology and Culture, White Horse Press, UK, 1994.

M.A. Toman, Ecosystem Valuation: An Overview of Issues and Uncertainties, in: R.D. Simpson, N.L. Christensen jr., *Ecosystem Function & Human Activities, Reconciling Economics and Ecology*, Chapman & Hall, New York, 1997, p. 25-44.

F.L. Toth, Practice and Progress in Integrated Assessments of Climate Change, A Workshop Overview, in: *Energy Policy*, **23** (4/5), 1995, p. 253-267.

W.C. Turkenburg (ed.), Assessment Report on NRP Subtheme "Energy Demand and Supply Options to Mitigate Greenhouse Gas Emissions", in: S. Zwerver, R.S.A.R. van Rompaey, M.T.J. Kok, and M.M. Berk (eds.), *Climate Change Research; Evaluation and Policy Implications*, Studies in Environmental Science 65 B, Elsevier Science B.V., 1995.

W.C. Turkenburg and A.J.M. Van Wijk, *Onze Kennis over Klimaatverandering, Verslag van een Workshop*, Report Lucht 99, DOP (Depot Overheids Publicaties), Leiden, 1991.

M.B.A. van Asselt, A.H.W. Beusen, and H.B.M. Hilderink, Uncertainty in Integrated Assessment, A Social Scientific Perspective, in: *Environmental Modeling and Assessment*, **1** (1,2), 1996, 71-98.

J.P. van der Sluijs, Uncertainty Management in Integrated Modeling, the IMAGE case, in: S. Zwerver, R.S.A.R. van Rompaey, M.T.J. Kok, and M.M. Berk (eds.), *Climate Change Research; Evaluation and Policy Implications*, Studies in Environmental Science 65 B, Elsevier Science B.V., 1995.

J.P. van der Sluijs, C.A. Hendriks, K. Blok, "Feasibility of polymer membranes for carbon dioxide recovery from flue gases", In: *Energy Conversion and Management*, **33**, 429-436 (1992) (Proc. First Int. Conf. on Carbon Dioxide Removal).

J.P. Van der Sluijs, *Integrated Assessment Models and the Management of Uncertainties*, IIASA Working Paper no. WP-96-119, Laxenburg, October 1996, 80 pp.

J.C.M. van Eijndhoven and P. Groenewegen, The Construction of Expert Advice on Health Risks, *Social Studies of Science*, **21**, 1991, p. 257-278.

J.W. Velthuizen, *Determinants of Investment in Energy Conservation*, Ph.D. Thesis, University of Groningen, 1995, 349 pp.

R.T. Watson, M.C. Zinyowera, R.H. Moss, *Climate Change 1995: Impacts, Adaptations and Mitigation of Climate Change: Scientific-Technical Analyses*, contribution of Working Group II to the second assessment report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996, 878 pp.

WEC and IIASA, *Global Energy Perspectives to 2050 and Beyond*, World Energy Council, London, 1995.

P. Westbroek, *Life as a Geological Force, Dynamics of the Earth*, W.W. Norton & Compagny, New York, 1991, 240 p.

J.P. Weyant, *Integrated Assessment of Climate Change: A Overview and Comparison of Modeling Approaches*, paper for the writing team 6/7 of working III Intergovernmental Panel on Climate Change Lead Authors Meeting, Geneva, September 8-10 1994.

J. Weyant, O. Davidson, H. Dowlatabadi, J. Edmonds, M. Grubb, E.A. Parson, R. Richels, J. Rotmans, P.R. Shukla, R.S.J. Tol, W. Cline, and S. Frankhauser, Integrated Assessment of Climate Change: An Overview and Comparison of Approaches and Results, in: *Climate Change 1995, Economic and Social Dimensions of Climate Change*, Contribution to Working Group III to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996, 183-224.

G.M. Woodwell and F.T. MacKenzie, Biotic Feedbacks in the Global Climate System, Oxford University Press, New York, 1995, 416 pp.

E. Worrell, Potentials for Improved Use of Industrial Energy and Materials, Ph.D. Thesis, Utrecht University, 1994, 183 pp.

P. Young and S. Parkinson, Simplicity out of Complexity in Forecasting Environmental Change, in: B. Beck (ed.) *Forecasting Environmental Change* (forthcoming monograph).

B.C.J. Zoeteman, Verslag van de Directeur van Hoofdsector III: Chemie en Fysica, in: *Berichten uit het RIVM 1986*, Bilthoven, 1987.

APPENDIX

List of Integrated Assessment Models for the climate issue

Model	Full name	Source
AIM	Asian-Pacific Integrated Model	I;D;W;E
AS/ExM	Adaptive Strategies/Exploratory Model	I
ASF (EPA)	Atmospheric Stabilization Framework	
CETA	Carbon Emission Trajectory Assessment	I;D;W;E
Connecticut	(model by G. Yohe)	I;E
CRAPS	Climate Research And Policy Synthesis model	I;E
CSERGE	Centre for Social and Economic Research into the Global Environment	I;W;E
DGEM		D
DIALOOG	(Model developped by KEMA for a dialogue with the Netherlands electricity sector)	R
DIAM	Dynamics of Inertia and Adaptability Model	I
DICE	Dynamic Integrated Climate Economy model	I;D;W;E
Edmonds-Reilly-Barns		D
FUND	The Climate Framework for Uncertainty, Negotiation and Distribution	I;E
GCAM	Global Change Assessment Model	W
GEMINI		D
GLOBAL 2100		D
ICAM-2	Integrated Climate Assessment Model	I;D;W;E
IIASA	International Institute for Applied Systems Analysis	I;E
IMAGE 1	Integrated Model to Assess the Greenhouse Effect	D
IMAGE 2	Integrated Model to Assess the Greenhouse Effect	I;D;W;E
ISM	Integrated Science Model for assessment of climate change	W

MAGICC	Model for the Assessment of Greenhouse gas Induced Climate Change	W
MARIA	Multiregional Approach for Resource and Industry Allocation	I;E
MARKAL	Market Allocation model	D
MBIS	Mackenzie Basin Impact Study	D
MCW (WRI)	Model of Global Warming Commitment	
MERGE 2	Model for Evaluating Regional and Global Effects of GHG Reduction Policies	I;W;D;E
MIT	Massachusetts Institute of Technology	I;W;E
MiniCAM *	Mini Climate Assessment Model	I;W
MORI	(Also known as MARA of DICE+e)	E
New EARTH 21		W
OECD-GREEN		D
PAGE	Policy Analysis of the Greenhouse Effect	I;D;W
PEF	Policy Evaluation Framework	I;D;W
PoleStar	(Stockholm Environmental Institute)	
ProCAM	Process Oriented Global Change Assessment Model	I;D
RAND		E
RICE	Regional DICE	I
SLICE	Stochastic Learning Integrated Climate Economy Model	I
TARGETS	Tool to Assess Regional and Global Environmental and Health Targets for Sustainability	I;D;W

Explanation of the codes in the third column: D = Mentioned in the overview by Dowlatabadi (1995); W = Mentioned in the overview by Weyant (1994); E = Part of the comparison by the Energy Modeling Forum (1995b, 1996b); I = Mentioned in the overview by IPCC WG3 (Weyant *et al.*, 1996); R = Personal communication, Walter Ruigrok, KEMA (17 Sept. 1996).

Chapter 6

Integrated Assessment Models of Climate Change and the Management of Uncertainties

6.1 Introduction
6.2 Classifications of uncertainty
6.3 Addressing uncertainty due to inexactness
6.3.1 Uncertainties in input data and model parameters
6.3.2 Uncertainties regarding conceptual model structure and technical model structure
6.3.3 Uncertainties regarding model completeness
6.4 Addressing unreliability: quality control in IAM practice
6.5 Addressing ignorance
6.5.1 Reducing ignorance by research, a paradox
6.5.2 The modelling of surprise
6.6 Areas for improvement in uncertainty management
6.7 Disentangling the uncertainty problem: adding the quality dimension
6.8 Conclusions
6.9 Acknowledgements
6.10 References

Abstract

We analyse the problems in uncertainty management in Integrated Assessment Models of climate change (IAMs). Using a classification scheme comprising type and source of uncertainty we identify the major gaps in current practice. We found that: (1) not all relevant aspects of uncertainty are addressed; (2) sufficient insight into quality, uncertainties and limitations is missing; (3) the subjective component in the appraisal of uncertainties is not systematically addressed. The main areas for improvement are the assessment of error in model output due to unreliability of the knowledge about input data, parameters, and model structure, and the quantitative assessment of error in model output due to uncertainty in conceptual and technical model structure. We propose a NUSAP (Numeral Unit Spread Assessment Pedigree)-informed Delphic ranking procedure to disentangle the uncertainty problem in IAMs. This method makes it possible to discriminate between the potentially solvable and the currently unsolvable uncertainties. This information is crucial for the development of adequate response strategies.

6.1 Introduction

Integrated Assessment Models of climate change (hereafter referred to as *IAMs*) are playing an increasingly important role in decision making. This role requires good insight into the quality of these models. This chapter explores the problems of uncertainty management and quality control in climate IAMs. According to our definition, an IAM is a mathematical representation of a natural system coupled to a socio-economic system, modelling the *cause-effect chain* of anthropogenic climate change, including feedback loops, and explicitly designed to serve as a tool to analyse the potential impact and consequences of policies in order to guide and inform the policy process, mainly by means of scenario analysis.

In the chapter 5 of this thesis, we concluded that man's knowledge and understanding of the causal chain of climate change modelled in climate IAMs is incomplete and is characterized by large uncertainties and limitations to its use for guiding the policy process. Some of the uncertainties can be reduced, others are intrinsic to the modelled system or to the technique of modelling. Further, we showed that the current climate IAMs consist of a mixture of constituents covering a wide spectrum ranging from educated guesses to well-established knowledge, whereas the models are also incomplete.

We have seen that, despite the fact that some experts maintain that we are not ready for integrated assessment modelling of anthropogenic climate change, the models are being used at present to directly address policy questions. For instance, the IMAGE (Integrated Model to Assess the Greenhouse Effect) model 1996) is being used to calculate 'safe emission corridors'. The results are presented to negotiators on the Framework Convention on Climate Change as answers rather than as insights. We argued that it is highly questionable whether such use is justifiable, unless all actors that deal with IAMs and IAM results are fully aware of their limitations and caveats. These circumstances imply that there is an urgent need for uncertainty management, quality assurance, and high standards of IAM practice. Therefore, we need insight into uncertainties and their management in IAM practice.

If uncertainties are to be managed better, a first step is to disentangle the uncertainty problem in such a way that we can identify the reducible uncertainties and the irreducible uncertainties in the model. This is helpful for the setting of research priorities to reduce uncertainties in individual model constituents. Further, such a distinction enables the development of adequate response strategies that take the irreducible uncertainties into account.

In this chapter we seek answers to the following questions:

i) What are the types and sources of uncertainties in climate IAMs?
ii) What are the main areas of improvement in uncertainty management in IAMs?
iii) How can we distinguish between reducible uncertainty and irreducible uncertainty?
iv) How can we attribute part of the overall potential of improvement in a model to lack of quality in its individual constituents?

An answer to the latter question would enable us to identify those constituents whose individual potentially reducible uncertainty contributes most to the overall lack of quality in the model outcome.

On the basis of literature study and interviews with the model builders, we have made an inventory of the way in which questions of uncertainty and quality are being addressed and can be addressed in IAMs. Our main focus is on the IMAGE model (Integrated Model to Assess the Greenhouse Effect, see Box 6.1), which serves as a case study. The first version of IMAGE was developed in the period 1985-1990 (De Boois and Rotmans, 1986; Rotmans 1990). In this period climate change started to be signalled as a policy issue. IMAGE was even used to put the issue on the policy agenda and the model was developed despite initial lack of interest from policy-makers in such a model (Rotmans, 1994; Swart, 1994b). IMAGE has been designed as a deterministic model. The treatment of uncertainty was not considered explicitly in the design of the model. One reason is that the issue of uncertainty management was less urgent at the time the model was developed than it is nowadays. Another reason is that IMAGE was one of the very first attempts to build an integrated model of climate change, and in such a situation it is wise to start with the simple deterministic case. Despite these circumstances, sensitivity analyses and uncertainty analyses were carried out from the very beginning.

IMAGE plays a leading role in process-oriented (as opposed to macro-economic, see also section 5.3.1 in chapter 5 of this thesis) integrated modelling of climate change. According to an independent evaluation of the Dutch NRP (National Research Programme on Global Air Pollution and Climate Change), *"IMAGE is an outstanding undertaking, the first attempt to produce a comprehensive integrated assessment framework for the climate change problem and still at the forefront of the field internationally."* and *"IMAGE has established a niche as a world leader in integrated systems modelling."* (Science and Policy Associates, Inc., 1995).

In the following, we analyse the mismatch between the types and sources of uncertainty that should be addressed in uncertainty management on the one hand and the types and sources of uncertainty addressed in current practice of uncertainty management in IAMs on the other hand. We will also look what methodologies are available to address different types and sources of uncertainty in models. Furthermore, we look at the reasons for the mismatch and identify areas in which the uncertainty management needs to be improved. In the final section, we discuss the question of how to proceed in uncertainty management, given the current problems. Building further on the work by Funtowicz and Ravetz (1990), we propose a method for disentangling the uncertainty problem in complex Integrated Assessment Models.

6.2 Classifications of uncertainty

There are many different kinds of uncertainties and there is no single method to manage all of them. Therefore we first need to seek a consistent way to classify uncertainties. In the literature a variety of classifications of uncertainty can be found which we have summarized in Table 6.1.

Table 6.1. Classifications of uncertainty

Vesely and Rasmuson, 1984	1. Data uncertainties (arise from the quality or appropriateness of the data used as inputs to models); 2. Modelling uncertainties: a. incomplete understanding of the modelled phenomena; b. numeral approximations used in mathematical representation; 3. Completeness uncertainties (all omissions due to lack of knowledge).
Environmental Resources Limited, 1985	Errors in modelling: a. process error (due to model simplification); b. functional error (uncertainty about the nature of the functional relations); c. resolution error; d. numerical error.
Hall, 1985	1. Process uncertainty; 2. Model uncertainty; 3. Statistical uncertainty; 4. Forcing uncertainty (involved in predictions which presuppose values that are unknowable).
Alcamo and Bartnicki, 1987	Type of uncertainty: 1. Model structure; 2. Parameters; 3. Forcing functions; 4. Initial state; 5. Model operation. For each type subdivided into: a. Diagnostic; b. Prognostic.
Beck, 1987	1. Uncertainty in internal description of the system: - errors of aggregation (temporal, spatial, ecological); - numerical errors of solution; - errors of model structure; - errors in parameter and state estimation; 2. Uncertainty in external description of the system: - Uncertainty (natural variability) due to unobserved system input disturbances; - Measurement errors; 3. Uncertainty in initial state of the system; 4. Propagation of state and parameter errors.

Morgan and Henrion, 1990	1. Sources of uncertainty in empirical quantities; a. Statistical variation and random error; b. Subjective judgement and systematic error; c. Linguistic imprecision; d. Variability; e. Inherent randomness and unpredictability; f. Disagreement; g. Approximation; 2. Uncertainty about model form.
Funtowicz and Ravetz, 1990	1. Inexactness (significant digits/error bars); 2. Unreliability; 3. Border with ignorance.
Wallsten, 1990	1. Ambiguity (confusion in communication, avoidable); 2. Vagueness (imprecision in meaning); 3. Precise uncertainties (objective and subjective probability);
Wynne, 1992	1. Risk (know the odds); 2. Uncertainty (don't know the odds); 3. Ignorance (don't know what we don't know); 4. Indeterminacy (open-ended causal chains or networks).
Helton, 1994	1. Stochastic uncertainty (arises because the system under study can behave in many different ways. It is a property of the system.); 2. Subjective uncertainty (arises from a lack of knowledge about the system. It is a property of the analysts performing the study.).
Hoffman and Hammonds, 1994	1. Uncertainty due to lack of knowledge; 2. Uncertainty due to variability.
Rowe, 1994	1. Four dimensions of uncertainty: a. Temporal (uncertainty in future states/ past states); b. Structural (uncertainty due to complexity); c. Metrical (uncertainty in measurement); d. Translational (uncertainty in explaining uncertain results); 2. Variability is a contributor to uncertainty in all dimensions. Sources of variability: a. Underlying variants - inherent to nature - that contribute to the spread of parameter values: i. apparent inherent randomness of nature; ii. inconsistent human behaviour; iii. nonlinear dynamic systems (chaotic) behaviour; b. Collective / individual membership assignment; c. Value diversity.

On the basis of Vesely and Rasmuson's (1984) classification of sources of uncertainty and Funtowicz and Ravetz' (1990) classification of types of uncertainty, Van der Sluijs (1995) has proposed a two-dimensional classification scheme with the dimensions *type* and *source*, which we will use here as a starting point.

Uncertainty can be classified according to its *source* as:

1) uncertainty in *input data*;
2) uncertainties in:
   a) *conceptual model structure*;
   b) *technical model structure*;
3) uncertainty about *model completeness*.

Uncertainty in input data arise from the quality or appropriateness of the data used as inputs to models. Uncertainties in conceptual model structure arise from lack of understanding of the modelled system. Uncertainties in technical model structure arise from simplifications, and errors in software and hardware. Uncertainty about *model completeness*, covers all omissions due to lack of knowledge.

According to its *type*, uncertainty can be classified as:

i) *inexactness*;
ii) *unreliability*;
iii) *ignorance.*

Inexactness refers to significant error-bars, probability distribution functions, multiple tenable model structures etc. Unreliability refers to the level of confidence, quality, soundness, scientific status etc. of the knowledge. Ignorance refers to all 'don't know what we don't know'. Funtowicz and Ravetz talk about the border with ignorance rather than ignorance because by definition we cannot say anything useful about that of which we are ignorant, *"but the boundless sea of ignorance has shores which we can stand on and map."*

The two-dimensional classification scheme defines areas to be addressed in uncertainty management in integrated models. This scheme is presented in Table 6.2.

Table 6.2 Two-dimensional classification scheme for uncertainties, defining areas to be addressed in uncertainty management in IAMs (modified from Van der Sluijs, 1995, 1996).

source	**type**	inexactness	unreliability	ignorance
input data				
conceptual model structure	parameters			
	relations (functional error)			
technical model structure	process error			
	resolution error			
	aggregation error			
	model fixes			
bugs	numerical error			
	software error			
	hardware error			
model completeness				

In the following we use this scheme to analyse the match between the types and sources of uncertainty that should be addressed on the one hand and the current practice of uncertainty management in IAMs and the available methodologies for addressing different types and sources of uncertainty in models on the other hand. Further we will look at the reasons for the mismatch and identify areas for improvement.

6.3 Addressing uncertainty due to inexactness

This section discusses the methods and practices of addressing uncertainties due to *inexactness* uncertainties. In section 6.3.1 we discuss inexactness in input data and model parameters. Section 6.3.2 deals with uncertainties regarding conceptual and technical model structure. Section 6.3.3 discusses uncertainties regarding model completeness.

6.3.1 Uncertainties in input data and model parameters

Uncertainties in *input data* and model *parameters* can be addressed by *sensitivity analysis* and more thoroughly by *uncertainty analysis*. Sensitivity analysis is the study of the influence of variations in model input data, parameters etc. on model outputs. Uncertainty analysis is the study of the uncertain aspects of a model and their influence on the (uncertainty) of the model output (Janssen *et al.*, 1990; Janssen *et al.*, 1994).

The best way to analyse how the quantified uncertainties in *input data* and model *parameters* propagate through the model is by means of stochastic modelling. In stochastic models the values of input data, parameters and variables are represented by probability distribution functions. Monte Carlo Simulation is a technique for stochastic model-calculations. In its simplest form, Monte Carlo traces out the structure of the probability distributions of model output by calculating the deterministic results (realizations) for a large number of random draws from the distribution functions of input data and parameters. In the case of random sampling, the number of realizations has to be rather large to obtain sufficient information about the probability distribution in the model outcome. This takes a lot of computing-time. For that reason more advanced sampling methods have been designed that reduce the required number of model runs needed to get sufficient information about the distribution in the outcome. Latin Hyper Cube sampling is the most efficient method currently available. It makes use of stratification in the sampling of individual parameters and preexisting information about correlations between input variables (McKay *et al.*, 1979; Janssen *et al.*, 1991; Janssen *et al.*, 1994; Schimmelpfennig, 1996). If applied to the entire model, Monte Carlo techniques also assess how uncertainties propagate through the model. In terms of Table 6.2, these techniques map the uncertainty in model outcome which results from inexactness in the value of *input data* and *parameters*.

A major problem in uncertainty analysis in IAMs is that sensitivity analysis and Monte Carlo modelling - even when using the most efficient techniques- are resource consuming (time, money, research capacity, computer capacity): "*Using a coupled ocean atmosphere model to perform a single several-century simulation might take a year on a super computer. Exploring the effect of a* *single uncertain input parameter would require 50 or 100 runs - a task so expensive and time-consuming that it precludes any formal uncertainty or sensitivity analysis in such models.*" (E-lab January-March 1995, MIT). For this reason, as an alternative to stochastical modelling less time-consuming sampling methods have been designed which consequently provide less complete information on the uncertainty in the outcome. The simplest sampling method is best-case, mean-case worst-case sampling.

The TARGETS (Tool to Assess Regional and Global Environmental and Health Targets for Sustainability) modelling group at the Netherlands National Institute of Public Health and Environmental Protection (RIVM) uses an other rationale for non-random sampling from the almost infinite number of combinations of tenable (discrete) choices for values of input data and parameters, in order to provide consistent routes through the tree of (discrete) choices constituted by the interpretive space in present-day knowledge. Within TARGETS, the subjective component in uncertainty is operationalized by well-defined cultural perspectives which guide the choice of values for *input data* and *parameters*. The three cultural perspectives are called Hierarchist, Egalitarian, and Individualist, which is a simplification of the original group-grid classification developed by Douglas and Wildavsky (1982) and Schwartz and Thompson (1990), who distinguish two more categories (the Fatalist and the Hermit). The axiom is that each perspective consists of a different myth of nature (fragile, robust, or robust within limits) and a different management style for managing the risk (prevention, adaptation or control).[1]

There is, however, no *a priori* reason why cultural-theory sampling would provide better insights than, for instance the more traditional 'best-case/mean-case/worst-case' rationale for non-random sampling. This depends on the questions addressed and the object in view by the uncertainty analysis.

Further, non-random sampling structurally ignores a large number of possible routes through the tree of choices. Consequently, cultural-theory sampling structurally ignores part of the extant uncertainty. The only way to reveal the total distribution of the outcome associated with the total set of model assumptions and their quantified uncertainties is Monte-Carlo analysis with random sampling or Latin Hyper cube Sampling[2].

This does not mean that cultural-theory sampling is useless, but its value lies somewhere else. Cultural-theory sampling allows sensitivity studies of the type: "suppose I choose a hierarchist management style for managing the risk of climate change, based on the sampling from the uncertainty ranges and alternative model structures that best correspond to the hierarchist myth of nature, what then will happen if the hierarchist myth is wrong and nature acts instead according to the egalitarian myth?"

---

1 Cultural theory has been criticized for its oversimplification of reality, for being too static (in reality one can be a hierarchist at work, a fatalist in leisure time, and an egalitarian at home), for its undue universal claims (whereas, in reality, one can for instance act as a hierarchist when confronted with problem A, and as an egalitarian when confronted with problem B) and for not taking account of complex systems of myths of nature (in complex adaptive systems such as the earth system, one finds a multiplicity of response patterns to forcing which are changing over time as well, rather than an eternal three-fold structure of robust, fragile and robust within limits). For a critique of cultural theory, we refer to Trisoglio (1995).

2 On the other hand, completely-random sampling runs the risk of overestimating the total uncertainty in a model result, because it can include realizations based upon combinations of parameter values that are unlikely or impossible in reality.

The method is in an experimental phase and has been tested on a few of the sub-models of TARGETS (Van Asselt and Rotmans, 1995; 1996; Van Asselt, Beusen and Hilderink, 1996).

A second problem in determining total inexactness uncertainty by Monte Carlo Simulation is the quantification of the spread in, and the identification of the distribution functions of all input data and model parameters. Information about the probability distribution functions of the values of most input data and model parameters is usually not available or is insufficient. In the absence of information on the shape of the probability distribution function, a Gaussian (normal) distribution is usually assumed as a first approximation. In the case of Gaussian assumptions, the vector of all input-data and parameters of the model can be represented stochastically by its first two statistical moments, a vector of mean values and an associated covariance matrix. Usually a vector of standard deviations of the input-data and parameters is used instead of a covariance matrix, due to lack of information on the nature of the co-variance matrix. Such a simplification conveys the implicit assumption that the probability distribution functions of the values of input data and parameters are independent, which is unrealistic if joint distributions exists. This caveat must be taken into account when the results of subsequent uncertainty analysis are being evaluated (Young and Parkinson, to be published).

In several Monte Carlo uncertainty analyses, the lack of knowledge about the shape of probability distribution function was a reason to analyse the sensitivity of the results to the shape of the distribution function chosen (e.g. uniform, triangle, truncated normal and irregular) (Alcamo and Bartnicki, 1987). These analyses have demonstrated that in the tested cases, the sensitivity to the assumed shape was low, indicating that uncertainty about its shape might be negligible. As far as we know, there is however no *a priori* reason that this is a general principle. However, in practice in cases where the shape is unknown, a Gaussian shape is usually chosen, without analysing the sensitivity to the shape. This is an omission.

A substitute for the lack of knowledge on the distribution functions is the use of subjective probability functions which are obtained by the Delphi-method (Dalkey, 1967) for consensus estimates or by the systematic combination of expert judgments (for a review of methods, see Genest and Zidek, 1986). Titus and Narayanan (1996) have used this *Delphic Monte Carlo* technique to identify the distribution functions in input data and parameters for their probabilistic model study of a sea level rise, using Monte Carlo techniques. The IIASA population project has also used this Delphic Monte Carlo technique to develop probabilistic world-population projections (Lutz *et al.*, 1996).

The application of Delphi-methods for the drafting of probability distribution functions of model input and model parameters for Monte Carlo Analysis, brings several important methodological difficulties. First, the fraction of experts holding a given view is not proportional to the probability of that view being correct. Second, one may safely average estimates of model parameters, but if the expert's models were incommensurate, one may not average models (Keith, 1996). Third, if differences in expert opinion are unresolvable, weighing and combining the individual estimates of distributions is only valid if weighted with competence of the experts regarding making the estimate. There is no good way to measure the competence. In practice, the opinions are weighted equally, although sometimes self-rating is used to obtain a weight-factor for the experts competence (one of E.Paté-Cornell's referee-comments on, and cited in, Titus and Narayanan, 1996). Fourth, the results are sensitive to the selection of the experts whose estimates are gathered.

Although subjective probability is an imperfect substitute for established knowledge and despite the problems of aggregation of expert judgement, if nothing better is available it is better to use subjective probability distributions than deterministic point-values so that one has at least a first approximation of the uncertainty.

A third problem with stochastic models can be the interpretation of the results. According to Rotmans, there is no means for representing the outcomes of stochastic models without introducing confusion. This also enhances the chance that results will be misused (Rotmans, 1994). Alcamo also stresses the problems that will arise in interpreting the outcomes from stochastic models: *"we haven't even figured out how to use that new explicit information"*.

When applied to scenario calculations with IAMs, the result of Monte Carlo Simulation can be represented graphically as a bundle of trajectories (that is: the set of Monte Carlo realizations for a given scenario) representing the distribution function of the outcome for a given input scenario. In theory, that information is valuable for identifying for instance to what extent the bundle of trajectories calculated from a given scenario, remains within a pre-defined 'safe corridor'. Then we can perform a goal-searching procedure by adjusting the scenario according to the discrepancy between the calculated bundle and the safe corridor. We can repeat this until we have e.g. the most cost-effective scenario that is safe enough (e.g. 90% of the bundle lies inside the corridor).

There are two major difficulties in applying such an approach. The first one results from the circumstance that the results of IAM studies need to be comprehensible for policy-makers. This puts the experts under pressure to reduce the complexity to simplified unambiguous quantitative information (See section 1.4 of this thesis). If the 'safe corridor' has more than two dimensions, graphical representation of the bundles of trajectories and the corridor is no longer possible. This implies that model-specific aggregated performance indicators have to be constructed to reduce the number of dimensions of the corridor to two or less. Such constructs give rise to a loss of information and rely upon the assumptions that have to be made to construct them. In other words, such indicators are tricky, and one has to do a lot of thinking about the consequences of the assumptions made when interpreting the results. For instance, in the IMAGE 'safe landing zone' studies, four indicators were used (see chapter 5 of this thesis). As a means of graphical representation, these IMAGE studies use the three colours from a traffic light. In the resulting graphs in which a climate variable is plotted as a function of time. For each scenario, a different line is produced: If all criteria stay at least 20% below their target value, the line is green, if at some moment in the simulated future at least one indicator is exceeded by more than 20% the line turns red, if at least one criterion is in the 20% interval around the target value, the line is amber. The loss of information in this example is obvious. If we see a colour change from amber to red in a line in the resulting graphs, we don't know which of the four criteria is violated. For such information we need to go back to the multi-dimensional results of the model, which complexifies the communication of the results to the policy-makers.

The second difficulty is that, unfortunately, computer resource limitations make the above-described procedure impossible in practice, because the computing time required for such stochastic goal-searching is almost infinite, especially if the 'safe corridor' is multi-dimensional. There are advanced stochastic goal-searching techniques which make it unnecessary to calculate the whole bundle of trajectories before an adjustment is made to a scenario (see e.g. Ermoliev and Wets, 1988). This is achieved by implementing an algorithm in the calculation which makes specific adjustments in the scenario after each single realization. The number of iterations to obtain results that fit with the safe corridor, is hardly larger than the number required to calculate a bundle of Monte Carlo realizations for one scenario. The stochastic search technique allows comparative analysis of solutions such as optimal structures of future societies (in terms of mix of technologies and practices to fulfil demands for goods and services), policy variables and solutions that are robust against the uncertainties.

There is an important methodological constraint of stochastic goal searching for the climate change issue. The problem is that many possibly significant uncertainties cannot yet be quantified (see chapter 5 of this thesis). Consequently, the results are only robust against the uncertainties accounted for in the stochastic model. Such results need not be robust against the unquantified uncertainties, which makes the stochastical goal searching approach of limited use for the climate issue, given the state of knowledge.

Fransen and Reuvekamp (1995) developed an alternative approach to tackle the uncertainties in climate model outcomes. Their approach combines model results and aggregated subjective correction parameters. The method starts with a model result which they call the *modelled wisdom* (for instance, the projected temperature increase in 2100 relative to 1900). To account for uncertainties, limitations and omissions in the model, the method establishes four correction parameters to correct the modelled wisdom. These are: *added wisdom*, *global ignorance*, *zonal ignorance* and *regional ignorance*. Each parameter is expressed as a mean value with a standard deviation, and has the same unit (for instance °C) as the *modelled wisdom*. The method relies on the (tricky!) assumption that each parameter is normally distributed around its mean value, indicating that the interval constituted by the standard deviation corresponds to a 68% probability interval, and twice this interval represents the 95% probability interval. By combining the modelled wisdom with the four correction parameters, a probabilistic estimate is produced, which allows the calculation of the probability of exceeding a specified threshold of a climatic variable. The method further relies on the capacity of the four correction parameters to correct for the omissions in uncertainty analysis in the models. Given our analysis of limitations of the models presented in chapter 5 of this thesis, we have severe doubts as to whether their over-simplistic method yields reliable results. An additional methodological problem with the subjective correction factors is the circumstance that experts tend to be systematically overconfident about their ability to make predictions (Lichtenstein *et al.*, 1982; Henrion and Fischhoff, 1986), leading to an underestimation of the uncertainty. We think that before Fransen and Reuvekamp’s method can be of use, a more explicit and comprehensive analysis of uncertainty in the modelled wisdom must become common practice.

For the IMAGE-2 model, Monte-Carlo-based uncertainty analysis has so far been carried out on several of its sub-models (e.g. Krol and Van der Woerd, 1994). The technique used is Latin Hypercube Sampling. A special software package for this purpose, UNCSAM (UNCertainty analysis by Monte Carlo SAMpling techniques) was developed (Janssen *et al.*, 1994). Although this is a first step, complete insight into how the quantified uncertainty in model input and in model parameters propagate through the model, can be obtained only by applying Monte Carlo Analysis to the entire integrated model. However, the lack of resources and competing priorities restrain the IMAGE modellers from doing so, even while the tools are available.

6.3.2 Uncertainties regarding conceptual model structure and technical model structure

Quantitative assessment of spread in key model outcomes caused by uncertainties associated with the way processes have been modelled is an unexplored area in the IAM field. This is also where the issue of model-validation comes in.

We use a taxonomy for errors in model structure, based partly upon a report by Environmental Resources Limited (1985, their taxonomy is listed in Table 6.1). First we distinguish between *conceptual model structure* and *technical model structure*. The technical model structure is the implemented version of the conceptual model structure on a computer with finite capacity, finite possibilities, finite reliability and encoded by imperfect software engineers. The compromises that have to be made to make the model computable with todays computers gives rise to a range of possible error sources. Our taxonomy subdivides conceptual model structure into uncertainty in values of model *parameters*, and *functional error*. We devide uncertainty in technical model structure into (1) errors introduced by the technique of modelling, including *process error*, *resolution error*, *aggregation error*, and *model fixes*, and (2) bugs, including *numerical error*, *programming error*, and *hardware error*. Uncertainty in model *parameters* can be treated in the same way as uncertainty in input data, which we discussed together in section 6.3.1. Below we discuss each of the other categories of our taxonomy.

Functional error

Functional error arises from uncertainty about the nature of the process represented by the model. Uncertainty about model structure frequently reflects disagreement between experts about the underlying scientific or technical mechanism. In practice, functional error is almost only assessed indirectly by inter-IAM comparison. A major IAM inter-comparison that included IMAGE was carried out by the Energy Modelling Forum (Energy Modelling Forum, 1994a, 1994b, 1995a, 1995b; Tol, 1994, 1995).

An alternative approach to address functional error is chosen by the TARGETS modelling group. Within TARGETS, the subjective component in uncertainty is operationalized by cultural theory sampling not only for the values for *input data* and *parameters* but also for different model *relations*. The innovative elements of the TARGETS/cultural-theory sampling method are that expert disagreement is incorporated in the model and that variable functional relations are introduced between the variables in the model. Although this method is of great value for managing uncertainty that stems from disagreement amongst experts, we think that the cultural theory approach is only a partial solution for the analysis of uncertainties. Cultural theory cannot be the panacea because it does not address the *per se* quality of the assumptions concerning model structure. It even runs the risk of marginalizing the quality issue by taking as an axiom that all thinkable perspectives are equally legitimate. When one applies that principle in its extreme form to model structure, then any conceivable model structure is legitimate and a plausibility-ranking of possible model structures based on scientific soundness would be just one of many legitimate views rather than intersubjective guidance for preferring one conceivable model structure to another. This would lead to total relativism, making science useless in the policy process. We take the position that science can provide authoritative arguments to rank the alternative model structures accoring to their plausibility. Also we think that only scientifically tenable model structures should be used in science for policy, but we acknowledge the co-existence of multiple scientifically tenable interpretations of reality. In our view, systematic evaluation of the quality of each interpretation is desirable to further limit the interpretative space and (at least to attempt) to provide intersubjective rationales for discriminating between - or at least for ranking - different possible model structures.

Process error

Process error arises from the fact that a model is by definition a simplification of the real system represented by the model. Examples of such simplifications are the use of constant values for entities that are functions in reality, or focusing on key processes that affect the modelled variables by omitting processes that play a minor role or are considered not significant. What processes are considered relevant is often related to the time-scale of the model. For instance, for very long term-carbon cycle modelling, many mechanisms that are dominant on shorter time-scales become insignificant, whereas mechanisms that are dominant on the very long time-scales, such as the carbonate silicate geochemical cycle, are assumed to be insignificant for models of the shorter time scales (see chapter 4 of this thesis).

Another example of a process error is that if GCMs simulate climates that are far removed from the current climate, they become inadequate e.g. with a temperature increase of 5.5°C you might completely wipe out Antarctic sea-ice, with massive changes in physical processes affecting climate. This mechanism is not yet taken into account by the models (see chapter 2 of this thesis).

Process errors can also occur from the way in which sub-models in IAMs are linked. In most of the current IAMs feedbacks between variables and relations located in different sub-models are not evaluated after each time step of integration of the model. In other words, the models are not really integrated.

The magic key word in handling this type of uncertainty is model validation. It has however been argued that model-validation is in principle impossible (Beck *et al.*, 1996a; Beck *et al.*, 1996b). One can test the performance of the model compared to data, but that says nothing about the validity of the conceptual model structure. If the conceptual model structure is wrong, its forecasts are wrong most probably, even if the model reproduces past and present features in a reliable way. The conceptual model structure is based on theories. As Popper argued: theories cannot be validated, they can only be falsified. For that reason Konikow and Bredehoeft (1992, cited in Beck *et al.* 1996) prefer terms such as *"model testing, model evaluation, model calibration, sensitivity testing, benchmarking, history matching, and parameter estimation."* Beck *et al.* (1996a) discussed the problems of model validation and observed that "*The difficulty of model validation tends to increase as the degree of extrapolation from observed conditions in the past increases. And not surprisingly, the greater the degree of extrapolation so the greater is the necessity of relying on a model for the conduct of an assessment.*" (See also Beck, 1994).

Toth (1995, p226) has proposed three routes for model verification: 1) check against historical records; 2) adoption of models and codes from other modelling groups for conceptual verification;

and 3) model inter-comparisons. We can add a fourth route to this list, namely to make short-term predictions and check these against what actually happens. Keilman (1996) did such a test for past population forecasts of statistical agencies, and found that these old forecasts supplied useful information for perhaps up to 10-15 years ahead, but certainly not longer. To give another example of this way of testing models: using the GCM model of the NASA Goddard Institute of Space Studies (GISS), Hansen (1992) made a five-year prediction of the effect of (stratospheric) dust emissions of the June 1991 Mount Pinatubo volcanic eruption on the global climate, just after the eruption took place. Another prediction was made by Kolomeev *et al.* (1993). Both predictions were checked independently against observations by the UK Hadley Centre (Parker *et al.*, 1996). The timing and size of the predicted and observed global land and ocean surface air temperature anomalies matched rather well, although not perfectly.

For most sub-models of IMAGE, the data from 1970-1990 were used for model verification. In 1993, the sub-models for agricultural demand, terrestrial vegetation, land use change and atmospheric composition were tested against historical data (Hordijk 1993). In 1994, the IMAGE team constructed a hundred-year historical scenario and database for model verification (Batjes and Goldewijk, 1994).

Resolution error

Resolution error arises from the spatial and temporal resolution of the model. The possible error introduced by the chosen spatial and temporal resolutions can be assessed by means of sensitivity analysis. However, this is not as straightforward as it looks, since the change in spatial and temporal scales might require significant changes in the model structure. For instance, going from annual time steps to monthly time steps requires the inclusion of the seasonal cycle of insolation. An other problem can be that data are not available for a lower resolution.

Aggregation error

The scaling up or scaling down of variables to meet the required aggregation level of the sub-modules they feed into is another possible source of error. In cases of non-additive variables, the scaling-up or scaling-down relations are always to a certain degree arbitrary (Hellström, 1996). For instance, IMAGE uses a down-scaling procedure to produce local climate in one grid cell, so that it can take biospheric feedbacks between climate and vegetation into account. In each time step, the interaction between the down-scaled climate in the grid cell and the vegetation type in the grid cell is evaluated, followed by an upscaling step from the grid level to regional figures by means of aggregation. The credibility and soundness of this high resolution two-dimensional modelling practice that produces climate on a grid scale is subject to scientific controversy because of the difficulties of regional climate predictions with our present state of knowledge. In the evaluation of the first phase of the Dutch NRP it was concluded that *"A crucial area of weakness in IMAGE is the regionalization of climate change. Previous reviews recommended abandoning the 2D model, but this does not seem to have been done."* The reason that it was not abandoned is that the IMAGE modellers believe that the methodology is scientifically sound, and that it is the only available way to dynamically include the feedbacks between vegetation and climate. Alcamo (1994b) maintains that unbiased errors on a geographic grid sometimes cancel each other out when gridded data are aggregated up to regional averages. He believes that, as long as one doesn’t see the grid specific calculations as predictions, and one only uses the regionally aggregated figures, the uncertainty becomes acceptable. However, there is no a *priori* reason to assume that the errors at grid-level are unbiased.

Model-fix error

Model-fix errors are those errors that arise from the introduction of non-existent phenomena in the model. These phenomena are introduced in the model for a variety of reasons. They can be included to make the model computable with today's computer technology, or to allow simplification, or to allow modelling at a higher aggregation level, or to bridge the mismatch between model behaviour and observation and or expectation.

An example of the latter is the flux adjustment in coupled Atmosphere Ocean General Circulation Models used for climate projections. Flux adjustment is a fix in coupled atmospheric and ocean GCMs to circumvent the problem that the fluxes of heat and fresh water between the ocean and the atmosphere calculated by the coupled model lead to phenomena that contradict the observations. Without such adjustments the simulated reference climate pattern for increased CO_2 experiments drifts away from the observed climate. The flux adjustment terms are calculated from the difference between the modelled surface fluxes and those required to keep the model close to current climate (Gates *et al.*, 1996). Flux adjustment has been criticized particularly because the flux adjustment terms for the heat fluxes between ocean and atmosphere are larger than the anthropogenic forcing term caused by a doubling of CO_2. According to IPCC, the main purpose of flux adjustment is *"to ensure that any perturbation, such as that due to increased CO_2, is applied about a realistic reference climate so that the distortion of the major climate feedback processes is minimized"* (Gates *et al.*, 1996). For a critical review of the issue of flux adjustment we refer to Shackley *et al.* (1996).

The effect of such model fixes on the reliability of the model outcome will be bigger if the simulated state of the system is further removed from the (range of) state(s) to which the model was calibrated.

Numerical error

Numerical error arises from approximations in numerical solution, rounding of numbers and numerical precision (number of digits) of the represented numbers. When we asked Leen Hordijk[1] (1994) how these uncertainties are being assessed in IAM practice, he said that *"in the models I have seen there is not much attention paid to this. But there are not very many numerical approximations. The models are usually simple and linear. Behind these linear equations there is sometimes non-linearity. I agree that the IMAGE model should pay attention to numerical errors. But there is always the tension between further development of the model, its use in policy-making, and other issues in the modelling team. It is also a matter of money*."

Complex models such as IMAGE include a large number of linkages and feedbacks. This property of process-oriented IAMs enhances the chance that unnoticed numerical artifacts co-shape

[1] In the eighties Leen Hordijk was project leader of the RAINS model at IIASA. In 1987 Hordijk moved to RIVM and from the sidelines became involved in the IMAGE project. Later Hordijk was appointed as a professor at Wageningen Agricultural University where he leads a research group on Environment and Climate (WIMEK). In 1992 he set up the IMAGE advisory board.

the model behaviour to a significant extent. Therefore, the analysis of numerical error in IAMs deserves more attention. A difficulty in doing this is that a systematic search to artifacts in model behaviour which are caused by numerical error, requires a mathematical ’tour de force’ for which no standard recipe can be given. It will depend on the model at hand how one should set up the analysis.

To secure against error due to rounding of numbers, one can test the sensitivity of the results to the number of digits accounted for in floating-point operations in the model calculations.

Software error

Software error arises from bugs in software, design errors in algorithms, type-errors in model source code, etc. Here we encounter the problem of code verification which is defined as: examination of the numerical technique in the computer code to ascertain that it truly represents the conceptual model and that there are no inherent numerical problems in obtaining a solution (ASTM E 978-84, cited in Beck *et al.*, 1996).

If one realizes that some of the models have hundreds of thousands of lines of source code, errors in it cannot easily be excluded and code verification is difficult to carry out in a systematic manner. In our interview with Hordijk (1994) we obtained some examples of software errors in the RAINS model (Regional Acidification INformation and Simulation, see section 5.2 in chapter 5 of this thesis), an intergated model for the assessment of acidification in Europe: *"In the RAINS model I once encountered a serious error. You could view the RAINS model not only for the whole of Europe, but also for a geographical rectangle. If you take for instance West Germany, and you put a rectangle over it, then you have not only West Germany, but also a part of the Netherlands, a part of East Germany, a part of Switzerland, a part of the Czech republic. The old soil model showed a bar-diagram for Germany indicating the pH distribution of the soil. In addition it gave a bar-diagram of the entire rectangle. That one was different, of course. For England the diagrams should be equal, because the rectangle did not include surrounding countries. To my surprise this turned out not to be the case when I tried it once. Max Posch, who did the computer programming at that time, searched for a very long time why this was happening. Finally it turned out that for a big part of Europe the file with soil-types - we distinguish 88 soil types - was read the wrong way round. Consequently, the diagram of the rectangle over England used the soil data of a different rectangle, which contained a part of France. So all the diagrams for any rectangle in Europe were wrong."*

Another example from RAINS mentioned by Hordijk is: *"It hasn’t had any effect on the results, but it was discovered only recently, although the model has existed for 10 years or so. If one chooses Bulgaria from one of the menus from RAINS, then Rumania appears on the screen. And if you choose Rumania, then you get Bulgaria on the screen. So, somewhere in the early period of RAINS someone put a wrong pointer in the software. And once such a map has been made, it is passed from one version to the next version of the model. The error goes with it and can remain undiscovered for years. Nobody can guarantee that nothing else has not yet been discovered which could have influence on the results. But you can minimize that risk by what I did as project leader. When my team had finished a new version of the model, I sat for many days behind my PC, running the model with many different scenarios, the one just a bit different from the other, and* *then comparing the results; a sort of a on-line sensitivity analysis."*

The latter method is what Hordijk calls the 'rack method', which he learned from Professor Somermeijer: *"That means, you take the model and you enter very extreme values and see what happens. Repeat this for a range of extreme values. If the model does not exhibit strange behaviour, there is a fair chance that the model is stable. If it does show strange behaviour, then you've got to search. If it exhibits strange behaviour for values that deviate significantly from the default settings but that are not really extreme, then you have a serious problem. That is a rough first recipe, but it does work."*

To secure against undiscovered bugs in the compiler-software, one can test the sensitivity of critical model outputs to the choice of the compiler-software used to compile the source code. Just use another compiler and see if the result is reproducible. In practice, one often encounters portability problems when trying to compile the source code with another compiler, due to a diversity of standards in programming languages, which means extra work (adjustment of the source code to match the standard of the compiler).

Hardware error

Hardware errors in the outcomes of IAMs arise from bugs in hardware. An obvious example is the bug in the early version of the Pentium processor for personal computers, which gave rise to numerical error in a broad range of floating-point calculations performed on that processor. The processor had already been widely used worldwide for quite some time, when the bug was discovered. It cannot be ruled out that hardware used for IAMs contains undiscovered bugs that might affect the outcomes, although it is unlikely that they will have a significant influence on the models' performance. To secure against hardware error, one can test critical model output for reproducibility on a computer with a different processor before the critical output enters the policy debate.

6.3.3 Uncertainties regarding model completeness

The IAM can have omissions at three levels: *causes*, *processes*, and *impacts*. We might overlook or underestimate anthropogenic *causes* of climate change, e.g. water emissions by aircraft in the upper atmosphere; indirect climate effects of perturbation of geochemical cycles other than carbon (e.g. phosphate) via its effect on the biota; climate effects of ocean pollution via its effects on the biota. Also we might overlook greenhouse gases. For instance, the greenhouse effect of SF_6 entered the assessments only recently (Houghton *et al.*, 1994; Cook, 1995), whereas the greenhouse gas NH_3 is not included in any current climate risk assessments. NH_3 was mentioned as an anthropogenic greenhouse gas in earlier assessments (Wang *et al.*, 1976; Hekstra, 1979; Schuurmans *et al.*, 1980; RMNO, 1984, see chapter 3 of this thesis) and it plays a crucial role in geological models of the earths' early atmosphere (e.g. Margulis and Lovelock, 1974). NH_3 has a pre-industrial ambient concentration of 6 ppbv and an atmospheric life-time of about one week in the present day atmosphere (figures from Margulis and Lovelock, 1974 and Wang *et al.*, 1976). Its concentration is therefore not well-mixed over the globe, but in the immediate environment of permanent emission sources such as intensive cattle-breeding it might significantly affect the local radiation balance and hence influence the local climate. Wang *et al.* calculated that a doubling of NH_3 from 6 ppbv to 12 ppbv might cause a temperature increase of 0.12°C. Further, NH_3 can form aerosols such as ammoniumsulphate (Junge, 1950) or ammoniumchoride, also affecting the climate system.

We might also overlook important *processes*: unknown feedback loops and supposed feedback loops that are not yet mathematically representable due to lack of knowledge (see e.g. section 5.4.5.ii in chapter 5 of this thesis). It is also possible that important processes are missing because scientists and modellers are not (yet) aware of their importance. We know that not all relevant substances and processes in complexly coupled atmospheric chemistry pathways of greenhouse gases have been identified. The uncertainties in the carbon budgets still allow for the existence of missing sinks. Recently it was discovered that fungi play an important role in the CH_4 cycle in peats because they effectively fix CH_4 emitted from lower peat layers (personal communication Mark Kilian, May 1996). This might affect our understanding of what could happen if the permafrost starts thawing and the CH_4 fixed in clathrates will be emitted to the atmosphere.

Finally, we might overlook *impacts* of climate change on aspects that are valued by actors who are not yet participating in the assessment projects. Currently, the macro-economic-oriented IAMs are dominated by economists, whereas the process-oriented IAMs are dominated by natural scientists. According to Kasemir *et al.* (1996), *"scientific modelling without sufficient input from public discussion risks focusing on irrelevant issues while ignoring questions of interest to the public."* Examples of omissions on the impact side of IAMs can be the impact on human migration patterns and environmental refugees or the extinction of species.

Systematic methodologies for assessing model completeness directly are not available. It can only be addressed indirectly by quality control processes such as advisory boards, peer review, model inter-comparison, competition between modelling groups, etc. We will discuss these in the next section.

6.4 Addressing unreliability: quality control in IAM practice

The lack of quality control and good scientific practice in policy-oriented modelling was already stressed by Keepin and Wynne (1984)[1]. Their twelve-year-old findings on energy modelling are still very topical and highly relevant to current IAM practice. Keepin and Wynne (1984) analysed the energy models of IIASA and found that despite the appearance of analytical rigour, IIASA's widely acclaimed global energy projections were highly unstable and based on informal guesswork. According to Keepin and Wynne, this was partly due to inadequate peer review and quality control, which raised questions about political bias in scientific analysis. They concluded amongst other

[1] The issue of quality assurance in risk-management was also prominent in the debates on light-water reactor safety in the seventies. For instance, Lewis *et al.* (1975) recommended that *"measures should be taken to quantify the effectiveness of the present quality assurance program, using both the analysis of experience already reported and new measurements on the quality assurance system."* This quote also illustrates the demand for tools to measure quality for the purpose of risk management, which we address in section 6.7 of this chapter.

things: "*First, many crucial components of the scenarios are generated informally and supplied as inputs to the formal computer models, which then reproduce these projections with only minor alterations. Thus, the models have not analytically "discovered" feasible energy futures. Indeed, despite the appearance of analytic sophistication and rigor, the models serve primarily as a static accounting framework of the analysts.*" and "*One important lesson is that the most exquisite formal analytic modelling still embodies informal assumptions (often about sociopolitical values and institutional behaviour) that affect what technical outcomes are conceivable. Peer review of formal models can expose these assumptions for external debate and evaluation. Indeed, rather than attempting to identify objective policy truths, perhaps a more realistic role for policy modelling is to explore origins and consequences of different social and institutional assumptions. Such an approach would embrace (rather than deny) the interpenetration of science and politics in policy analysis.*". More than ten years later Toth (1995) still concludes: *"My review of current integrated assessments indicates the emerging need for a systematic and critical appraisal."*

J. Ravetz (E-mail message to J. van der Sluijs, 8 July 1996) suggests that the lack of quality control and good scientific practice might have three causes:

"1. Some people have effective "good practice" but their standards are not diffused among all practitioners. This is quite common in science, where a "leading" lab can get genuine results that few others can emulate.

2. The discussion of "good practice" is of the sort I call "lamp-posting", from the old story about a man who was seen by a neighbour in the early hours of the morning, crawling on the ground near the lamp-post. Asked what he was doing, he replied that he was looking for his keys. "Did you drop them there by the lamp-post?", the neighbour asked. "No, near my front door". "Then why are you looking near the lamp-post?" "Because at least it is light here, so if they were here I would find them."

Translated into practical terms, this means that the researchers concentrate on the soluble problems, even if the insoluble ones are more important. I got this impression from the discussion with Alcamo.

3. Finally, there is the possibility that discussions of quality are only a game. This might be played for political advantage within the field (who can demolish the other's research more effectively?), or to comply with external requirements."

To Ravetz's Number 1, we can add the cost-problem. We saw that despite the availability of tools for uncertainty assessment such as Monte Carlo Simulation, these are hardly being used in IAMs yet, due to their resource-consuming character. For the issue of cost-benefit analysis of the costs of generating more information on uncertainty and the benefits of this information, we refer to Hirshleifer and Riley (1992). Several studies show that the value of uncertainty reduction in climate risk assessment is likely to be great, especially for the following components: climate sensitivity, temperature damage function, GDP growth rate, and rate of energy efficiency improvement (Bruce *et al.*, 1996).

We found support for Ravetz's Number 2 in the evaluation report of the Dutch NRP, where it was observed that the program focused on the strengthening of existing areas of excellence in Dutch climate research, rather than on the key questions that need to be answered (Science and Policy Associates, 1995).

Ravetz's Number 3 is supported by the 'backlash phenomenon' which is most prominent in the

US, where industrial organizations such as the Western Fuels Association fund research to actively undermine the scientific credibility of the IPCC, while on the other hand the IPCC does its best to enhance its own credibility by maximizing representativeness and maximizing the process legitimacy of the consensus-building process (see e.g. Lunde, 1991).

Recently the greenhouse ’rebels’[1] (although some of the established IPCC scientists would call them ’cranks’) organized themselves in the European Science and Environment Forum that *"will seek to provide a platform for scientists whose views are not being heard, but who have a contribution to make"* (Emsley, 1996). They strongly criticize the ’science by consensus’ approach of the IPCC and have issued a book in which almost every link in the IPCC chain of arguments is challenged (Emsley, 1996). However, in their criticism on IPCC they make a delusion, because what IPCC does is not ’science by consensus’ but ’assessment’. As we argued in chapter 1 of this thesis, assessment is a different task from doing science. The abundance of conflicting scientific evidence was the very reason that IPCC was established. Not to force closure of disputes in the science, but rather to provide balanced scientific judgments: to provide a scientific base for the climate policy debate in the quicksand of abundant conflicting evidence and uncertainties.

We have to bear in mind that **all** actors with a stake in global warming have agendas of their own and are not always averse to manipulating uncertainty for various reasons. Uncertainties are often magnified and distorted to prevent public insight into the policy-making process and to obstruct the policy process (see e.g. Helström, 1996). The uncertainty question can be (and is) actively used as a strategy to undermine the role of assessment as a shared source of information, to achieve postponement of measures.

Clark and Majone (1985) have designed a taxonomy of criteria for quality control of policy-oriented science (Table 6.3). The taxonomy acknowledges that each actor that has a stake in quality control has a different role in the process of critical evaluation. For instance, scientists will emphasize other criteria in quality control than policy-makers. Further, Clark and Majone’s taxonomy distinguishes three general modes of critical appraisal: the *input*, the *output* and the *process* by which inquiry is conducted. As Ravetz (1986) stressed ten years ago, mastery of Clark and Majone’s table would make an excellent introduction to the methodological problems of policy-related science. Despite its potential value, Clark and Majone’s approach is not yet disseminated to the IAM practice.

Shackley and Wynne (1995) showed that the criteria for good scientific practice with respect to climate research are not solely determined from within science itself. Most of them emerge by a process of mutual construction with government policy institutions. According to them, this may now risk inadvertently foreclosing the consideration of potentially significant alternative scientific research and policy approaches. Boehmer-Christiansen (1994a, 1994b, 1994c, 1995) stresses an even more dominant role for institutions in this mutual construction of criteria for good scientific practice. Funtowicz and Ravetz (1993) have made a case for opening up the issue of good scientific practice by what they call ’extended peer communities’ (see chapter 1 of this thesis).

---

[1] The terms ’rebels’ and ’cranks’ stem from Funtowicz and Ravetz (1990), see also Table 8b. ’Rebels’ have some standing among their colleagues, whereas ’cranks’ have none. Who is a ’crank’ and who a ’rebel’ may change over time.

Table 6.3 A taxonomy of criteria for quality control of policy-oriented science (Clark and Majone, 1985)

	Critical mode		
Critical role	Input	Output	Process
Scientist	Resource and time constraints; available theory; institutional support; assumptions; quality of available data; state of the art.	Validation; sensitivity analyses; technical sophistication; degree of acceptance of conclusions; impact on policy debate; imitation; professional recognition.	Choice of methodology (e.g., estimation procedures); communication; implementation; promotion; degree of formalization of analytic activities within the organization.
Peer group	Quality of data; model and/or theory used; adequacy of tools; problem formulation; input variables well chosen? Measure of success specified in a advance?	Purpose of the study; conclusions supported by evidence? Does model offend common sense? Robustness of conclusions; adequate coverage of issues?	Standards of scientific and professional practice; documentation; review of validation techniques; style; interdisciplinarity
Program manager or sponser	Cost; institutional support within user organisation; quality of analytic team; type of financing (e.g. grant versus contract)	Rate of use; type of use (general education, program evaluation, decision making etc.); contribution to methodology and state of the art; prestige; can results be generalized, applied elsewhere?	Dissemination; collaboration with users; has study been reviewed?
Policy-maker	Quality of analysts; cost of study; technical tools used (hardware and software); does problem formulation make sense?	Is output familliar and intelligible? Did study generate new ideas? Are policy indications conclusive? Are they constant with accepted ethical standards?	Ease of use; documentation; are analysts helping with implementation? Did they interact with agency personnel? With interest groups?
Public interest groups	Competence and intellectual integrity of analysts; are value systems compatible? Problem formulation acceptable? Normative implications of technological choices (e.g. choices of data)	Nature of conclusions; equity; analysis used as rationalization or to postpone decisions? All viewpoints taken into consideration? Value issues	Participation; communication of data and other information; adherence to strict rules of procedure

If IAMs can be used by all actors and not only by experts, other issues of quality control can emerge. For instance, if policy-makers make direct use of IAMs there is a risk that the IAM may be used manipulatively, because policy-makers are not subject to the standard scientific quality control procedures such as peer review. The classic example is the case of US-president Bush's adviser Sununu. Sununu was President Bush's chief of staff from 1989 until 1991. He was a high-level policy-maker with a strong technology background (doctoral degree in engineering at MIT). Sununu had a reduced-form version of the NCAR climate model installed on his office computer. He used (according to several members of the IAM community: misused) the model to support the stance that measures were not necessary. He is widely reported to have convinced President Bush that the threat of global warming was overblown, and that arbitrary limits on carbon dioxide emissions would have a cost vastly exceeding their benefit (Technology Review, 1992; Risbey and Stone, 1992). According to Parson (1995), "*the resultant outrage among modellers and analysts was in part puzzling, since this story seems to realize the vision of senior policy-makers becoming fully conversant with assessment models. Several legitimate bases for the outrage are plausible, though. He was a busy man, using a simplified (but still very complex) model but not able to spend much time on it, and so was no doubt at risk of serious misunderstandings. A model on his machine in the White House is not open to scrutiny and technical argument. Nor might it be easily updatable to reflect advances in understanding.* The Sununu experience shows that it is desirable not to completely replace the experts by models, but it also raises questions about the democracy of the process of establishing policy-meaning of scientific knowledge and expert interpretation. A monopoly of scientists in this process also is undesirable, because as Funtowicz and Ravetz have argued (1992): "*in the light of such uncertainties, they* [the experts] *too are amateurs*".

The IMAGE 1 model was developed with virtually no external quality control. Consequently, the model received only low peer acceptance. IMAGE 1 received a lot of criticism from scientists, who thought that the approach was far too simplified (Rotmans, 1994). IMAGE 2 did better, partly because it was less simplified, partly because it was incorporated in the Dutch NRP, partly because it had learned from the criticism levelled at IMAGE 1 and partly because Joe Alcamo brought with him his experience of the RAINS model where peer review was common practice.

A problem in quality control is that due to the resolution and the aggregation level, IAMs contain many parameters (and other constituents) that are constructs resulting from simplification processes and are hence not well-specified, that is, they fail to pass Howard's clarity test. Howard's clarity test (cited in: Morgan and Henrion, 1990) reads: *"Imagine a clairvoyante who could know all facts about the universe, past, present and future. Could she say unambiguously whether the event will occur or had occurred, or could she give the exact numerical value of the quantity? If so, it is well-specified."*

An example from IMAGE 1 of a parameter that is not well-specified is the "thickness of the warmer ocean mixed layer". An example from IMAGE 2 is the "effective depth of the surface" in the surface heat balance equation in the zonal atmosphere climate model. These constructs are, in fact, model fixes to simplify the model, make it computable and allow modelling at higher aggregation levels. The validity of using such constructs depends on the validity of the assumptions made in each step of the simplification process. This chain of successive assumptions is usually poorly documented and relies highly upon tacit knowledge and wisdom of the modellers. From our attempts to understand the IMAGE 2 model on the basis of its documentation and its peer reviewed publications, we think that there is an urgent need for a comprehensive documentation of the chains of assumptions followed in the construction process of IAMs in order to allow for quality assessment and quality control. Without such documentation we can only resort to debriefing by depth-interviews with the modellers in order to get hold of the full set of assumptions on which the model is based.

A good example from the current practice of quality control of IMAGE is the multidisciplinary and international IMAGE Advisory Board, which was set up in 1992 with the explicit task of addressing the scientific quality of the model and its usability for policy development support (Hordijk, 1993; Solomon, 1994). The IMAGE modelling team took part in the meetings of the board. According to Alcamo (1994b), the advantage of mixed advisory boards is that the policy-makers hear from the scientists what the limitations are of the science, and the scientists can hear from the decision-makers what needs the model should fulfil. Alcamo calls the advisory board also a *"device to obtain the support of not only policy-makers, but also scientists. Because a model like IMAGE should be steered through the best science available, and the most policy relevant fashion as possible."* This implies that the board also is important as a means of obtaining legitimation and negotiating credibility for the model.

The advisory board played a significant role in the construction of the model. At the first meeting (December 1992) the first design of the IMAGE 2 model was revealed to the board to test whether it complied with both scientific and policy requirements. Using the board's comments, the first fully operational version, 2.0 was completed by the middle of 1993. Changes in the model induced by policy-makers (both directly from DGM and via the board) were, *inter alia,* more focus on the impacts on the food system, and on the adaptation capacity of ecosystems (both due to Article 2 of the FCCC), and a change in the regional break-down, in that the US and Canada were to be treated as separate regions to allow comparison and exchange with national research programs in that region (Alcamo, 1994b).

Another strategy to improve quality is to include the best science available (Alcamo, 1994b). We asked Alcamo[1] to describe the selection procedure used in the IMAGE project to ensure that the best scientific understanding is included, and how they find out whether all relevant processes are included in the model. He answered *"The selection is based on having a good interdisciplinary team. We propose something in our best judgement, and then test it against a wide variety of scientists. The procedure I use is as follows. (1) Design a preliminary version of the sub-model as simple as possible, yet with an "adequate" representation of key processes. (2) Review this preliminary design with an expert in the subject covered by the model, and add detail (begrudgingly) until the expert is more or less satisfied that key processes are represented. For example, we began modelling global land cover in IMAGE 2 by assuming that future agricultural land will be located according to very simple "land use rules". Later, experts in land cover studies advised us about more detailed rules and factors that we should include in our calculations, for*

---

[1] Joe Alcamo was prominently involved in the development of both the RAINS model and the IMAGE 2 model. He used to be deputy project leader of the RAINS model at IIASA. From 1 January 1992 to 29 February 1996 he was project leader of IMAGE 2. Nowadays, Alcamo is a professor at the Center for Environmental Systems Research at the University of Kassel.

example including the role of rivers in steering inland agricultural development. These and other factors we included in later versions of our land cover sub-model." (Alcamo, 1994b, revised in personal communication 1996)

Alcamo pointed out that another important mechanism for controlling the quality of the IMAGE model is the exposure of the model to peer review by submitting model-results for publication in scientific journals. At an early stage of IMAGE 2, an overview of the entire model was published as a special issue of the journal Water Air and Soil Pollution, and later published as a book. As we found in the case of the IMAGE advisory board, quality control was not the only motivation for publication. Another important aspect was credibility and acceptance by policy-makers and scientists. As a further means to strengthen the embedding of IMAGE in the scientific community, the terrestrial environment research of the IMAGE project is an activity of the Core Project "Global Change and Terrestrial Ecosystems" (GCTE), of the International Geosphere Biosphere Program (IGBP).

Not all actors who have a stake in climate change and climate policy (Table 6.3 shows a selection of these) are currently involved in the quality control procedures. A recent innovation in this field of quality control of IAMs is the use of focus groups. Focus groups are currently being used in a European research project called ULYSSES (Urban LifestYles, SuStainability and Environmental aSsessment). Experiments are conducted on the interaction of monitored focus groups with IAMs, which *"will be a microcosm of social learning, and will include the concerns of the intended users of IEA* [that is: Integrated Environmental Assessment, JvdS], *emphasizing the role of ordinary citizens"* (Kasemir *et al.*, 1996).

6.5 Addressing ignorance

Ignorance is the most difficult category of uncertainty to address. Ignorance refers to all 'don't know what we don't know'. In section 6.5.1 we discuss research as a strategy to address ignorance. In section 6.5.2 we discuss a special category of ignorance, namely *surprise*.

6.5.1 Reducing ignorance by research, a paradox

Ignorance is unassessable, so the only thing we can do is explore the border with ignorance. The paradox is that we try to reduce ignorance by doing more research, whereas more research increases the border with ignorance and ignorance increases with increased commitments based on given knowledge (e.g. Wynne, 1992). Pascal once said: *"Science is like a ball in a universe of ignorance. The more we expand knowledge, the greater the ignorance encountered by the ball's expanding surface."* (Cited in: Giarini and Stahel, 1993). Giarini and Stahel (1993, p219/220) have put forward the philosophical notion that *"Our ignorance and our imperfect information are an instance of disequilibrium, a condition of life and of evolution. Our growing ignorance, determined by the growth of our knowledge which increases the number of unanswered questions, is the best evidence that we are part of the flow of life. Experience tells us that whenever we have the feeling of having completely mastered and understood a problem, it is often because the object or the* *situation of reference no longer exists: we are just about to discover that our confidence in our capacity "totally" to understand is at least partly misplaced."*.

In the IAM practice, ignorance is addressed indirectly by embedding IAM projects in broader research programs. For instance, the IMAGE project is partly embedded in the Dutch NRP (Berk, 1993, Science and Policy Associates, 1995).

6.5.2 The modelling of surprise

"Much of the work to date has been based, implicitly or explicitly, on an evolutionary paradigm - the gradual, incremental unfolding of the world system in a manner that can be described by surprise-free models, with parameters derived from a combination of time series and cross-sectional analysis of the existing system. ... The focus on surprise-free models and projections is not the result of ignorance or reductionism so much as of the lack of practically usable methodologies to deal with discontinuities and random events. The multiplicity of conceivable surprises is so large and heterogeneous that the analyst despairs of deciding where to begin, and instead proceeds in the hope that in the longer sweep of history surprises and discontinuities will average out, leaving smoother long-term trends that can be identified in retrospect and can provide a basis for reasonable approximations in the future." (Brooks, 1986).

Surprise can play a role in every step of the causal chain. Examples from the past are discrete events such as the oil shocks of 1973 and 1979; discontinuities in long-term trends, such as the acceleration of USA oil imports between 1966 and 1973; but also events that turn out to trigger or accelerate the policy process such as the 1988 US heat wave, and the unprecedented damage (US$ 15,500,000,000) caused by super storm Andrew in 1992 (Property Claim Services, 1996). The natural system also has surprises such as the volcanic eruption of Mt. Pinatubo in June 1991 which is believed to be responsible for the observed discontinuity in the trends in atmospheric concentrations of CO_2, CO and CH_4 and in temperature (McCormick *et al.*, 1995).

A further issue is that non-linear stochastic systems might have contra-intuitive future states which are missed if the system representation is inadequate. Such an inadequacy can be the neglect of feedbacks in the system (see our discussion in chapter 5 of this thesis).

Another problem that might make models inadequate is that in real-world stochastic complex systems, the variable probability values are constantly in flux. Further, the natural stochasticity in nature constantly alters the relationships between system components, and new external variables are added regularly, which change the natural conditions for the overall system. For instance, the introduction of human-made substances, such as CFCs, into the atmosphere has dramatically changed stratospheric chemistry. As another example, the emission of a certain component can change the atmospheric chemistry pathways of a range of other components. These categories of "dynamic system dynamics" are not represented or are only poorly represented in current models.

The simplifications made to model complex systems despite our limited understanding might well rule out certain characteristics of system dynamics such as the existence and nature of attractors[1] in the system, which might be crucial in the evaluation of future behaviour of the system. This was demonstrated in chapter 4 of this thesis, where we showed that scientifically tenable moderate changes in the assumptions of the BLAG model of the long-term carbon cycle can have a dramatic influence on the resulting qualitative model behaviour, by introducing the existence of a transition of the system from point value equilibrium solutions of the system to a stable limit cycle solution when the system is exposed to external forcing.

The possibility of modelling surprise was an important discussion point at the recent IIASA and LOS Centre Meeting "Climatic Change: Cataclysmic Risk and Fairness"[2]. Some of the participants argued that because most of the Earth Systems Models use smoothened, idealized and deterministic functional relations, a part of the potentially identifiable surprise is ruled out by the way the model is constructed, namely idealized smooth curves are used to represent relations between variables, whereas nature contains noise and proves time and time again to be much more capricious and erratic. For instance, the temperature record of the global mean temperature obtained from aggregated measurements and advanced reconstructions of past climates is non-smooth and is understood to be a mixture of cyclical behaviour on virtually all time scales (such as the diurnal cycle, the seasonal cycle, the El Niño Southern Oscillation, the 11 year and 22 year solar cycles, the 80-90 year solar cycle, the Milankovitch cycles of 22, 43 and 100 thousand years), trends, and irregular fluctuations. These fluctuations are usually called 'natural variability' of the climate. The trends and the cyclic behaviour can be modelled with smooth assumptions, the irregular part cannot.

Recently, a new bottom-up modelling technique for complex adaptive (social) systems has been developed, called agent-based modelling. It can to a certain extent be used to model some of the aspects of surprise. The method has been demonstrated with a traffic model for the city Albuquerque. Travel behaviour and decision-rules of every single inhabitant of the city (the agents) are modelled, together with the road network. The resulting aggregated traffic patterns over the time of a day show the build-up of morning rush-hour traffic and resulting traffic jams. The hope is that a deeper understanding of how complex adaptive systems work will suggest the right type of mathematical structures and lead to a decent theory of these processes, which this new school of modelling believes will ultimately lead to the increased predictability of surprises, such as traffic jams (Casti, 1996a; Casti, 1996b).

A bottom-up approach for modelling the biosphere has been proposed by Westbroek and Muyser (1992), but their approach has not yet been demonstrated with an operational model.

We cannot solve unknowns simply by putting every single detail that we know in the computer and hoping that something shows up. In our view, what is needed for scientific progress is to ask the right questions, not to produce an endless stream of answers to 'what if' questions. In that sense computers are useless, because they cannot ask questions, they only produce answers. We think that at least in current state of the art, the usefulness of agent-based modelling and bottom-up biosphere modelling for IAMs is questionable. The agent-based approach can be of some use in a broader

[1] An attractor is a set of points which trajectories approach as the number of iterations goes to infinity, given that initial points are within the *basin of attraction* for that attractor (Hilborn, 1994). Examples of attractors are equilibrium points, limit cycles, chaotic attractors and strange attractors.

[2] Laxenburg, 20 to 22 july 1996; the author participated in this meeting.

process of integrated environmental assessment for the purpose of tentatively modelling decision systems or future technology choice. Such an approach could give us some kind of feeling for the possible dynamics of such systems.

Given the absence of adequate methodology to model surprise, a systematic search for examples of non-linearities from the past might be the prelude to a search for possible future surprises (Brooks, 1986). Other strategies that can help us to understand surprise include focusing on the underlying principles of surprise, which is what happens in surprise theory (Holling, 1986) and systematic ’thinking the unthinkable’ by imagining unlikely future events followed by the construction of plausible scenarios by which they might be realized (Kates and Clark, 1996).

Non-smoothness introduces a problem into sensitivity and uncertainty analysis because classic uncertainty analysis is based on smooth systems. Sensitivity analysis of non-smooth systems is a special topic that deserves more attention. Such analysis should focus on the identification of (thresholds in) indicators which could be used to predict jumps in the system and discontinuities in trends.

6.6 Areas for improvement in uncertainty management

In Table 6.4 we summarize the various tools available or currently being developed for managing uncertainties in integrated models. The results can be summarized in terms of the horizontal dimension of Table 6.2: Inexactness can be addressed by stochastic modelling which may be combined with subjective probability distributions and by ’cultural-theory sampling’ from the ensemble of tenable expert interpretations. Unreliability requires quality assessment (see also section 6.7) and quality control. Ignorance requires research programs. Surprise requires systematic search for surprises in the past and systematic imagination of plausible surprises for the future

Table 6.4 Tools available to address different sorts of uncertainties in IAMs.

source	**type**	inexactness	unreliability	ignorance
input data		-Sensitivity analysis -Monte Carlo simulation -Subjective Probability Distributions -Cultural theory sampling	-Quality-assurance -Quality assessment -Advisory boards -Peer review -Rack-method	-Research
conceptual model structure	parameters	-Sensitivity analysis -Monte Carlo simulation -Subjective Probability Distributions -Cultural theory sampling	-Quality-assurance -Quality assessment -Advisory boards -Peer review -Rack-method	-Research
	relations	-Monte Carlo Simulation -Cultural Theory Sampling -Inter-model comparison -Testing against historical data -Test short-term predictions -Code exchange between IAMs -Rack-method -Advisory boards -Peer review	-Quality-assurance -Quality assessment -Inter-model comparison -Testing against historical data -Test short-term predictions -Code exchange between IAMs -Rack-method -Advisory boards -Peer review	-Research
technical model structure	process error	-Inter-model comparison -Testing against historical data -Test short-term predictions -Code exchange between IAMs	-Quality-assurance -Inter-model comparison -Testing against historical data -Test short-term predictions -Code exchange between IAMs -Rack-method -Advisory boards -Peer review	-Research
	resolution error	-Sensitivity analysis	-Quality-assurance -Rack-method	-Research
	aggregation error	-Sensitivity analysis	-Quality-assurance -Rack-method	-Research
	model fixes	-Sensitivity analysis -Inter-model comparison -code exchange between IAMs	-Quality-assurance -Advisory board -Peer review -Rack-method	-Research
bugs	numerical error	-Sensitivity analysis (to number of digits in floating point operations)	-Quality-assurance -Rack-method	-Mathematical analysis of the model
	software error	-Sensitivity analysis -Code verification	-Quality-assurance -Code verification -Rack-method -Reproducibility testing	-Rack-method
	hardware error	-Sensitivity analysis	-Quality-assurance -Rack-method -Reproducibility testing	-Rack-method
model completeness		-Advisory boards -Inter model comparison -Peer review -Competition among IAM groups -Focus groups	-Quality-assurance -Advisory boards -Inter model comparison -Peer review -Competition among IAM groups -Focus groups	-Research

From our analysis we conclude that in current IAM practice, the best covered areas of uncertainty are in the upper left cells of the table, namely the *inexactness* in *input data* and *parameters*. The other areas defined by the table get significantly less systematic attention in uncertainty analysis in IAMs.

Recapitulatting, we found that techniques currently available for uncertainty analysis and uncertainty treatment in IAMs have three major shortcomings:

1. They do not fully address all relevant aspects within the whole spectrum of types and sources of uncertainty;
2. They fail to provide unambiguous comprehensive insight for both the modeller and the user into:
   a) the quality and the limitations of the IAM;
   b) the quality and the limitations of the IAM-answers to the policy questions addressed;
   c) the overall uncertainties;
3. They fail to systematically address the subjective component in the appraisal of uncertainties (with a partial exception for the cultural-theory-sampling method in the TARGETS model).

In terms of Tables 6.2 and 6.4, the areas of uncertainty analysis where improvement is needed most are the second column, namely the assessment of error due to the *unreliability* of the knowledge about input data and model structure, and the rows in the middle of the table, namely the assessment of error due to uncertainty in *conceptual model structure* and *technical model structure*.

We have proposed taking Funtowicz and Ravetz's (1990) NUSAP (Numeral, Unit, Spread, Assessment, Pedigree) methodology as a starting point for better uncertainty management in IAMs (Van der Sluijs, 1995). In the next section we will discuss the NUSAP methodology and explore how it can be used to improve uncertainty management in IAMs.

6.7 Disentangling the uncertainty problem: adding the quality dimension

In this section we present a methodology for addressing model quality that combines information obtained from sensitivity and uncertainty analysis with systematically obtained qualitative knowledge and tacit knowledge of experts. This information is used in an informed Delphic ranking exercise by an expert panel, in order to rank uncertainties in a model constituents according to their contribution to the (lack of) quality of the model output.

If uncertainties in IAMs are to be managed better, a first step is to distinguish between *quality*, which can be viewed as the inverse of "potential for improvement", and *limitations*, which refers to our limited capacity to know and understand and the inherent uncertainty in the system that remains if the "potential for improvement" has declined to zero. It should be remembered that what is an inherent limitation and what is, in principle, reducible uncertainty will change over time because of ongoing research and innovations that enlarge the toolbox (invention of better computers, invention of new mathematics for complex systems, invention of new modelling techniques, paradigm changes). This implies that "limitations" and "potential for improvement" can never be absolute and should always be treated as tentative wisdom rather than stable truth.

We illustrate the usefulness of the distinction between uncertainty due to limitations and

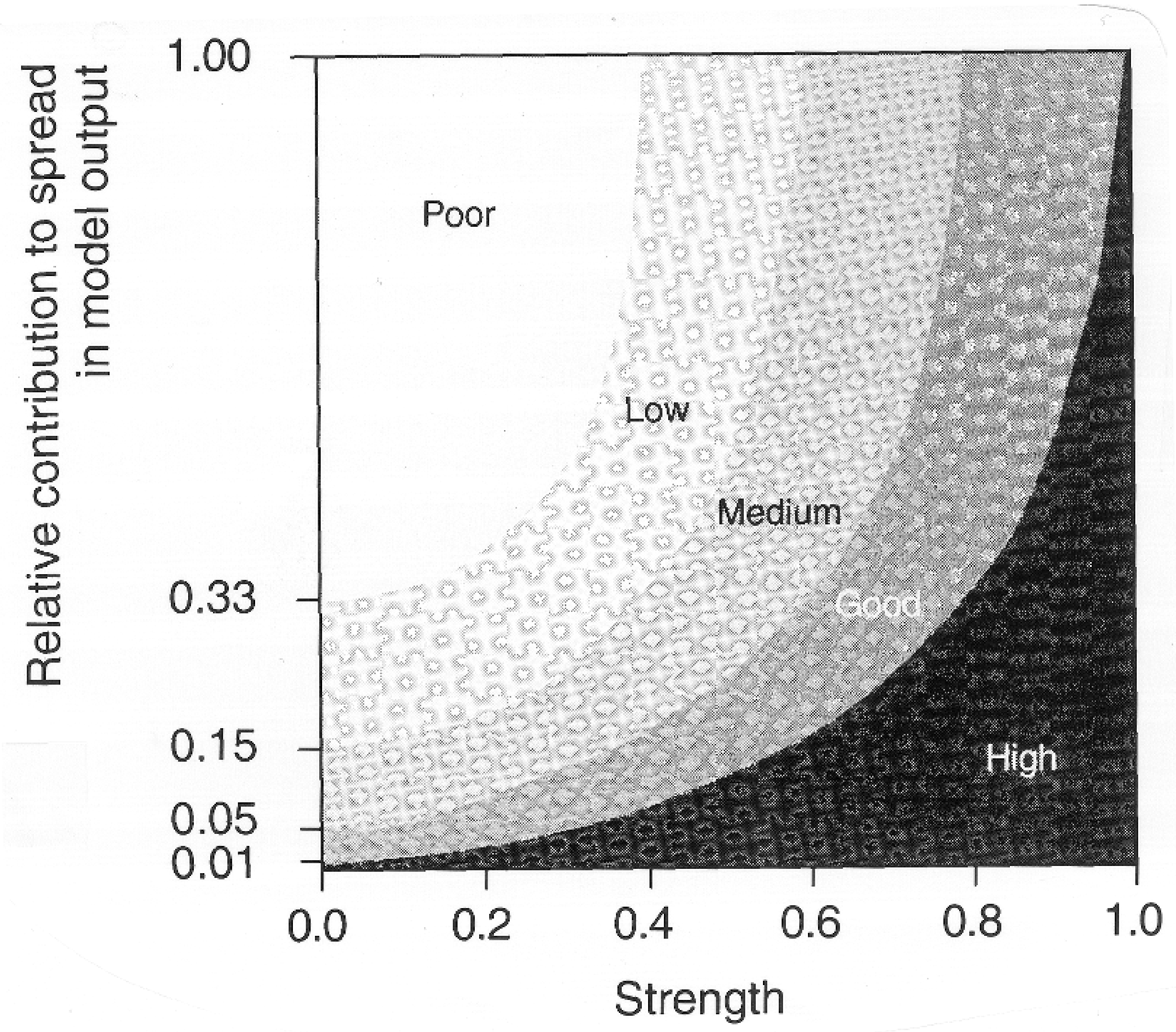

Figure 1 Assessment diagram, showing zones for the evaluation of model output (Funtowicz and Ravetz, 1990).

uncertainty due to lack of quality by presenting the assessment diagram for the evaluation of model output as designed by Funtowicz and Ravetz (1990) (Figure 6.1). In this diagram one can map model constituents (parameters, model input, model assumptions etc.) to find out how they contribute to the overall quality of the calculated model output.

The vertical axis of this diagram shows the relative contribution of uncertainty about a model constituent (e.g. a parameter value) to the total spread in model output. This relative contribution can be estimated using sensitivity analysis and Monte-Carlo-based uncertainty analysis. It should be noticed that this indicator is imperfect, because it is calculated with fixed values for all other parameters. It cannot be ruled out that the sensitivity of model output to uncertainty in one parameter will change if other values are taken (within the bounds of their uncertainty ranges) for the other fixed parameters. This implies that from a perfectionist point of view, an analysis of the sensitivity of the outcome to the uncertainty in one variable, provided the other parameters have fixed values, should be followed by an analysis of the *sensitivity of the sensitivity* to the uncertainties in the values of the other parameters. Deterministic and stochastic dependencies between parameters, in particular co-variances and conditional distributions (histograms), should also be assessed. The more parameters a model has, the more computing time it will take to do this meta-sensitivity analysis systematically. If after such a meta-sensitivity analysis the ’sensitivity of the sensitivity’ turns out to be high, it will be more and more difficult to disentangle the uncertainty problem in such a way that you really can attribute a certain percentage of the error in model output to the error in each single parameter. Instead, it might be more adequate to attribute it to, for instance, a joint-distribution of a cluster of parameters. This is an important methodological problem that is not discussed in Funtowicz and Ravetz’ (1990) book but certainly deserves attention. A solution could be to search for another measure of performance for the vertical axis, one that is more unequivocally attributable to the uncertainties in individual model components. However, it is doubtful whether such a measure will exist in all cases, especially in complex coupled non-linear models and in cases where parameters have joint-distributions. As a first approximation and despite the imperfection of this measure, the relative-contribution-to-spread, using fixed values for all other parameters is at least an objective measure for disentangling the uncertainty problem.

The horizontal axis maps the strength or quality of the model constituents. For well-established physical constants, such as the Stefan-Boltzmann constant, the strength is very close to one. Parameters whose value is just an educated guess, such as the carbon dioxide fertilization parameter in the first generations of carbon cycle models, the strength will be low, e.g. 0.1. Mapping all model constituents in the assessment diagram reveals the weakest links in the model. It also helps in the setting of priorities for model improvement. But the most important advantage of this diagram is that it enables us to distinguish between the part of the uncertainties in model outcome which is solvable (by increasing the strength of the model constituents by further research) and the part which is intrinsic to the modelled system or otherwise unsolvable. This distinction is important when designing strategies to cope with the uncertainties. With respect to the intrinsic uncertainties, the challenge is to design response strategies that are robust against these uncertainties. On the other hand, the uncertainties due to lack of quality of the model constituents will not be dispelled without adequate research programs.

An estimate of the time-frame needed to jack up the quality to the required level is also needed. Note that the required level of quality for each model constituent is a function of the relative contribution to spread in model output caused by the uncertainty in that constituent, as can be seen from Figure 6.1. If the estimated time-frame to attain the required quality for a model constituent is too long or if the costs are unacceptable, the challenge is to design response strategies that are robust against these uncertainties.

The NUSAP methodology designed by Funtowicz and Ravetz (1990) provides a good starting point to identify the strength or quality of model constituents. NUSAP is a notational scheme for scientific information. It is designed to act as a heuristic for good scientific practice and as a system for expressing and communicating uncertainties. It consists of five qualifiers: **N**umeral, **U**nit, **S**pread, **A**ssessment and **P**edigree. The last three qualifiers address the various aspects of uncertainty:

- *Spread* conveys an indication of the inexactness;
- *Assessment* expresses a judgement on the reliability and indicates the *strength* of the data;
- *Pedigree* conveys an evaluative account of the production process of the information, and indicates the *scientific status* of the knowledge.

Pedigree is expressed in a set of evaluation criteria, a so-called pedigree matrix. Depending on its application a pedigree matrix consists of a set of suitable evaluation criteria (e.g. peer acceptance), and defines modes of these criteria (e.g. low, high) which are coded hierarchically. Evaluation criteria used in a pedigree matrix are in fact yard-sticks for quality. These yard-sticks can be cognitive (e.g. theoretical structure) or social (e.g. peer acceptance). What criteria should be included in a pedigree matrix depends on the portion of information whose pedigree has to be determined. Examples of pedigree matrices are given in Table 6.5. Note that the columns are independent of each other.

Table 6.5a The pedigree matrix for research as designed by Funtowicz and Ravetz (1990).

Code	Theoretical Structure	Data input	Peer acceptance	Colleague consensus
4	Established theory	Experimental data	Total	All but cranks
3	Theory-based model	Historic/Field data	High	All but rebels
2	Computational model	Calculated data	Medium	Competing schools
1	Statistical processing	Educated guess	Low	Embryonic field
0	Definitions	Uneducated guess	None	No opinion

Table 6.5b The pedigree matrix for environmental models as designed by Funtowicz and Ravetz (1990).

Code	Model structure	Data input	Testing
4	Comprehensive	Review	Corroboration
3	Finite-element approximation	Historic/field	Comparison
2	Transfer function	Experimental	Uncertainty analysis
1	Statistical processing	Calculated	Sensitivity analysis
0	Definitions	Expert guess	None

Table 6.5c The pedigree matrix for radiological data entries (Funtowicz and Ravetz, 1990)

Code	Type	Source	Set-Up
4	Constants	Reviewed	Universal
3	Deduced	Refereed	Natural
2	Estimated	Internal	Simulated
1	Synthesized	Conference	Laboratory
0	Hypothetical	Isolated	Other

The choice of the quality criteria in a pedigree matrix, the choice of the quality modes in each column and the ranking of quality modes within one column, are all open to discussion. This makes it desirable to search for intersubjective procedures to establish pedigree matrixes. Also, the determination of the pedigree scores of a portion of information is not trivial. In the following we will discuss each of these methodological problems.

It is quite a complicated and not self-evident matter to select adequate quality modes for each column and to rank their quality from low to high. We will illustrate this with the following example. For *theoretical structure* Funtowicz and Ravetz propose the quality modes (we put them in reverse order:)

0. Definition
1. Statistical processing
2. Computational model
3. Theory-based model
4. Established Theory

We tend to interpret this ranking also as a linear path through which research usually progresses. From that point of view we have difficulties with the mode 'definition' and we do not know the precise difference between 'statistical processing' and 'computational model'. A typical evolution over time of a *theoretical structure* of a model constituent, resulting from research, might look something like this:

[*mode 0*] The first stage is, for instance, a (theory driven or empirically driven) notion that there is a correlation between quantities (e.g. global CO_2 concentration and global plant growth), but we have insufficient knowledge about the precise nature of the correlation in both a qualitative (that is: theoretical) and a quantitative sense (either empirical or theoretical & empirical).

[*mode 1*] The notion of mode 0 is extended with empirical data. Statistical analysis of the data supports the notion.

[*mode 2*] A quantitative input-output 'black box' model is constructed that fits with the empirical data. Theory that explains from high process-detail the nature of the mathematical relations in the input-output model is lacking or is rather incomplete.

[*mode 3*] The mechanisms that constitute the correlation between the quantities are understood well enough to construct a theory-based model with high process-detail: the black box from mode 2 is now transparent. However, more empirical data are needed to quantify certain parameters in the model with high process-detail. Thorough verification and testing are still required.

[*mode 4*] A theoretically and empirically sound high process detail model, free of tricky model fixes and sufficiently tested, has been achieved. Often this model is simplified to a straightforward meta-model (which the users-community can use as a black-box model).
The resulting set of modes of *theoretical structure* is:

0. Notion
1. Statistically indicated
2. Black-box model
3. Theory based model
4. Established theory

[5. simple meta-model - backed by established theory]

This ranking of the indicated modes gives rise to a few difficulties that we would like to discuss. First, the evolution of a portion of scientific information over time might not proceed linearly from mode 0 to mode 4. A theoretically based model can precede statistical indication and black-box models; e.g. the paths 0-3-4 or 0-3-1-4 are also conceivable. If we assume that quality of the knowledge about a model constituent improves with time, due to increasing research on that constituent, then we have a ranking problem for the quality modes of the quality-yardstick in this example.

Second, the ranking of the modes depends strongly on the use (and the user) of the scientific information concerned. When tested in our interviews, the hierarchy in the columns of Funtowicz and Ravetz' pedigree matrix for environmental models (Table 6.5b) proved to be controversial: according to Alcamo (1994b), there is no 'good' or 'bad' model structure. His alternative ranking codes are given between brackets in Table 6.6.

Table 6.6 The pedigree matrix for environmental models as designed by Funtowicz and Ravetz (1990). Between brackets: alternative ranking codes, attributed by Alcamo, a model-builder (Alcamo, 1994b).

Model structure		Data input		Testing	
Comprehensive	(1)	Review	(1)	Corroboration	(2)
Finite-element approximation	(1)	Historic/field	(1)	Comparison	(1)
Transfer function	(1)	Experimental	(1)	Uncertainty analysis	(1)
Statistical processing	(1)	Calculated	(1)	Sensitivity analysis	(1)
Definitions	(1)	Expert guess	(0)	None	(0)

Another difficulty related to the ranking of quality modes in a column, is that someone who builds integrated assessment models would prefer a working black-box model to a scientifically sound theoretically based model with insufficient empirical data to quantify the key parameters. However, a scientist from a deterministic school would consider any "black box" relation inferior to even poorly developed scientific theory on that relation; even if the black-box performs better in the light of available empirical data.[1] The nature of science is to open black-boxes.

It is not surprising that model-builders use other criteria to evaluate model quality than do Funtowicz and Ravetz. The differences can be understood from different roles in the process of quality control, as we discussed in section 6.4 (see Table 6.3). Being philosophers, Funtowicz and Ravetz attempted to address the quality of science-based claims as such, whereas an IAM-builder has to fulfil the needs of policy-makers without compromising too much the scientific credibility of the model. This results in usefulness as the main quality criterion (Mermet and Hordijk, 1989; Swart, 1994a).

A second methodological problem is associated with the drafting of pedigree scores for IAMs and their sub-models. With respect to model structure it is obvious that a black-box model has a lower scientific status (if any at all) than a model that is completely governed by established physical laws and has high process detail. However, if a very simple meta model is derived from, and secured by, a complex model with high process detail, the simple meta model can be equally good to model the process. For these cases we propose to apply the pedigree matrix to the mother model while taking into account the consequences of the simplifications in the meta model. One such consequence can be that the interaction of the simple models in the integrated framework results in other dynamics than that would result from the interaction of the complex models that back them.

The above discussion shows that the measurement of the strength or quality of model constituents is not trivial and can be a function of the perspective taken by the actor that uses the model, even with regard to single dimensions of the quality hyper-space constituted by all conceivable quality criteria.

A third problem in the operational use of the assessment diagram is the inherent impossibility of objectively aggregating the scores for a set of quality evaluation criteria to a single number between zero and one which represents strength as plotted along the horizontal axis of the assessment diagram. We will show that these difficulties do not detract in any way from the value of the assessment diagram as a heuristic tool in model evaluation and priority setting for research.

In their book Funtowicz and Ravetz (1990, chapter 12) elegantly show how the diagram can be applied to a radiological model of milk contamination with Caesium 137, for which they used *inter alia* the pedigree matrixes given in Tables 6.5b and 6.5c. The model however is extremely simple when compared to for instance the IMAGE 2 model. The file of IMAGE 2 with input-data is about 20 megabytes (personal communication Rik Leemans, October 1996). It is an impossible task to map all the constituents of a complex IAM such as IMAGE 2 in the same way in the assessment diagram. And it could be a thousand-year research project to map all constituents of a coupled ocean-atmosphere general circulation model in the assessment diagram. However, when the application of the assessment diagram is combined with expert judgement in selecting what should be mapped, it can be a valuable tool, helping in quality control, model improvement, communication of model uncertainties, and, most important, helping to distinguish between

[1] Compare this to a medicine that is effective in curing a certain disease, although scientists do not know why it works. The physician will be satisfied with it and use it in her or his practice, whereas the medical researcher will not be satisfied.

limitations and lack of quality. By providing a sound basis for this distinction, the method can generate key input for the design of response strategies, as we argued earlier in this section.

As an example, we will now sketch a possible procedure by which the assessment diagram from Figure 6.1 can be used to evaluate the quality of an outcome of an IAM. A good example of such an outcome are the 'safe emission corridors' calculated with the IMAGE 2 model (Alcamo and Kreileman, 1996; Swart *et al.*, 1996). In our example-procedure, objective information obtained from sensitivity and uncertainty analysis will be combined with systematically obtained, qualitative knowledge and tacit knowledge of experts and expert judgement. This information is used in a NUSAP-informed Delphic ranking exercise to determine the X-axis and Y-axis position in the assessment diagram of the model constituents that are to be mapped. The example is inspired by the methodology developed by Morgan and Keith (1995) for ranking key uncertainties in climate modelling.

Step 0 Select experts to form a panel. Make sure that the competence of the panel covers the whole domain of the model. Preferably, select experts who were not involved in the development of the model. It might be a good idea to include modellers from rival IAM-groups as well.

Step 1. Let the expert panel together with the modelling group select model constituents to be mapped in the assessment diagram. Instead of mapping every single parameter and model assumption of an IAM, one can start at a higher aggregation level and map, for instance, entire sub-models of the IAM.

Step 2. The modelling group carries out sensitivity analysis and Monte-Carlo-based uncertainty analysis to find out how much each constituent contributes to the total spread in model outcome. It is likely that information and resources are insufficient for a complete Monte-Carlo-based uncertainty analysis for each constituent. For this reason we added step 9.

Step 3. Choose (or design if necessary) adequate pedigree matrices to apply to each model constituent that is to be mapped.

Step 4. Let the panel determine the pedigree scores for each constituent.

Step 5. Make one information-card for each model constituent with its pedigree scores from step 4 and all available information from sensitivity and uncertainty analysis from step 2.

Step 6. Let each panel-member individually sort the cards according to strength. After sorting, let each member position the cards along a linear scale from 0 to 1.

Step 7. Determine the average scores and their standard deviations for all constituents and discuss the results at a plenary meeting of the panel. Pay special attention to scores with a high standard deviation. Try to get the reasons for eventual disagreement between the experts as clear as possible and record them.

Step 8 Allow re-ranking by individual members if they changed their mind after the plenary discussion. Now the X-axis scores are available for each constituent to be mapped in the assessment diagram.

Step 9. If the panel thinks the information of step 2 too incomplete to determine the Y-axis scores of each constituent straightforwardly, repeat steps 6 to 8 but let the members sort the cards according to their relative contribution to spread in the model outcome.

Step 10. Map the resulting average scores and their standard deviations in both dimensions, the latter as error bars, in the assessment diagram. The size of the error bars represents the disagreement among the members on the position of each constituent in the diagram.

Steps 4 and 9 can be further improved by adding debriefing sessions with key experts in the field for each constituent. The debriefing sessions can reveal tacit knowledge regarding (the quality of) that constituent, making the ranking-exercise better informed. To measure the social dimensions of quality such as 'peer acceptance', scientometric methods could be of help in determining the pedigree scores. However, scientometric methods are imperfect in measuring peer acceptance, and have been criticized for several methodological shortcomings.

The procedure can be used iteratively. After step 10, one has insight into the weakest constituents of the model - in view of the policy question addressed - and one can further disentangle the uncertainty problem by going back to step 1 and selecting model constituents at a lower aggregation level.

The NUSAP methodology can also be of help in the drafting of subjective probability distributions for Delphic Monte Carlo Analysis. In the current studies (e.g. Titus and Narayanan, 1996; Lutz *et al.*, 1996), the experts consulted are only asked to provide the first three qualifiers: a numeral, a unit and a distribution function. Adding the qualifiers *pedigree* and *strength* in Delphic Monte Carlo Analysis would make it possible to apply the assessment diagram to the outcomes of Delphic Monte Carlo Analysis as well. The relative contribution to spread would then be determined by analysing the sensitivity to the spread in the distribution functions, for instance by comparing model-outcome-distribution assuming half the spread and double the spread for each parameter distribution function. Adding such a quality analysis would remove some of the criticism concerning the use of subjective distribution functions.

6.8 Conclusions

In this chapter we have explored the problems of uncertainty management in Integrated Assessment Models (IAMs) of Climate Change. We have identified areas for improvement in uncertainty management in IAMs and we propose a methodology, based on the work by Funtowicz and Ravetz, for disentangling the uncertainty problem in IAMs. This methodology will enable us to assess the quality of the model results and to identify the weakest links in the models.

We conclude that:

i We have identified a mismatch between the types and sources of uncertainty that should be addressed on the one hand (Table 6.2) and the current practice of uncertainty management in IAMs and the tools available, on the other hand. From our analysis we conclude that the best covered areas of uncertainty analysis concern the *inexactness* in *input data* and *parameters*.

The other areas defined by table 6.2 get significantly less systematic attention in uncertainty analysis in IAMs.

ii We made an inventory of methodologies available to address different types and sources of uncertainty in models. We found that techniques currently available for uncertainty analysis and uncertainty treatment in IAMs have three major shortcomings:

1. They do not fully address all relevant aspects within the whole spectrum of types and sources of uncertainty;
2. They fail to provide unambiguous comprehensive insight for the modellers and the users into:
   a. the quality and the limitations of the IAM;
   b. the quality and the limitations of the IAM-answers to the policy questions addressed;
   c. the overall uncertainties;
3. They fail to systematically address the subjective component in the appraisal of uncertainties (with the partial exception of the cultural-theory-sampling method in the TARGETS model).

iii The areas of uncertainty analysis where improvement is most eligible are the assessment of error in model output that results from the *unreliability* (that is the lack of quality; not to be confused with the spread) of the knowledge about *input data*, *parameters*, and *model structure*, and in the quantitative assessment of error in model output due to uncertainty about *model structure*.

iv Stochastic modelling by Monte Carlo Simulation is needed to fully assess how uncertainties propagate through the model and to identify the distribution function of the outcome of an IAM which results from the distribution functions of the *input data*, the *model parameters* and the *model relations*. Monte Carlo Simulation is not commonly used in IAMs because of its resource-consuming character and because there is a lack of information about the distribution functions of all individual model constituents. In the case of the IMAGE-2 model we found that Monte-Carlo simulation was applied to individual sub-models only and these instances relied on (imperfect) subjective probability functions. This implies that error-propagation through the IAM has not yet been addressed.

v Building further on Funtowicz and Ravetz' (1990) NUSAP methodology and their Assessment Diagram, combined with an informed Delphic ranking procedure, we propose a 10-step iterative methodology to disentangle the uncertainty problem in IAMs. The methodology adds the quality-dimension, which makes it possible to discriminate between the potentially solvable and the currently unsolvable uncertainties. This information is crucial for the development of adequate response strategies. The response strategy has to be robust against the currently insoluble uncertainties, whereas adequate research programs need to be designed to reduce the potentially solvable uncertainties. The methodology also has the potential to assess the overall quality of model output and to identify the parts of the model whose individual weakness contributes most to the overall potential for the improvement of model quality. The latter information would be useful for setting research priorities.

vi Evaluation criteria for model quality used by model-builders differ from the criteria proposed by the philosophers Funtowicz and Ravetz becaue model-builders and philosophers play different roles in the critical evaluation of models. These different criteria are however no obstacle to using the NUSAP methodology as a checklist to identify weak parts of the model in terms of the applied criteria. For quality assessment of IAMs, further research is needed to identify suitable sets of evaluation criteria (these sets are called *pedigree matrices*) which will accommodate different critical roles and different assessment goals.

vii The methodology can also improve the drafting procedures that are used to obtain subjective probability distributions of parameters and input data used in Delphic Monte Carlo analysis of climate change and its impacts. The method adds information about the quality of each subjective distribution function. It can then be used to identify which subjective distribution functions need to be tackled first to improve the quality of the model output. The proposed method has not been tested so far. The sets of quality yardsticks (the so-called pedigree matrixes) and procedures for drafting adequate pedigree matrices and for determining pedigree scores for individual model constituents, will need to be standardized.

6.9 Acknowledgements

An earlier version of this chapter was published as a part of an IIASA-working paper (Van der Sluijs, 1996). We thank Jill Jäger, Joanne Linnerooth-Bayer, Yuri Ermoliev, Jerry Ravetz, Josee van Eijndhoven, Wim Turkenburg, Rob Swart, Anne Stijkel and Stephen Healy for making useful comments on the IIASA working paper and/or on earlier versions of this chapter. We are grateful to Lauren Hale and Sheila McNab for linguistic and stylistic improvements.

6.10 References

J. Alcamo and J. Bartnicki, A Framework for Error Analysis of a Long-Range Transport Model with Emphasis on Parameter Uncertainty, *Atmospheric Environment*, **21** (10), 1987, p. 2121-2131.

J. Alcamo (ed), IMAGE 2.0: Integrated Modelling of Global Climate Change, in: *Water, Air, and Soil Pollution*, **76**, Nos. 1/2, 1994a.

J. Alcamo, *personal communication* (interview) 21 November 1994b.

J. Alcamo and E. Kreileman, *The Global Climate System: Near Term Action for Long Term Protection*, RIVM, Bilthoven, 1996.

J.J. Batjes and C.G.M. Goldewijk, *The IMAGE 2 Hundred Year (1890-1990) Database of the Global Environment (HYDE)*, RIVM report no, 481507008, Bilthoven, 1994.

M.B. Beck, Water Quality Modelling: A Review of the Analysis of Uncertainty, *Water Resources Research*, **23** (8), 1987, p. 1393-1442.

M.B. Beck, Understanding Uncertain Environmental Systems, in: *Predictability and Nonlinear Modeling in Natural Sciences and Economics*, J. Grasman and G. van Straten (Eds.); Kluwer Academic Publishers: Dordrecht / Boston / London, 1994, p. 294-311.

M.B. Beck, J.R. Ravetz, L.A. Mulkey, and T.O. Barnwell, *On The Problem of Model Validation for Predictive Exposure Assessments,* 1996a.

M.B. Beck, L.A. Mulkey, T.O. Barnwell, and J.R. Ravetz, *Model Validation for Predictive Exposure Assessments*, EPA, 1996b.

M. Berk, *Integratie van NOP-onderzoek en het IMAGE model*, Report of a NRP Study-day, Utrecht, 22 January 1993.

S. Boehmer-Christiansen, Global Climate Protection Policy: the Limits of Scientific Advice, Part 1, *Global Environmental Change*. **4** (2), 1994a, p. 140-159.

S. Boehmer-Christiansen, Global Climate Protection Policy: the Limits of Scientific Advice, Part 2, *Global Environmental Change*. **4** (3), 1994b, p. 185-200.

S.A. Boehmer-Christiansen, A Scientific Agenda for Climate Policy?, *Nature*, **372**, 1994c, p. 400-402.

S.A. Boehmer-Christiansen, Comments on S. Shackley and B. Wynne Paper "Global Climate Change: the Mutual Construction of an Emergent Science-Policy Domain", in: *Science and Public Policy*, **22** (6), 1995, p.411-416.

H. Brooks, The Typology of Surprises in Technology, Institutions and Development, in: W.C. Clark (ed.), *Sustainable Development of the Biosphere*, Cambridge University Press, New York, 1986, p.325-348.

J.P. Bruce, H. Lee and E.F. Haites (eds.), *Climate Change 1995 : Economic and Social Dimensions of Climate Change*, contribution of Working Group III to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996, 448 p.

J. Casti, What if..., in: *New Scientist*, 13 July 1996.

J. Casti, *Would-Be Worlds*, John Wiley & Sons, 1996.

W.C. Clark and G. Majone, The Critical Appraisal of Scientific Inquiries with Policy Implications, in: *Science Technology and Human Values*, **10** (3), 6-19, 1985.

E. Cook, *Lifetime Commitments: Why Climate Policy-makers can't Afford to Overlook Fully Fluorinated Compounds*, World Resources Institute, 1995, 16 pp.

N.C. Dalkey, *Delphi*, RAND Corporation paper P-3704, Rand Corporation, Santa Monica (CA), 1967.

H. de Boois and J. Rotmans, Overzichtsmodel van de CO_2-Problematiek, in: *Berichten uit het RIVM 1985*, Bilthoven, 1986.

M. Douglas and A. Wildavsky, *Risk and Culture: An Essay on the Selection of Technological and Environmental Dangers*, University of California Press, Los Angeles, 1982, 221 pp.

J. Emsley (ed.), *The Global Warming Debate, The Report of the European Science and Environment Forum*, Bourne Press, Dorset, 1996, 288 pp.

Energy Modeling Forum, *Final First Round Study Design for EMF 14: Integrated Assessment of Climate Change*, Energy Modeling Forum, Stanford University, Stanford, 1994a.

Energy Modeling Forum, *Model Results*, Stanford University, Stanford, 1994b.

Energy Modeling Forum, *Second Round Study Design for EMF 14: Integrated Assessment of Climate Change*, Energy Modeling Forum, Stanford University, Stanford, 1995a.

Energy Modeling Forum, *Second Round Scenario Results*, Energy Modeling Forum, Stanford University, Stanford, 1995b.

Environmental Resources Limited, *Handling Uncertainty in Environmental Impact Assessment*, Environmental Resources Limited, London, 1985.

Yu. Ermoliev and J.B. Wets, *Numerical Techniques for Stochastical Optimisation*. Springer Verlag, Berlin, 1988, 571 pp.

W. Fransen (ed.) and A. Reuvekamp, *Probabilities of Climatic Change, a Pilot Study*, Koninklijk Nederlands Meteorologisch Instituut, Scientific Report WR 95-08, De Bilt, 1995, 147 pp.

S.O. Funtowicz and J.R. Ravetz, *Uncertainty and Quality in Science for Policy*, Kluwer, Dordrecht, 1990.

S.O. Funtowicz and J.R. Ravetz, Science for the Post Normal Age, *Futures*, September 1993, p. 739-755.

C. Genest and J.V. Zidek, Combining Probability Distributions: A Critique and an Annotated Bibliography, *Statistical Science*, **1**, 1986, 114-148.

O. Giarini and W.R. Stahel, *The Limits to Certainty, Facing Risks in the New Service Economy*, 2nd revised edition, Kluwer Academic Publishers, Dordrecht, 1993.

M.C.G. Hall, Estimating the Reliability of Climate Model Projects, Steps towards a Solution. In: *The Potential Climate Effects of Carbon Dioxide*, M.C. MacCracken and F.M. Luther (eds), US-DOE/ER-0237, Washington D.C., 1985, p. 337-364.

J. Hansen, A. Lacis, R. Ruedy and M. Sato, Potential Climate Impact of the Mount Pinatubo Eruption. Geophysical Research Letters, **19**, 1992, p. 215-218.

G.P. Hekstra, The Case for a Global CO_2-Emission Ceiling. Paper presented at *UNEP/ECE Regional Seminar on Alternative Patterns of Development and Lifestyles*, Ljubljana, 3-8 December 1979, 10 pp.

T. Hellström, The Science-Policy dialogue in transformation: model-uncertainty and environmental policy, in: *Science and Public Policy*, **23** (2), 1996, p.91-97.

J.C. Helton, Treatment of Uncertainty in Performance Assessments for Complex Systems, *Risk Analysis*. **14** (4), 1994, p. 483-???

M. Henrion and B. Fischhoff, Assessing the Uncertainty in Physical Constants, *American Journal of Physics*, **54** (9), p.791-798.

R.C. Hilborn, *Chaos and Non-Linear Dynamics*, Oxford University Press, Oxford, 1994, 654 pp.

J. Hirshleifer and J.G. Riley, *The Analytics of Uncertainty and Information*, Cambridge University Press, Cambridge, 1992, 465 pp.

F.O. Hoffman and J.S. Hammonds, Propagation of Uncertainty in Risk Assessments: The Need to Distinguish Between Uncertainty Due to Lack of Knowledge and Uncertainty Due to Variability, *Risk Analysis*, **14** (5), 1994, p. 707-712.

C.S. Holling, The Resilience of Terrestrial Ecosystems: Local Surprise and Global Change, in: W.C. Clark and R.E. Munn (eds.) *Sustainable Development of the Biosphere*, Cambridge University Press, 1986, p. 292-317.

L. Hordijk (ed.), *Report International Review Meeting IMAGE 2.0 11-13 January 1993 Amsterdam*, NRP report 00-09, Bilthoven, 1993.

L. Hordijk, *personal communication* (interview), 15 November 1994.

J.T. Houghton, G.J. Jenkins, and J.J. Ephraums (eds.), *Climate Change, The IPCC Scientific Assessment*, Cambridge University Press, 1990, 365 pp.

J.T. Houghton, L.G. Meira Filho, B.A. Callander, N. Harris, A. Kattenberg, and K. Maskell (eds.), *Climate Change 1995, The Science of Climate Change*, Contribution of WGI to the Second Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, 1996.

P.H.M. Janssen, W. Slob, and J. Rotmans, *Sensitivity Analysis and Uncertainty Analysis: A Statement of Affairs of Ideas, Methods and Techniques*, National Institute of Public Health and the Environment (RIVM) report no. 958805001, Bilthoven, The Netherlands, 1990 (in Dutch).

P.H.M. Janssen, P.S.C. Heuberger, and R. Sanders, UNCSAM: a Tool for Automating Sensitivity and Uncertainty Analysis, in: *Environmental Software*, **9**, 1-11, 1994.

C.E. Junge, Sulphur in the Atmosphere, *Journal of Geophysical Research*, **65**, 1950, p. 227-237.

R.W. Kates and W.C. Clark, Expecting the unexpected, in: *Environment*, **38** (2), 1996, p. 6-11 and 28-34.

B. Kasemir, C.C. Jaeger, and G. Dürrenberger, Embedding Integrated Assessment Models in Social Discourse, in: *Science and Public Policy*, **23** (2), 1996, p. 124-125.

B. Keepin and B. Wynne, Technical Analysis of IIASA energy scenarios, in: *Nature*, **312**, 1984, p. 691-695.

N. Keilman, Ex-Post Errors in Official Population Forecasts in Industrialized Countries, *Journal of Official Statistics*, (accepted for publication; forthcoming).

D.W. Keith, When is it Appropriate to Combine Expert Judgements? An Editorial Essay, in: *Climatic Change*, **33** (2), 1996, p.139-143.

M.P. Kolomeev, S.A. Nikonov, O.S. Sorokovikova, and S.S. Khmelevtsov, Simulation of the Nothern Hemisphere Climate Response to the Eruption of Mount Pinatubo, Russian Meteorol. Hydrol, **4**, 1993, p. 9-13 (referred to in Parker *et al.*, 1996).

L.F. Konikow and J.D. Bredehoeft, Ground-water Models cannot be Validated, in: Advances in Water Resources, **15** (2), 1992, p.75-83.

M.S. Krol and H.J. van der Woerd, Uncertainty Analysis for the Computation of Greenhouse Gas Concentrations in IMAGE, in: J. Grasman and G. van Straten (eds), *Predictability and Nonlinear Modeling in Natural Sciences and Economics*, Kluwer, Dordrecht, 1994.

H.W. Lewis, R.J. Budnitz, A.W. Castleman, D.E. Dorfan, F.C. Finlayson, R.L. Garwin, L.C. Hebel, S.M. Keeny Jr, R.A. Muller, T.B. Taylor, G.F. Smoot and F. von Hippel, Report to the American Physical Society by the Study Group on Light-Water Reactor Safety, *Reviews of Modern Physics*, **47**, suppl. No. 1, 1975, p. s1-s125.

S. Lichtenstein, B. Fischhoff and L. Phillips, Calibration of Probabilities, in: D. Kahnemann, P. Slovic, and A. Tversky (Eds.), *Judgment Under Uncertainty: Heuristics and Biases*, Cambridge University Press, Cambridge, 1982.

L. Lunde, *Science or Politics in the Greenhouse? A Study Towards Scientific Consensus on Climate Change*, Report no. 1991/8, Fridtjof Nansen Institute, Lysaker, 1991.

W. Lutz, W. Sanderson, and S. Scherbov, *Probabilistic World Population Projections Based on Expert Opinion,* IIASA working paper WP-96-17, Laxenburg, February 1996, 27 pp.

L. Margulis and J.E. Lovelock, Biological Modulation of the Earth's Atmosphere, in: *Icarus*, **21**, 1974, p. 471-489.

M.P. McCormick, L.W. Thomason, and C.R. Trepte, Atmospheric Effects of the Mt Pinatubo Eruption, in: *Nature*, **373**, 1995, p. 399-404.

M.D. McKay, R.J. Beckman, and W.J. Conover, A Comparison of Three Methods for Selecting Values of Input Variables in the Analysis of Output from a Computer Code, in: *Technometrics*, **21** (2), 1979, 239-245.

M.G. Morgan and M. Henrion, *Uncertainty, A Guide to Dealing with Uncertainty in Quantitative Risk and Policy Analysis*, Cambridge University Press, 1990.

M.G. Morgan and D.W. Keith, Subjective Judgements by Climate Experts', in: *Environmental Science and Technology*, **29**, 1995, p.468A-476A.

D.E. Parker, H. Wilson, P.D. Jones, J. Christy, and C.K. Folland, The Impact of Mount Pinatubo on World Wide Temperatures, *International Journal of Climatology*, **16**, 1996, p.487-497.

Property Claim Services, *Hurricane Record 1949-1995, Property Claim Services Report Prepared for International Institute for Applied Systems Analysis*, Property Claim Services (A division of the American Insurance Services Group, Inc.), 1996.

J. Ravetz, Usable Knowledge, Usable Ignorance: Incomplete Science with Policy Implications, in: W.C. Clark (ed.), *Sustainable Development of the Biosphere*, Cambridge University Press, New York, 1986, p.415-432.

RMNO, *Onderzoek in Nederland naar de gevolgen van toenemende CO_2 en andere sporegassen in de atmosfeer door menselijke activiteiten.* RMNO, Rijswijk, 1984, 48 pp.

J. Rotmans, *personal communication* (interview), 9 november 1994.

W.D. Rowe, Understanding Uncertainty, *Risk Analysis*, **14** (5), 1994, p. 743-750.

D. Schimmelpfennig, Uncertainty in Economic Models of Climate-Change Impacts, in: *Climatic Change*, **33** (2), 1996, p. 213-234.

C.J.E. Schuurmans, H.M. van den Dool, and J.A. Wisse, Luchtverontreiniging en wereldklimaat (1) *Zenit*, **7** (1), 1980, p. 8-13.

M. Schwartz and M. Thompson, *Divided we Stand: Redefining Politics, Technology and Social Choice*, Harvester Wheatsheaf, London, 1990, 176 pp.

Science and Policy Associates, inc., *Dutch National Research Programme On Global Air Pollution and Climate Change, NRP Phase I Final Review Report*, Science and Policy Associates, inc., Washington D.C., March 1995.

S. Shackley and B. Wynne, Global Climate Change: the Mutual Construction of an Emergent Science-Policy Domain, in *Science and Public Policy*, **22** (4), 1995, p.218-230.

S. Shackley, J. Risbey, P. Stone and B. Wynne, Adjusting to Policy Expectations in Climate Change Modeling: An Interdisciplinary Study to Flux Adjustments in Coupled Atmosphere-Ocean General Circulation Models, (June 1996 draft of forthcoming paper, CSE, University of Lancaster).

A.M. Solomon, *Report of International Review Meeting IMAGE 2.0,* 20-22 June 1994 Amsterdam, The Netherlands, Dutch National Research Program on Global Air Pollution and Climate Change (NRP) Report 00-09, 1994.

R. Swart, *Climate Change: Managing the Risks*, (Thesis), Free University Amsterdam, 1994a.

R. Swart, *personal communication* (interview), 23 November 1994b.

R. Swart, M. Berk, E. Kreileman, M. Janssen, J. Bollen, R. Leemans, and B. de Vries, *The Safe Landing Approach: Risks and Trade-offs in Climate Change*, RIVM, Bilthoven, 1996.

J.G. Titus and V. Narayanan, The Risk of Sea Level Rise, A Delphic Monte Carlo Analysis in which Twenty Researchers Specify Subjective Probability Distributions for Model Coefficients within their Respective Areas of Expertise, in: *Climatic Change*, **33** (2), 1996, p.151-212.

R.S.J. Tol (ed.), *Integrated Assessment of Climate Change, Report EMF14 Meeting 2,* IvM, Free University of

Amsterdam, 1994.

R.S.J. Tol (ed.) *Integrated Assessment of Climate Change, Report EMF14 Meeting 3*, IvM, Free University of Amsterdam, 1995

F.L. Toth, Practice and Progress in Integrated Assessments of Climate Change, A Workshop Overview, in: *Energy Policy*, **23** (4/5), 1995, p. 253-267.

A. Trisoglio, *Plurality and Political Culture, A Response to Thompson and Drake*, paper Presented at workshop "Risk, Policy and Complexity", IIASA, Laxenburg, 7-9 August 1995, 18 pp.

M.B.A. van Asselt and J. Rotmans, *Uncertainty in Integrated Assessment Modelling: a Cultural Perspective Based Approach,* National Institute of Public Health and Environmental Protection (RIVM), report no. 461502009, Bilthoven, 1995.

M.B.A. van Asselt and J. Rotmans, Uncertainty in Perspective, in: *Global Environmental Change, Human and Policy Dimensions*, **6** (2), 1996, p. 121-157.

M.B.A. van Asselt, A.H.W. Beusen, and H.B.M. Hilderink, Uncertainty in Integrated Assessment, A Social Scientific Perspective, in: *Environmental Modeling and Assessment*, **1** (1), 1996, p. 71-97.

J.P. van der Sluijs, Uncertainty Management in Integrated Modeling, the IMAGE case, in: S. Zwerver, R.S.A.R. van Rompaey, M.T.J. Kok, and M.M. Berk (eds.), *Climate Change Research; Evaluation and Policy Implications*, Studies in Environmental Science 65 B, Elsevier Science B.V., 1995.

J.P. Van der Sluijs, *Integrated Assessment Models and the Management of Uncertainties*, IIASA Working Paper no. WP-96-119, Laxenburg, October 1996, 80 pp.

W.E. Vesely and D.M. Rasmuson, Uncertainties in Nuclear Probabilistic Risk Analyses, in: *Risk Analysis*, **4**, 313-322, 1984.

T.S. Wallsten, Measuring Vague Uncertainties and Understanding their use in Decision Making, in: G.M. von Furstenberg (ed.) *Acting under Uncertainty, Multidisciplinary Conception*, Kluwer Academic Publishers, Dordrecht, 1990.

W.G. Wang, Y.L. Yung, A.A. Lacis, T. Mo, and J.E. Hansen, Greenhouse effects due to man-made perturbations of trace gases, *Science*, **194**, 1976, p.685-690.

P. Westbroek and G. Muyzer, Modeling the Biosphere, *Terra Nova*, **4**, 1992, p. 539-541.

B. Wynne, Uncertainty and Environmental Learning, in: *Global Environmental Change*, **2**, 1992, p.111-127.

Chapter 7

Major conclusions of this thesis

The overall scientific objective of this dissertation was to gain insight into the processes by which assessments of the risks of anthropogenic climate change are constructed and more specifically into the way in which uncertainty management is conducted within these processes. This objective has been operationalized in the following research questions:

A. How has consensus been achieved and sustained regarding key elements in the assessments against a background of progressing scientific understanding, a growing body of climatic data, huge uncertainties and unresolved scientific puzzles surrounding the climate issue? (addressed in chapters 2 and 3)

B. 1. What are the different types and sources of uncertainties and their peculiarities? (addressed in chapters 4, 5 and 6)

   2. How have uncertainties been handled in the processes by which a scientific basis for the climate policy debate has been constructed? (addressed in chapters 2, 3, 5 and 6)

C. How can the management of uncertainties in the *post-normal* assessment practice be improved? (addressed in chapter 6)

In the following we summarize our conclusions:

A. Conclusions regarding the achievement and maintenance of consensus:

A.1 We found that the estimate of 1.5°C-4.5°C for climate sensitivity to CO_2-doubling has not changed in policy-makers' summaries of climate risk assessments over the past two decades in spite of the fact that climate research and scientific knowledge have expanded enormously and climate models have become much more complex. The stability of the 1.5°C to 4.5°C temperature range is all the more surprising because we found that the ranges of individual estimates for climate sensitivity reported in the full scientific texts of the assessments did in fact change in this period (Chapter 2).

A.2 We empirically identified a repertoire of sources from which the experts managed to acquire flexibility in maintaining consensus on the estimate of 1.5°C to 4.5°C for climate sensitivity to CO_2-doubling, without ignoring changing scientific ideas or losing credibility. The changing science was absorbed by the subtle deconstruction and reconstruction (mostly tacit and implicit) of the argumentative chains that link data, expert interpretation and policy meaning. More specifically this happened by means of:

- changes in the mode of reasoning;
- changes in the types of uncertainty taken into account;
- changes in the best estimate rather than the high and low ends of the temperature range;
- subtle implicit changes in the definition of climate sensitivity;
- subtle implicit changes in the connotation and meaning of the temperature range.

A.3 The estimate of 1.5°C to 4.5°C for climate sensitivity to CO_2-doubling acts as a highly aggregated simplified multivalent consensus summary of climate science, interfacing science and policy (Chapter 2).

A.4 The maintained consensus about the temperature range for climate sensitivity operates as an *anchoring device* in science for policy, helping to hold together a variety of social worlds (scientific disciplines, policy-makers). It can exist only as a result of an implicit social contract among the various scientists and policy-specialists involved, which allows 'the same' temperature range to accommodate tacitly different local meanings. Our findings indicate that scientific consensus can be a much more multi-dimensional and problematic concept than a simple agreement based on shared beliefs and uniform interpretations (Chapter 2).

A.5 From our study of closure in risk assessment in the international arena and the Netherlands arena, and of the diffusion of insights between the arenas, it follows that - in the case we investigated - it was not the paradigmatic predisposition of the experts that was decisive for the outcome of the (contingent) deconstruction and reconstruction process of claims made in the assessment reports, but it was the context in which the experts operated and the commitments they had made (Chapter 3).

B. Conclusions regarding different types and sources of uncertainties and their peculiarities and how uncertainties are handled in climate risk assessments:

B.1 In the assessment community there is a growing realization that research does not necessarily reduce the overall uncertainties regarding future climate. However, some uncertainties with regard to some aspects of the climate system and its dynamics have been and can be reduced (for instance, the uncertainty about the stability of the West Antarctic Ice Sheet). Ongoing research is also revealing unforeseen complexities in the climate system and novel uncertainties (for instance, the uncertainty about the sensitivity of the large-scale thermohaline circulation in the oceans - the so-called *conveyer-belt* - to climate change) (Chapters 1, 2 and 6).

B.2 Man's knowledge and understanding of the modelled causal chain of climate change are incomplete. Climate change is characterized by large uncertainties and the risk of climate change cannot be reliably assessd. In each stage of the causal chain there are both potentially reducible and probably irreducible uncertainties that affect estimates of the future states of key variables and the future behaviour of system constituents. The potentially reducible parts stem from incomplete information, incomplete understanding, limited quality in data and model assumptions, and disagreement between experts. The probably irreducible parts stem from ignorance, epistemological limits of science, non-deterministic system elements, the practical unpredictability of chaotic system components, limits to our ability to know and understand, limits to our ability to handle complexity, the 'unmodelability' of surprise, non-smooth phenomena and from intransitive system components (that is: without a single stable state) due to the existence of more than one likely equilibrium solution of the (sub)system (chapters 1 and 5).

B.3 Integrated Assessment Models (IAMs) used in science for policy rely upon the Earth System Models (ESMs) that back them scientifically. Consequently, the accuracy of IAM assessments depends on the accuracy of the ESMs on which they draw (Chapter 5). One of the major shortcoming of General Circulation Models is that biotic feedbacks are only included in a very symplified form (Chapters 2 and 5). The inclusion of the biota in ESMs can dramatically change not only their quantitative behaviour, but also their qualitative behaviour: in our case, the inclusion of the biota could change the attractor of the system from an equilibrium point into a stable limit cycle.

B.4 The findings in chapter 4 suggest that the hitherto widely believed but untested prejudgment that the biota in all cases stabilize the earth system is not valid. The study in chapter 4 provides a plausible case of feedback via biogenic carbonate precipitation in the oceans, which destabilizes the global climate naturally.

B.5 The IAMs currently available do not really integrate the entire causal chain, nor do IAMs take dynamically into account all feedbacks and linkages between the different stages of the causal chain (chapter 5).

B.6 The state of science that backs the (mono-disciplinary) sub-models of IAMs differs across sub-models. In other words, the current climate IAMs consist of a mixture of constituents which covers a wide spectrum ranging from educated guesses to well-established knowledge (chapter 5).

B.7 There is a controversy about the usefulness of IAMs for the assessment of climate change. The positions in the debate vary from "We are not ready to do integrated modelling, we must wait until all science used in the model has the status of well-established knowledge" to "We have the responsibility to use our best scientific understanding to develop reasonable policies. Integrated modelling is the best way of combining our knowledge in such a way that we can evaluate the consequences of different policy scenarios, do cost-benefit framing or optimize cost effectiveness to reach a target."

There is however agreement that IAMs are not truth-machines and cannot reliably predict the future, but are heuristic tools. IAMs are capable of testing sensitivity, of answering ’what if’ questions (although each answer has to be followed by ", given the total set of assumptions of this model"), of ranking policy options, of assessing the relative importance of uncertainties, of identifying research priorities and of providing insights that cannot easily be derived from the individual natural or social science component models that have been developed in the past (chapter 5).

B.8 Despite the fact that some experts maintain that we are not ready for integrated assessment, the models are being used at present to directly address policy questions. For instance, they are being used to identify ’safe emission corridors’, which are presented to negotiators as answers rather than as insights. It is highly questionable whether such use is justifiable, unless all actors that deal with IAMs and IAM results are fully aware of the limitations and caveats of IAM assessments. These circumstances imply that there is an urgent need for uncertainty management, quality assurance, high standards of IAM practice, and a high awareness of the limitations of models (chapter 5).

B.9 We developed a two-dimensional classification scheme which comprises the type and the source of uncertainties in IAMs (see Table 7.1). This classification scheme defines areas to be addressed in uncertainty analysis and uncertainty management in IAMs.

Table 7.1 Areas to be addressed in uncertainty management in IAMs.

source	**type**	inexactness	unreliability	ignorance
input data				
conceptual model structure	parameters			
	relations (functional error)			
technical model structure	process error			
	resolution error			
	aggregation error			
	model fixes			
bugs	numerical error			
	software error			
	hardware error			
model completeness				

B.10 Using the above sketched classification scheme for uncertainty, we concluded that in the current IAM practice, the best covered areas of uncertainty analysis are in the upper left cells of the table, namely the *inexactness* in *input data* and *parameters*. The other areas defined by the table get significantly less systematic attention in uncertainty analysis in IAMs.

B.11 We made an inventory of methodologies available to address different types and sources of uncertainty in models. We found that techniques currently available for uncertainty analysis and uncertainty treatment in IAMs have three major shortcomings:

1. They do not fully address all relevant aspects within the whole spectrum of

types and sources of uncertainty;

2. They fail to provide unambiguous comprehensive insight for the modellers and the users into:
   a. the quality and the limitations of the IAM;
   b. the quality and the limitations of the IAM-answers to the policy questions addressed;
   c. the overall uncertainties;
3. They fail to systematically address the subjective component in the appraisal of uncertainties (with the partial exception of the cultural-theory-sampling method in the TARGETS model) (chapter 6).

B.12 The areas of uncertainty analysis where improvement is needed most are the assessment of error in model output that results from *unreliability* (that is the lack of quality; not to be confused with the spread) of the knowledge about *input data*, *parameters*, and *model structure*, and in the quantitative assessment of error in model output due to uncertainty about *model structure* (chapter 6).

B.13 Stochastic modelling by Monte Carlo Simulation is needed to fully assess the propagation of uncertainties through the model and to identify the distribution function of the outcome of an IAM which results from the distribution functions of the *input data*, the *model parameters* and the *model relations*. Monte Carlo Simulation is not commonly used in IAMs because of its resource-consuming character and because there is a lack of information about the distribution functions of all individual model constituents. In the case of the IMAGE-2 model we found that Monte-Carlo simulation was applied to individual sub-models only and these instances relied on (imperfect) subjective probability functions. This implies that propagation of uncertainties through the IAM has not yet been addressed (chapter 6).

C. Conclusions regarding how the management of uncertainties in the post-normal assessment practice can be improved:

C.1 Climate risk assessment is an instance of *post-normal science*, which means that it is conducted in a context of hard political pressure, values in dispute, high decision stakes and high epistemological and ethical systems uncertainties (chapter 1).

C.2 Because of the *post-normal* situation, scientific consensus about the truth of the risk of climate change is unlikely to be achieved. Consequently, we will have to abandon our unrealistic demand for a single certain truth and strive instead for transparency of the various positions and learn to live with pluralism in risk assessment. This means that uncertainties in climate risk assessment will have to be treated explicitly and communicated adequately to decision-makers (chapter 1).

C.3 Building further on Funtowicz and Ravetz' (1990) NUSAP methodology and their Assessment Diagram, combined with an informed Delphic ranking procedure, we propose a 10-step iterative methodology to disentangle the uncertainty problem in IAMs. The methodology adds the quality-dimension, which makes it possible to discriminate between the potentially solvable and the currently unsolvable uncertainties. This information is crucial for the development of adequate response strategies. The response strategy has to be robust against the currently insoluble uncertainties, whereas adequate research programmes need to be designed to reduce the potentially solvable uncertainties. The methodology also has the potential to assess the overall quality of model output and to identify the parts of the model whose individual weakness contributes most to the overall potential for the improvement of model quality. The latter information would be useful for setting research priorities (chapter 6).

C.4 Evaluation criteria for model quality used by model-builders differ from the criteria proposed by the philosophers Funtowicz and Ravetz becaue model-builders and philosophers play different roles in the critical evaluation of models. These different criteria are however no obstacle to using the NUSAP methodology as a checklist to identify weak parts of the model in terms of the applied criteria. For quality assessment of IAMs, further research is needed to identify suitable sets of evaluation criteria (these sets are called *pedigree matrices*) which will accommodate different critical roles and different assessment goals (chapter 6).

C.5 The methodology can also improve the drafting procedures that are used to obtain subjective probability distributions of parameters and input data used in Delphic Monte Carlo analysis of climate change and its impacts. The method adds information about the quality of each subjective distribution function. It can then be used to identify which subjective distribution functions need to be tackled first to improve the quality of the model output. The proposed method has not been tested so far. The sets of quality yardsticks (the so-called pedigree matrixes) and procedures for drafting adequate pedigree matrices and for determining pedigree scores for individual model constituents, will need to be standardized (chapter 6).

Curriculum Vitae

Jeroen Pieter van der Sluijs
Date of birth: October 25, 1965
Place of birth: Herwijnen, The Netherlands

After my secondary education at "De Lage Waard" in Papendrecht, I started studying chemistry at Leiden University in September 1983. I completed the propaedeutic examination in 1984. In 1986 my study was crudely interrupted by the military police and I spent nine months in prison as a conscientious objector of both military service and alternative civil service. After my release I continued my study and entered the research group geobiochemistry of Dr. Peter Westbroek at the University of Leiden. I investigated the role of the biota in the Carbonate-Silicate Geochemical Cycle (which is believed to act as a long term global thermostat), using a dynamic simulation model. This work led to my graduation in Theoretical Ecology ('vrij doctoraal examen') in May 1990.

In February 1991 I was appointed as a junior researcher at the Department of Science Technology and Society of Utrecht University, led by prof. dr. Wim Turkenburg. Under the supervision of Dr. Kornelis Blok, I carried out a comprehensive feasibility study to CO_2 removal from flue gases of coal fired power plants using polymer-membranes. In August 1991 I was appointed at the same department as PhD. candidate and started to investigate the assessment practice of the climate issue, with a focus on the Netherlands, under the supervision of prof. dr. Josee van Eijndhoven. This research work was embedded in the international research project "Social Learning in the Management of Global Environmental Risks", coordinated by prof. Dr. W.C. Clark, Harvard University. During my research I became more and more interested by the questions of uncertainty management in climate risk assessments. In the summer of 1996 I spent three months at the International Institute for Applied Systems Analyses (IIASA) in Austria, in the framework of the IIASA Young Scientists Summer Program. My research work there was on the question of uncertainty management in climate Integrated Assessment Models and was supervised by Dr. Jill Jäger. It contributed to the work of the IIASA Risk Policy and Complexity program led by Joanne Linnerooth-Bayer and Yuri Ermoliev. Both the work in the framework of the Social Learning project and the work at IIASA have resulted in the present thesis.

Research Publications

1. J.P. van der Sluijs, *Kernenergie en stralingsrisico's, controversen rond risicoschattingen en normstelling van blootstelling aan lage dosis ioniserende straling*, Wetenschapswinkel RUL, Leiden, 1988.

2. J.P. van der Sluijs, J.P., C.A. Hendriks, K. Blok, *Feasibility of polymer membranes for carbon dioxide recovery from flue gases* (Research report NW&S Nr.:92006), Utrecht, 1991.

3. S. Toet and J.P. van der Sluijs, *Climate Change: The Reporting in "De Telegraaf" 1970-1991*. January 1992, ver. 1, contribution F7 to the Social Learning project (Research report NW&S Nr:92014), Utrecht, 1992.

4. J.P. van der Sluijs, *Experts in the Netherlands and the risk of Global Climate Change, 1946 - present v.1 (working draft)*, February 1992, contribution F6 to the Social Learning project (Research report NW&S Nr:92012), Utrecht, 1992.

5. J.P. van der Sluijs, C.A. Hendriks, K. Blok, Feasibility of polymer membranes for carbon dioxide recovery from flue gases, *Energy Conversion and Management*, **33**, 1992, p. 429-436 (Proc. First Int. Conf. on Carbon Dioxide Removal).

6. J.P. van der Sluijs and G.J. de Bruyn, *Biogenic Feedbacks in the Carbonate-Silicate Geochemical Cycle and the Global Climate*, paper presented on the CO_2-Chemistry workshop, September 20-23 1993, Hemavan, Sweden, 1993.

7. J.P. van der Sluijs, Inertie in de beleidsgeorienteerde wetenschappelijke adviespraktijk betreffende het klimaatvraagstuk. Short paper in *SWOME marktdag boek 1994*, SISWO, Amsterdam. 1994, p. 49-51.

8. J.P. van der Sluijs, Uncertainty management in integrated modelling, the IMAGE case in: S. Zwerver, R.S.A.R. van Rompaey, M.T.J. Kok and M.M. Berk (eds.), *Climate Change Research; Evaluation and Policy Implications*, Studies in Environmental Science 65 B, Elsevier Science B.V., 1995, p.1401-1406.

9. J.P. van der Sluijs, Omgaan met onzekerheden in Geintegreerde Klimaatmodellen, Short paper in *SWOME marktdag boek Zesde Markdag, Milieu en Samenleving*, SISWO, Amsterdam, 1995, p. 68-70.

10. J.P. van der Sluijs, *Integrated Assessment Models and the Management of Uncertainties*, IIASA Workingpaper no. WP 96-119, Laxenburg, Austria, 1996, 80 pp.

11. J.P. van der Sluijs, G.J. de Bruyn, and P. Westbroek, Biogenic Feedbacks in the Carbonate-Silicate Geochemical Cycle and the Global Climate, *American Journal of Science*, **296** (8), 1996, p.932-953.

12. J.P. van der Sluijs, J.C.M. van Eijndhoven, B. Wynne, and S. Shackley, Anchoring Devices in Science For Policy: The Case of Consensus Around Climate Sensitivity, *Social Studies of Science* (accepted for publication; scheduled for the spring 1997 issue).

13. J.P. van der Sluijs and J.C.M. van Eijndhoven, Closure of Disputes in the Assessments of Climate Change in the Netherlands Arena (submitted to *Environmental Management*).

14. J.C.M. van Eijndhoven, G. Dinkelman, J.P. van der Sluijs and R. Pleune, Finding your Place,

A History of the Management of Global Environmental Risks in the Netherlands, in: The Social Learning Group, *Learning to Manage Global Environmental Risks: A Comparative History of Social Responses to Climate Change, Ozone Depletion and Acid Rain*. MIT Press (forthcoming)

15. J. Jäger, J. Cavender Bares, N. Dickson, A. Fenech, P. Haas, F. Hampson, P. Hughes, A. Liberatore, E.A. Parson, V. Sokolov, F. Toth, C. Waterton, J.P. van der Sluijs, J.C.M. van Eijndhoven, and B. Wynne, Risk Assessment, in: The Social Learning Group, *Learning to Manage Global Environmental Risks: A Comparative History of Social Responses to Climate Change, Ozone Depletion and Acid Rain*. MIT Press (forthcoming).

16. J. Jäger, *et al.*, Monitoring, in: The Social Learning Group, *Learning to Manage Global Environmental Risks: A Comparative History of Social Responses to Climate Change, Ozone Depletion and Acid Rain*. MIT Press (forthcoming).

Nawoord

Aan het slot van dit proefschrift wil ik de vele mensen bedanken die elk op hun eigen manier bijgedragen hebben aan dit proefschrift. Ik dank Josee van Eijndhoven voor haar deskundige inhoudelijke begeleiding, haar geduld, en haar onophoudelijke aansporingen om meer expliciet te plannen dan ik gewend was. Ik dank Wim Turkenburg voor zijn waardevolle heldere commentaar op mijn concepten, en vooral ervoor dat hij mij zo actief heeft geïnspireerd, me heeft aangemoedigd de ontwikkelingen aan het front van het klimaatveranderingsonderzoek nauwlettend te volgen, en me heeft geleerd om kritisch en scherp te blijven en overal vraagtekens bij te blijven plaatsen.

Ik dank alle studenten die ik heb mogen begeleiden en/of met wie ik heb samengewerkt en van wie ik veel geleerd heb, in het bijzonder: Sander Toet, Ellis Toxopeus, Ton van Burgsteden, Arnoud van der Meulen en Thijs Noppen.

I thank all contributors to the Social Learning Project, whose work was of crucial importance in the shaping of my ideas for this thesis. Without the discussions in the writing-team of the Risk-Assesment chapter, chaired by Jill Jäger, the analysis in chapter two of this thesis wouldn’t have existed. In het bijzonder dank ik ook Gerda Dinkelman met wie ik de eerste jaren van mijn onderzoek nauw heb samengewerkt.

Ik dank de leden van de onderzoekscluster risnorm van de Vakgroep Natuurwetenschap en Samenleving, Universiteit Utrecht die in o.a. de brainstorm sessies en in andere discussies belangrijke bijdragen geleverd hebben aan mijn werk. In het bijzonder dank ik Ruud Pleune, Anne Stijkel en Kaat Schulte-Fischedick.

Ik dank Marjan Ossebaard en Willem de Ruiter voor hun review-commentaar op het inleidende hoofdstuk van dit proefschrift.

Ik dank alle mensen die in de afgelopen vijf jaar mijn kamergenoten zijn geweest, eerst op Payenborg en later op CGN B229, te weten, Ruud Pleune, Marjan Blom, Jose Potting, Miranda Schreurs, Arlette van der Kolk, Detlev van Vuuren en Ewald Korevaar. Zonder de plezierige samenwerking met hen en de overige leden van de Vakgroep Natuurwetenschap en Samenleving was het werk in Utrecht beslist minder de moeite waard geweest.

Ik dank de NWO voor het faciliteren van deelname aan het 1996 Young Scientists Summer Program (YSSP) van IIASA. I thank all participants of the 1996 YSSP, who made my stay in Austria so worth while and who left me with very good memories, in particular my office mates Vladimir Sokolov, Partick Kalders and Tony Westerling, and of course Katya Samsonova for her friendship. Further, I thank Jill Jäger for her proficient and inspiring supervision and feedback.

Ik dank Kees Engel van Vereniging Milieudefensie voor de indrukwekkende onophoudelijke stroom systematisch verzamelde krantenknipsels over het broeikaseffect waarvan hij mij de afgelopen jaren heeft voorzien. Zijn knipsels maakten het mogelijk de publieke discussie over het broeikaseffect moeiteloos bij te houden.

I am grateful Sheila McNab, for making linguistic and stylistic improvements to this thesis.

Ik dank Ludie Feyen voor het ontwerp van de omslag en voor haar advies bij de vormgeving van het proefschrift.

Tenslotte dank ik Gemma Boetekees. In de periode van de afgelopen vijf jaar, temidden van alle onzekerheden die ik heb bestudeerd, is er steeds één zekerheid geweest, lieve Gemma, en dat ben jij. Je hebt in veel opzichten aan de totstandkoming van dit proefschrift bijgedragen en me geholpen de moed er in te houden. Je hebt me de vrijheid gelaten en de voorwaarden geschapen om de workaholic te zijn die je moet zijn om in slechts vijf jaar een toch nog enigszins wetenschappelijk verantwoord proefschrift schrijven. Het is daarom dat ik dit proefschrift aan jou opdraag.