Structure and Agent in Leadership Diplomacy

Institutional design and leadership performance in the science-politics interface of climate change

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by

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Structure and Agent in Scientific Diplomacy *

Introduction

Science constitutes a central component in international environmental policy-making. Scientific advisory bodies have increasingly become integral parts of negotiating efforts to combat common environmental problems. The effectiveness of international environmental regimes may depend upon the extent to which they build on the most updated scientific account of basic cause-effect relationships characterizing the phenomenon in question. Still, the conditions for, and mechanisms under which, scientific knowledge is transformed into decision premisses for policy decisions at the international level has to a very little extent been subject to systematic analysis.

This study takes as its point of departure the internal dynamics of both science and politics in order to explore the character of the interface between them, within which the science-policy dialogue of regime formation processes takes place. The process of transforming knowledge into decision premisses can be studied neither as a "purely" scientific nor as a "purely" political process. It has its own distinctive features, albeit with elements from both science and politics. Identifying the nature of the science-politics interface of international regime formation processes constitutes the crux of this analysis. In particular, the study investigates, first, the extent to which and how the institutional arrangements of the science-policy dialogue may influence, and serve as instruments for facilitating the transformation of scientific knowledge into decision premisses. Second, the paper explores the role and impact of scientific élites in performing leadership functions in the process. The paper explores these questions first theoretically, then empirically in a case study of the regime formation process on global warming.

The paper proceeds in three parts. In the first part, the research problem of the study is presented. In the second part, the theoretical framework on which the study builds is

* Acknowledgements: I am grateful to Arild Underdal for his comments on an earlier draft of this paper. The empirical analysis is written with valuable comments, information and help from several persons. I would like to express special thanks to Sonja Boehmer-Christiansen (The Science Policy Research Unit at the University of Sussex) and Leiv Lunde (The Fridtjof Nansen Institute) for generously giving me access to their data on the IPCC. The study would not have been possible without a good cooperation with the Norwegian Ministry of Environment in general, and Håvard Thoresen, Per Bakken and Øyvind Christophersen in particular. Conversations with central participants in the IPCC process have also been most valuable. Sir John Houghton, Dr. Bruce Callander, Prof. Ivar Isaksen, Dr. Robert Watson, Dr. John Mitchell, Dr. Cath Senior and Kathy Maskell should be mentioned specifically. The full responsibility of any errors, shortcomings or misintepretations rests, of course, with the author alone. The study builds on a dissertation project financed by the Norwegian Research Council.

discussed. In the third and final part of the paper, the questions raised in the study will be investigated empirically in the context of the regime formation process on climate change.

1. The Research Problem

The subject matter of this study is the role of science in international environmental regime building. More specifically, I will focus on institutional conditions for transforming knowledge into decision premisses in international environmental regime formation processes, the role and impact of leadership performances in this respect, as well as the interplay between these two factors. What is the impact of "structure" and "agent" in this context, and what characterizes the relationship between them?

One motivation for chosing this approach is that institutional arrangements constitute a *social construction* that, in principle, can be *designed* or manipulated by the actors themselves. Thus, institutional design holds an *instrumental potential* although the availability of this instrument may be restricted.

The impact of institutional arrangements will be studied in terms three dimensions that seem to be particularly important for a constructive science-policy relationship[1]. The first dimension concerns the *autonomy* or integrity of the scientific bodies. For scientific knowledge to be accepted as valid it seems to be important that the conclusions (or advice) are provided by bodies acknowledged as "scientific" by scientists as well as policy-makers, and that the conclusions are perceived to be valid and tenable, meeting the standards of the scientific communities themselves. The second dimension concerns the *representativity* of the scientific bodies; conceived as the extent to which all relevant scientific schools and traditions are represented in the process. This may also imply representativity in (geo-) political terms, if the content of science in any way is perceived to be correlated with social factors, or, more specifically, idiosyncratic properties of the scientists engaged in the effort. The third dimension concerns the level of *involvement* of scientific bodies in the political process. In order to achieve a *dialogue* between scientists and decision-makers, some level of involvement in the policy-making process is required.

In the empirical anaysis these dimensions will be studied in terms of several indicators including; rules of procedure, criteria for recruitment, main function and the "operational autonomy" of the body (the extent to which they can decide on aspects such as the organization of their own agenda, division of labour and allocation of specific roles).

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[1]See for instance Tora Skodvin and Arild Underdal (1994), "The Science-Politics Interface: Transforming knowledge into decision inputs for international environmental regimes" Paper prepared for the *Annual Meeting of the International Studies Association*, Washington D. C., 29 March - 1 April 1994.

The dimensions are to a large extent contradictory, particularly with regard to level of scientific autonomy and level of involvement. Thus, a primary focus of the study is to investigate how these dimensions may be combined or "balanced", and the role and impact of performances of leadership in this regard.

The impact of individual leadership performance has been given increased attention in studies of regime formation processes. Leadership performance is, by some, regarded to constitute a necessary (but not sufficient) condition for regime formation[2]. In this study I will focus on the impact of a leadership role I have labeled "scientific diplomacy", as a possible mechanism for achieving a balance between the dimensions of autonomy and involvement, through which the transformation of knowledge may be facilitated. "Leadership" will be conceived of in terms of the definition developed by Underdal: "an asymmetrical relationship of influence, where one actor guides or directs the behaviour of others towards a certain goal over a certain period of time"[3]. The term "scientific diplomacy" is chosen for two reasons: First, the activity is assumed to be carried out by individuals who are scientists by profession, but who also are familiar with the "art of diplomacy" and the "rules of the game" in international negotiations. Second, this term is chosen because the activity is assumed to hold an element of negotiation, although certainly of a different nature than what is usually associated with the term in "traditional" negotiation theory. One objective is to explore the nature of informal negotiations among scientists as well as between scientists and policy-makers, in particular the role of scientific élites, or "scientific diplomats", in this respect.

The factors singled out for closer scrutiny in this study, institutional arrangements and leadership performance, are not assumed to constitute the *most important* factors for explaining scientific impact in international policy-making. Other factors of equal or more importance are factors such as; i) The state of knowledge, particularly with regard to level of scientific uncertainty. The less conclusive the scientific evidence, the less likely it seems that it will constitute a basis for a policy response, at least with some scope. ii) The structure of the problem in question, particularly with regard to level and nature of political conflict. The more intense the political conflict, the less "room" there seems to be for taking scientific conclusions or advice into consideration in the development of joint solutions. Moreover, the structure of the problem may have impact on the design of institutions as well. If the institutional design is assumed (by the actors themselves) to influence the outcome, this aspect will also be subject to negotiations. Thus, the design of institutions may also be a

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2See for instance Oran R. Young (1991), Political Leadership and Regime Formation: On the Development of Institutions in International Society, *International Organization* 45:281-309.

3Arild Underdal (1991), Solving Collective Problems: Notes on Three Modes of Leadership. In Festschrift to Willy Østreng, *Challenges in a Changing World*, The Fridtjof Nansen Institute, pp. 138-153.

function of the configuration of power and interests among participating actors. iii) The public saliency of the issue. High public saliency may contribute to an increased "demand" for "more knowledge". It may, however, also serve to politicise the issue and increase the level of political conflict, and thereby reduce the scope for scientific impact. In this study, it is not possible to "control for" these factors in a strict sense. This modification regarding the relative explanatory power of the independent variables in the study, should, however, be kept in mind.

2. Developing a Theoretical Framework

The study takes as its point of departure the internal dynamics of both science and politics, in order to explore the nature of the interface between them, within which the science-policy dialogue of regime formation processes takes place. The theoretical account of the research problem confronts two major questions:

a) Under which conditions are regimes developed? (To what extent may institutional arrangments and leadership performance have an impact on the development of international regimes? And, to what extent can science be assumed to influence policy-making?)

b) To what extent is the content of science influenced by social factors, such as institutional arrangements or idiosyncracies of scientists engaged in the scientific process?

Discussing these questions, I will build upon two main bodies of thought. The nature and dynamics of regime formation processes will be discussed on the basis of negotiation and regime theory, while the social dimensions of science will be discussed on the basis of theories within the philosophy and sociology of science.

2.1. The Role of Science in International Policy-Making

Scholars of international relations have for some decades been studying the emergence, development and role of international regimes for altering and regulating actor behaviour in the international system. Two questions in particular, have dominated the research agenda in this period; First, why are international regimes established in some areas, while they are not established in other, seemingly similar areas? Second, why are some regimes more effective than others in solving the problems they are meant to solve? While the former question addresses conditions for regime *formation*, the latter addresses conditions for regime *effectiveness*. While the two problems are related, they are not analogous. Although the regime formation process has been successful, in the sense that a regime has been created, it need

not be effective: That is, the "...agreed-upon principles, norms, rules and decision-making procedures that govern the interactions of actors in specific issue areas"[4], may not have been designed so as to solve the problem in the most efficient manner[5].

The concept of regime "effectiveness"is complex and hard to measure. Questions like what constitutes the object to be evaluated, against which standard it should be evaluated, and how this evaluation operationally should be carried out, are not straightforward, neither methodologically nor empirically[6]. It seems reasonable, however, to argue that one basic condition for regime effectiveness is that the agreed-upon principles, rules, norms and decision-making procedures that are *meant* to solve the problem in question, should be based on knowledge of what the problem looks like in terms of cause-effect relationships, in order to do so. Thus, scientific knowledge and the transformation of this knowledge into decision premisses *in the regime formation process* may constitute a condition for regime *effectiveness*, although it is not obvious that it is a necessary condition for the regime formation itself. This proposition constitutes a basic assumption for this study. Thus, we will take as our point of departure the conditions under which regimes are developed, and investigate when and how science may influence policy-making.

2.1.1. Process Factors in Regime Formation

The negotiation process preceding international regime formation, may be conceived as a process of "institutional bargaining"; "[e]fforts on the part of autonomous actors to reach agreement among themselves on the terms of constitutional contracts or interlocking sets of rights and rules that are expected to govern their subsequent interactions."[7] The bargaining situation is usually characterized by a high level of complexity. In contrast to assumptions often made in formal theories of bargaining, the situation as experienced by negotiators may be characterized by a high level of uncertainty rather than perfect information; their preference orderings may be a function of what appears to be achievable rather than what is regarded as optimal; the information about other participants may be imperfect or even lacking, as may also knowledge about the alternatives and their impacts. In this situation,

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4Young and Osherenko (1993), *Polar Politics. Creating International Environmental* Regimes. Cornell University Press: Ithaca and London, p. 1.

5The distinction between "output", "outcome", and "impact" is relevant in this context. Output refers to the agreements made, for instance, in terms of agreements to reduce emissions of polluting substances. Outcome refers to the extent to which the agreement serves to alter behaviour. Impact refers to the problem-solving ability of the "amount" altered behaviour achieved, i.e., the extent to which the altered behaviour is suffiencient in order to solve the problem concerned. Here, we refer to the latter notion of "efficiency".

6See for instance Arild Underdal (1992), *The Concept of Regime "Effectiveness"*, Cooperation and Conflict, 27 (3):227-240.

7Oran R. Young (1991), op.cit., p. 282.

process features (which often constitute a "black box" within formal theories of bargaining) may have an independent impact on the outcome.

Underdal takes this into account when arguing that the *"problem-solving capacity"* of the effort to develop joint solutions to common problems constitutes one important determinant to its outcome (in *addition* to the character of the problem itself).[8] Problem-solving capacity is conceived as a function of three main determinants; "(a) the institutional setting, notably the "rules of the game"; (b) the distribution of power among the actors involved; and (c) the skill and energy available for designing and engineering cooperative solutions."[9]

The potential impact of institutional arrangements has been demonstrated by, *inter alia*, James Sebenius in his studies of the Third UN Conference on the Law of the Sea.[10] His studies show that the addition or subtraction of both issues and parties may have significant implications in terms of altering the "contract zone" or set of politically feasible solutions. The studies of, *inter alia*, Young[11] and Underdal[12] have shown that performances of leadership roles in the negotiations may have significant impact in terms of "discovering" or designing integrative solutions of this kind. Moreover, other studies have shown that the "success" of efforts to perform leadership roles may depend upon the extent to which institutional arrangements serve to provide "room" and incentives for this kind of actor behaviour, and also that performances of leadership may be directed towards designing institutions for enhancing the problem solving capacity of the effort.[13]

2.1.2. The Role of Science in International Regime Formation

The role of science and scientific communities in regime formation has been given an increased emphasis, particularly after the publication of Peter M. Haas' study of the role of "epistemic communities" in the development of the Mediterranean Action Plan[14] and the

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[8]See for instance Arild Underdal (1990), Designing Politically Feasible Solutions: Notes on the Poltical Engineering of International Cooperation. Paper prepared for the 9th Triannual Convention of the *Nordic Political Science Association*, Reykjavik, 16-18 August, 1990; and the 86th Annual Meeting of the *American Political Science Association*, San Francisco, 30 August - 2 September, 1990.
[9]Ibid., p. 2.
[10]James K. Sebenius (1984), *Negotiating the Law of the Sea*. Harvard University Press: Cambridge, Massachusetts.
[11]1991, op.cit.
[12]1990, op.cit.
[13]See for instance, Barry Buzan (1981), Negotiating by Consensus: Developments in Technique at the United Nations Conference on the Law of the Sea. *American Journal of International* Law, vol. 77, 2: 324-348. See also, Tora Skodvin Hegdal (1992), "Structure" and "Agent" in Institutional Bargaining. Institutional Design and Leadership Performance in the Third United Nations Conference on the Law of the Sea. Cooperation and Conflict, vol. 27, 2: 163-189.
[14]Peter M. Haas (1990), *Saving the Mediterranean. The Politics of International Environmental Cooperation*. Columbia University Press: New York.

international regime on ozone depletion.[15] The "epistemic communities approach" is, "...an approach that examines the role that networks of knowledge-based experts - epistemic communities - play in articulating the cause-and-effect relationships of complex problems, helping states identify their interests, framing the issues for collective debate, proposing specific policies, and identifying salient points for negotiation."[16] An epistemic community is defined as "...a network of professionals with recognized expertise and competence in a particular domain and an authoritative claim to policy-relevant knowledge within that domain or issue area."[17] The members of epistemic communities, are assumed to have, "...a shared set of normative and principled beliefs..."; "...shared causal beliefs..."; "...shared notions of validity..."; and, "...a common policy enterprise...".[18]

This approach seems to tie up to the emphasis of this study on the role and impact of performances of leadership, or "scientific diplomacy", in the regime formation process. In this study, however, the main focus is directed towards the *mechanisms* by which these professionals are recognized as experts with "an authoritative claim to policy-relevant knowledge", and the role of institutional arrangements in this respect. Moreover, the performance of "scientific diplomacy", as studied here, does not necessarily imply membership of an epistemic community, as defined by Haas.

Studies of the role of science in regime formation indicate that the potential scope of influence is not equal in all phases of the process. A general tendency seems to be that the potential scope of impact is relatively larger during the phases of pre-negotiation and implementation, than in the negotiation phase where the actual provisions of the constitutional contracts are developed.[19] On the basis of several case studies, Young and Osherenko suggest, "...that knowledge-based factors can and often do play a role of considerable significance in the early phases of regime formation but that they decline in importance as the negotiators begin to hammer out the actual provisions of a regime."[20]

Thus, process factors, such as institutional arrangements and performances of leadership roles may influence the development of international regimes, and science, particularly in the early phases of the regime formation process, may serve as a decision

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[15]Peter M. Haas (1992), Banning Chlorofluorocarbons: Epistemic Community Efforts to Protect Stratospheric Ozone. International Organization, vol. 46, 1:187-225. This issue of International Organization is, moreover, a special issue on "Knowledge, Power, and International Policy Coordination", edited by Peter M. Haas.

[16]Peter M. Haas (1992), Epistemic Communities and International Policy Coordination. Introduction to the Special Issue on "Knowledge, Power, and International Policy Coordination" of International Organization, op.cit., p. 2.

[17]Ibid., p.3.

[18]Ibid.

[19]See for instance Andresen & Østreng (eds.) (1989), *International Resource Management: The Role of Science and Politics*. Belhaven Press: London and New York; Underdal and Skodvin, 1994, op.cit.; Young and Osherenko, 1993, op.cit.

[20]Op.cit., p.254.

premisse. To what extent may the impact of science also be linked to the institutional arrangements and performances of leadership in the regime building effort? If the impact of science is associated with these factors, it would imply that the transformation of knowledge, and maybe also its content, is influenced by social factors, and not solely determined by the strength or objective validity of scientific evidence; a propsition which needs to be investigated on the basis of theories within the philosophy and sociology of science.

2.2. Exploring the Social Dimensions of Science

To what extent is science, and science-policy relationships influenced by social factors? A common perspective on the relationship between scientific knowledge and policy choice (characterized by Sheila Jasanoff as a "positivistic model of scientific knowledge"[21]) is that it is of a linear nature: As scientific knowledge of a phenomenon accumulates, the extent to which it serves as premisses for policy choice is also assumed to increase. According to the studies of Sheila Jasanoff, however, there is no evidence that, "...advances in knowledge ... predictably correlates with reductions or increases in policy conflict."[22] The assumption of a linear relationship between science and politics seems to reflect a scientific "ideal" where science and politics are seen as separate and separable entities, where science is seen as "objective" and value-free, and the scientific body (in a policy-making context) as a neutral provider of information/knowledge. This ideal seems to imply that the extent to which scientific knowledge is transformed into decision premisses is solely determined by its objective validity. Moreover, the dominance of this scientific ideal in the studies that have been carried out, seems to have contributed to an almost exclusive attention towards the manner in which science may influence policy-making, without a corresponding interest in how the involvement itself may influence the scientific process[23]. Influence in this direction has to a large extent been regarded as a negative and dysfunctional politicisation and "contamination" of scientific research. The involvement in a political, and often conflict-

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[21]Sheila Jasanoff (1991), *The Fifth Branch. Science Advisers as Policymakers*. Cambridge, Massachusetts and London, England: Harvard University Press.
[22]ibid., p. 8.
[23]Sonja Boehmer-Christiansen's studies constitute a notable exception to the "maintream" approach of studies in this field. She has focused on how scientists' ("non-scientific") interests (such as. for instance, increased funding) are served by participating in processes of this kind, and also how scientists' interests, in this respect, may be convergent with policy-makers' interests in the same process. See for instance; (1993a), "Scientific uncertainty and power politics: The Framework Convention on Climate Change and the Role of Scientific Advice" Paper prepared for the conference, *Geopolitics of the Environment and the New World Order*, Chantilly, January 1993; (1993b), "Science Policy, the IPCC and the Climate Convention: The Codification of a Global Research Agenda" *Energy & Environment*, vol. 4, no. 4.

ridden process does imply a significant risk of influence of this kind. However, it seems obvious that the involvement itself also may affect the scientific process in manners which do not represent politicisation (in terms of the negative connotations of the word), but rather constitutes the essence of an interactive dialogue between science and politics in a policy-making process.

A natural starting point for a discussion on the social dimensions of science seems to be the notion of scientific consensus, and particularly, the manners in which consensus in science is developed. Scientific consensus seems to be a prerequisite for policy decisions to be based on scientific knowledge. As Mulkay argues, "only propsitions which command (more or less?) universal assent on the part of competent judges can be said to be true "knowledge"; ...As soon as intellectual disagreement appears we seem to be dealing not with demonstrated knowledge, but merely knowledge-claims."[24] 'The role of consensus in science has, however, been subject to a relatively fierce argumentation between two prominent philosophers of science, Karl Popper and Thomas S. Kuhn. While Kuhn regards scientific consensus, or "convergent thinking", to be an important characteristic of "normal science"[25], Popper claims scientific consensus to be dogmatic, pseudo-scientific and inconsistent with the "critical attitude" he associates with science.[26] Taking a closer look at Popper's claims, however, we find that it is not consensus as such he resents, but an explicit aim on the part of scientists to *verify* rather than *falcify* existing theories. He acknowledges that hypotheses can be more or less "corroborated" as they repeatedly have passed tests of falcification, and that scientists do operate within "structures of scientific doctrines."[27] According to this perspective, however, whether or not scientific consensus prevails - or whether or not knowledge-claims are transformed into "certified" knowledge (on which a policy choice can be based) - is determined by the strength, or *objective validity*, of scientific evidence (alone), and the mechanism by which this "outcome" is provided is that of a scientific method. Thus, scientific controversy can only be resolved when scientists agree on the objective validity of scientific evidence (for instance when uncertainty is reduced), not through any social mechanism, such as for instance negotiations. This perspective, therefore, leaves little room for studying the social dimensions of science. As Mulkay points out, "as long as we accept that there is a strong intellectual consensus in science, and that consensus about and

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[24]Michael Mulkay (1978), Consensus in Science. *Social Science Information*, 17, 1:107-122, p.109.
[25]See for instance Thomas S. Kuhn [1962] (1970), the Structure of Scientific Revolutions. Second enlarged edition, London and Chicago: The University of Chicago Press. See also Thomas S. Kuhn (1970), Logic of Discovery or Psychology of Research, in Lakatos and Musgrave (eds.) *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press.
[26]See for instance Karl Popper (1963), Science: Conjectures and Refutations, in *Conjectures and Refutations*. London: Routledge and Kegan Paul, pp. 33-59, and Karl Popper (1970), Normal Science and its Dangers, in Lakatos and Musgrave (eds.) op.cit., pp. 51-59.
[27]1970, op.cit.

invariance of scientific knowledge are due to its objective validity, we experience some difficulty in regarding the *content* of scientific knowledge as being dependent in any direct way on social processes. These assumptions prevent us from treating scientific knowledge itself as a social product..."[28]

During the last couple of decades, however, sociologists of science have increasingly emphasised the influence of social factors on the content and authoritativeness of scientific knowledge-claims, particularly in policy-related science. Jasanoff argues that, "facts are accepted as authoritative not necessarily because they can be empirically verfied, but because they are validated through processes of *informal negotiation* and can be ranged into frameworks of shared assumptions and inferences"[29]. Similarly, Mulkay argues that, "...the eventual consensus which appears in many fields seems to be the outcome of a delicate balance between cooperation and collaboration, on the one hand, and dispute, competition and specialization, on the other hand. In other words, scientific consensus in research networks seems to be achieved, at least partly, by processes of informal negotiation between participants who have certain shared as well as certain conflicting interests"[30]. Thus, within this approach, as opposed for instance to a Popperian approach, aspects of negotiation and persuasion are increasingly emphasised.

Assuming that elements of negotiation within and among scientific communities may constitute a means for developing consensual knowledge, also implies that "...the processes whereby scientific consensus is established and maintained become open to sociological analysis"[31]. Moreover, Mulkay has found that the social context within which knowledge is applied also may have important implications for the conclusions reached, and that this "context-dependency" may explain why "...members of the same specialized and mature research community frequently reach different conclusions when they try to apply their expertise in practical situations"[32]. This, Mulkay argues, indicates, "...that intellectual consensus in science is relatively loose and flexible, and that its content is open to interpretation in numerous directions"[33]. In our context, this is important, because it implies that the institutional arrangements of the science-policy dialogue may have impact on its outcome. It also implies that the *personal* qualities, or status, of the scientists engaged in the effort may not be indifferent.

Although several scholars have pointed to processes of informal negotiation within scientific communities as important determinants to the content of "certified" knowledge (as distinguished from propositions that remain "knowledge-*claims*"), it is difficult to find a more specific account of the nature of these negotiations (where, how and among whom they take place). Mulkay´s work, however, indicates that scientific élites may play an important role

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[28]op.cit., p.110.

29Sheila Jasanoff (1987), Contested Boundaries in Policy-Relevant Science. *Social Studies of Science*, vol 17, 195-230, p. 195, italics added.

30op.cit., p.111

31ibid.:112.

32ibid.:118.

33ibid.

in this respect[34]. Mulkay finds that in Britain and America (which are the countries covered by his study) the academic research community contains a small and influential scientific élite, "...the members of which are widely regarded by fellow scientists as having made a major contribution to scientific knowledge. The members of the élite tend to be those who have demonstrated the highest commitment to an ethos in which the search for scientific knowledge is the highest objective and needs no further justification"[35]. He finds that the élite, due to their scientific merits, has significant influence on the selection of scientific work regarded as acceptable and important contributions to knowledge, and thus also, implicitly, are influencial in determining the content of knowledge regarded as "certified": "...those scientists who wish to advance their careers and/or to produce acceptable contributions to knowledge must comply with the cognitive standards set and exemplified by these leaders"[36].

Investigating the connections between the academic research community and the "wider society", Mulkay found that scientific élites also serve a central role as "mediators": "...much of the pressure exerted by governments, and perhaps by other lay groups, on the development of scientific knowledge is mediated through the élite"[37]. Moreover, he found that, "...at least in Britain, this scientific élite operates as a "buffer group", successfully resisting instrumental demands from outside and maintaining considerable freedom for members of the academic research community to pursue their own "scientifically defined" interests"[38]. Mulkay also finds that during the last couple of decades this intermediary role of the scientific élite has become firmly *institutionalized* both in Britain and America, in "...a complex hierachical system of scientific "advisory" bodies ... gradually established in parallel with the bureaucratic hierachy of government"[39].

Building on Michael Mulkay, Hart and Victor have conducted a case study of the role of scientific élites as mediating agents in the relationship between bench scientists and the political community. In their study of "Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74"[40], they found that élite entrepreneurship served as a mechanism by which scientific agendas were influenced by politics - for instance by redefining which problems were important, and allocate funds for further research - as well as they served to shape the beliefs of policy makers and the public. In particular, the integrated assessments and state-of-the-art reports made by "ad hoc, blue-ribbon groups", "...played important roles in élite mediation between the bench and the hearing room (...)

34Michael Mulkay (1976) The Mediating Role of the Scientific Elite. *Social Studies of Science*, vol. 6, no. 3 and 4, September 1976:pp. 445-471.
35ibid.:462-3.
36ibid.:454.
37ibid.:446.
38ibid.
39ibid.:455.
40David M. Hart and David G. Victor (1993), Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74. *Social Studies of Science*, vol.23:643-80.

Representing a presumed consensus, they were a more important tool for the élite than the normal publications that practitioners use to build the state-of-the-art (that is, technical journal articles). Generating them at strategic moments assisted entrepreneurs to link opportunities to research agendas"[41]. Later, (in 1988) the production of assessments and state-of-the-art reports on climate change research has been institutionalized at the international level - in the Intergovernmental Panel on Climate Change (IPCC). Thus, we see an interesting parallel, at the international level, to the nationally based institutionalization of scientific "advisory" bodies described by Mulkay.

The dialogue between scientists and policy-makers in, for instance, regime formation processes, takes place in the *interface* between science and politics. This implies that the scientific (advisory) process, can be studied neither as a (purely) scientific nor a (purely) political process. It has its own distinctive features, albeit with elements from both science and politics. Thus, policy-related science may be distinguished from "pure" science activities (or "research science"), not so much maybe for its intellectual content, as for the *contextual* and/or *institutional properties* of the activity; the *framework* within which the activiy takes place. What is the nature of the relationship between institutional arrangements and behaviour in the science-policy interface? Before I proceed with the empirical analysis I will discuss briefly some of the distinctive features of the science-policy interface in this respect, particularly as identified by Sheila Jasanoff in her studies of "regulatory sciences" in the United States.[42]

2. 3. The Science-Politics Interface

In her studies of scientific advisory proceedings in policy-making processes in the US, Sheila Jasanoff has found that "...negotiation - among scientists as well as between scientists and the lay public - is one of the keys to the success of the advisory process."[43] This, she argues, indicates that a strict separation between science and politics, in this context, is artificial: "The negotiated and constructed model of scientific knowledge, which closely captures the realities of regulatory science, rules out the possibility of drawing sharp boundaries between facts and values or claims and context. ... Evidence from regulatory case histories suggest, further, that proceedings founded on the separatist principle frequently generate more conflict than those which seek, however imperfectly, to *integrate* scientific and political decisionmaking."[44] Similarly, Lawrence Susskind emphasises that there is a need for a better "balance" between science and politics in international envionmental treaty making:

41ibid.
42 1991, op.cit.
43 ibid., p. 234.
44 ibid., p.231, italics added.

"Independent scientific investigations play a role in environmental treaty making, but they are *interwined with, not separate from*, political considerations."[45] The integration needed between scientific and political decision-making can be achieved by institutional means:

> "The forums at which scientists are brought together must have ground rules. Procedures governing the gathering of scientific evidence and the conduct of scientific meetings have, from time to time, been built into certain treaties, but these have rarely been crafted with an eye toward ensuring a more effective ongoing relationship between technical and political actors. Instead, they have assumed that the scientists should meet first and reach agreement on the facts, and then let the politicians take over. What we need are guidelines that encourage continuing accountable relationships."[46]

The paradox is, however, that although negotiations, both among scientists and between scientists and policy-makers, may be an essential part of a successful scientific advisory process, an equally essential part of the process, according the studies of Jasanoff, is to draw seemingly sharp *boundaries* between science and policy. In contrast to "classical" models of science, however, Jasanoff does not regard the boundaries as *objective* features of science, but as socially negotiated. And "boundary work" constitutes, according to her studies, an important part of "regulatory science".

> "By drawing seemingly sharp boundaries between science and policy, scientists in effect post "keep out" signs to prevent nonscientists from challenging or reinterpreting claims labeled as "science". The creation of such boundaries seems crucial to the political acceptability of advice ... Curiously, however, the most politically successful examples of boundary work are those that leave some room for agencies [EPA and FDA in her case studies] and their advisers to negotiate the location and meaning of the boundaries."[47]

How do these findings relate to the initial assumptions of this study, where the dimensions of *scientific autonomy, representativity and involvement* were assumed to be important for the extent to which knowledge is transformed into decision premisses in regime formation processes? Although this choice of concepts may signal adherence to the "classical" model of science, against which Jasanoff argues, their substantial content, in terms of which functions the institutional framework of the science-policy dialogue may serve, seems to be consistent with Jasanoff´s findings. First, "scientific autonomy" seems to be important in the sense that the advice, or the scientific basis for decision-making, is provided by a body acknowledged, by all parties, as "scientific". Second, since the transformation of knowledge into decision premisses, according to Jasanoff´s studies, occurs as a function of negotiations between scientists and policy-makers, bodies serving to integrate science and politics, or "involve"

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[45]1994, op.cit., p.62, italics added.
[46]ibid., p.78.
[47]1991, op.cit., p.236.

scientists in the policy-making process, would be instrumental towards this end. Finally, accepting that scientific knowledge is (partly) socially constructed, and that advisory bodies serve as fora for negotiating such constructs, also implies that "...committees should include representatives of all relevant political (as well as scientific) viewpoints"[48], however, with due consideration of the requirements to the "scientific" status of the body. As Jasanoff herself comments, although the picture she presents of the advisory process,

> "...is markedly at odds with the simple technocratic paradigm of "speaking truth to power" [it is], paradoxically, not inconsistent with it. According to this account, the committees attached to EPA and FDA do indeed help agencies define good science - and this consensus view of science in turn influences policy - but they perform this function in part through skilled boundary work and in part through flexible role playing. ... Though their purpose is to address only technical issues, committee meetings ... serve as forums where scientific as well as political conflicts can be simultaneuously negotiated. When the process works, few incentives remain for political adversaries to deconstruct the results or to attack them as bad science.""[49]

Thus, the *mechanisms* by which the functions are served, are significantly different than is assumed in the classical approach; differences which may have important implications for what constitutes the "best way" to organize processes of this kind in order to succeed.

3. From Science to Politics in the Global Greenhouse

A Brief Historical Introduction

Throughout the 19th century studies on the relationship between the earth's surface temperature and the chemical composition of the atmosphere appeared at intervals of some decades. The starting point for international efforts to better understand climate variations and the possible problem of a human induced climate change, however, is generally regarded to be the UN Conference on Human Development in Stockholm in 1972. By this time, the climate change issue increasingly surfaced on the international political agenda. During the period from 1972 to 1988, climate research was largely organized through WMO and UNEP sponsorship. In 1979 the World Climate Conference was held in Geneva, and the World Climate Programme (WCP) was launched.

The IPCC started its work in November 1988[50]. The objective of the IPCC has been "...to provide the scientific, technical and analytical basis for informed and intelligent policy

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[48]ibid., p.244.
[49]ibid., p. 236.
[50]For a detailed account of the history behind the IPCC establishment, see Sonja Boehmer-Christiansen (1993), "Science Policy, the IPCC and the Climate Convention: The Codification of a Global Research Agenda" *Energy & Environment*, vol.4, no.4, pp. 362-407.

choices."[51] The function of the IPCC has been to coordinate and initiate research on human-induced climate change in order to fulfil three tasks; i) assess available scientific information on climate change, ii) assess environmental and socio-economic impacts of climate change, and iii) formulate response strategies.[52] A working group was set up for each task. In 1990 IPCC's First Assessment Report was presented to the Second World Climate Conference, where it was accepted as an adequate basis on which to start negotiations. In 1992, Working Group I of the IPCC presented the "Supplementary Report". The Second IPCC Assessment Report is scheduled for November 1995, with an interim report scheduled for presentation in November 1994.

In December 1990 the Intergovernmental Negotiating Committee for a Framework Convention on Climate Change (INC/FCCC) was formally established in the UN General Assembly Resolution 45/212. In February 1991 the first session of the INC was launched. From February 1991 to April 1992, five sessions were held. The fifth session (held in two sections in February and April 1992) resulted in a Climate Convention ready for signing at the United Nations Conference on Environment and Development (UNCED) in Rio, June 1992. This convention does not represent the conclusion of the climate change process, but rather its beginning. Further negotiations on preparations for specific control measures to be adopted in separate protocols or Amendments by the Conference of the Parties were opened by end 1992, and before the agreement formally entered into force (which was set to 90 days after 50 countries had ratified it, and took place 21st of March 1994). The first meeting of the Conference of the Parties (CoP) will take place in Berlin in March 1995.

3.1. From Science to Politics in the Global Greenhouse?

There is no yardstick by which to "measure" the amount of scientific impact, or the relative importance of process factors. One option for dealing with this question is, however, to conceive the outcome of science-policy processes in terms of a cumulative scale with two levels[53]. At the first level, policy-makers accept the substantive conclusions provided by the scientific body as valid and tenable. Whatever the transnational community of scientists considers, by general consensus, to be the best knowledge so far available, is accepted as such also by policy-makers. At the second level, policy-makers accept not only the "factual" conclusions but also what might be called the "policy implications" of these conclusions, and respond in terms of specific (regulatory) actions or at least commitments to act.

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[51]IPCC, Overview and Conclusions, draft 1990.
[52]IPCC Scientific Assessment, 1990.
[53]This definition builds on previous work, see Skodvin and Underdal, 1994, op.cit.

Evaluating the outcome of the IPCC process in terms of the first level on this scale, it certaintly seems to have been a "success". The political nature of the IPCC process, and particularly the political dominance of its main decision-making bodies (in which the assessments were adopted), combined with a significant overlap in participation between these bodies of the IPCC and the INC - in which negotiations on eventual actions were carried out - does seem to imply that the substantive conclusions of the scientific assessments, were accepted, politically, at the moment of the Plenary approval. This was also the reason why they were so heavily negotiated in the fourth Plenary Session of the IPCC (see below). By approving the First Assessment Report of the IPCC, governments accepted that human induced climate change does represent a problem, and started negotiations on how to respond. If this had not been accepted, the first phase of the IPCC process simply would not have resulted in an assessment.

For this outcome, the "balance" between scientific autonomy and involvement achieved in part through skilful leadership, also seems to have made some difference. If the provision of the science base had not been acknowledged as "authoritative" and "objective" science, but rather could be suspected of a value bias, this would most probably have been used for what it was worth in the subsequent deliberations. On the other hand, if policy-makers had not been part of the process, or the process had not been that firmly linked to a policy-making process, the political acceptance of the conclusions would have constituted yet another phase of the process. Moreover, policy-makers would most probably have felt less "committed" by the conclusions. With regard to the first level of the "success" definition, therefore, I would evaluate the outcome of the process as a success that, in part, is due to the process factors to be studied here.

One thing is to accept "factual" conclusions, quite another to agree on whether, and at which scope, to respond. As it seems, neither science as such nor the specific procedural features of the science-policy dialogue, have anything like a decisive say for influencing the outcome in this respect. Accepting that a given phenomenon represents a problem, is not the same as giving priority to its solution in the face of other societal problems. This evaluation remains a ("purely") political matter, and is therefore not handled within the framework of the science-policy interface. Thus, although policy-makers' beliefs and perceptions of risk may be influenced by the proceedings taking place within the science-policy interface framework, other factors will have more emphasis, and thus more explanatory power. With regard to this specific case, moreover, the acceptance of the scientifically based definition of the problem, has not yet resulted in any substantial commitments in terms of regulatory actions. The Framework Convention on Climate Change does not hold binding commitments in this respect. Thus, the outcome of the IPCC process cannot be evaluated as a success in

terms of the second level of our "success" definition, although it did serve to bring about the necessary first step towards this end.

3. 2. The Institutional Structure of the IPCC

The IPCC is an exellent example of the "interface" nature of scientific advisory processes at the international level. The IPCC process is a scientific process in the sense that it has scientific knowledge accumulation, consensus-building and state-of-the-art assessments as its primary objective. At the same time, the process is organized within a political institutional framework (as an Intergovernmental UN body), and constitutes a distinct part of an international regime formation effort. Thus a science-politics *integration* characterizes the IPCC. On a broad scale, the IPCC may be regarded as constituting an intermediate body between the climate change *research* community ("bench scientists") on the one hand, and the climate change *political* community (INC/CoP) on the other. In this respect, the IPCC *constitutes* the science-politics interface of the regime formation process on human induced climate change.

Until 1992 the main bodies of the IPCC were the Plenary, the Bureau, three Working Groups (WGs), with task forces and subgroups on specific issues, and a Special Committee for Developing Countries (established early 1989)[54]. The mandate of the IPCC (given by UNEP and WMO) was to provide assessments of i) the science of climate change, ii) impacts of climate change, and iii) to formulate response strategies to climate change. The tasks were carried out by WGs 1, 2 and 3, respectively. In general, the division of labour was that assessments were developed at the task force/subgroup level of each WG, on the basis of the present "state of knowledge" as well as invited contributions and through informal scientific workshops and meetings. The assessments were then peer reviewed by scientists who had not participated in the preceding process. This outcome was then to be discussed and negotiated at all decision-making levels of the IPCC, and finally accepted in the Plenary as the "IPCC view".

All bodies of the IPCC have elements characteristic of both science and politics, although with shifts in the dominance of the one or the other. While the task force/subgroup level may be characterized as the "scientific core", WG Plenaries and the Panel Plenary constitute administrative bodies, with an increasingly political dominance (see fig. 1, next page).

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[54]In 1992, the IPCC's mandate was revised and enlarged, and the organization was restructured. This implied some changes particularly with regard to the division of labour between WGs 2 and 3. In this paper, the restructuring will be commented upon where relevant.

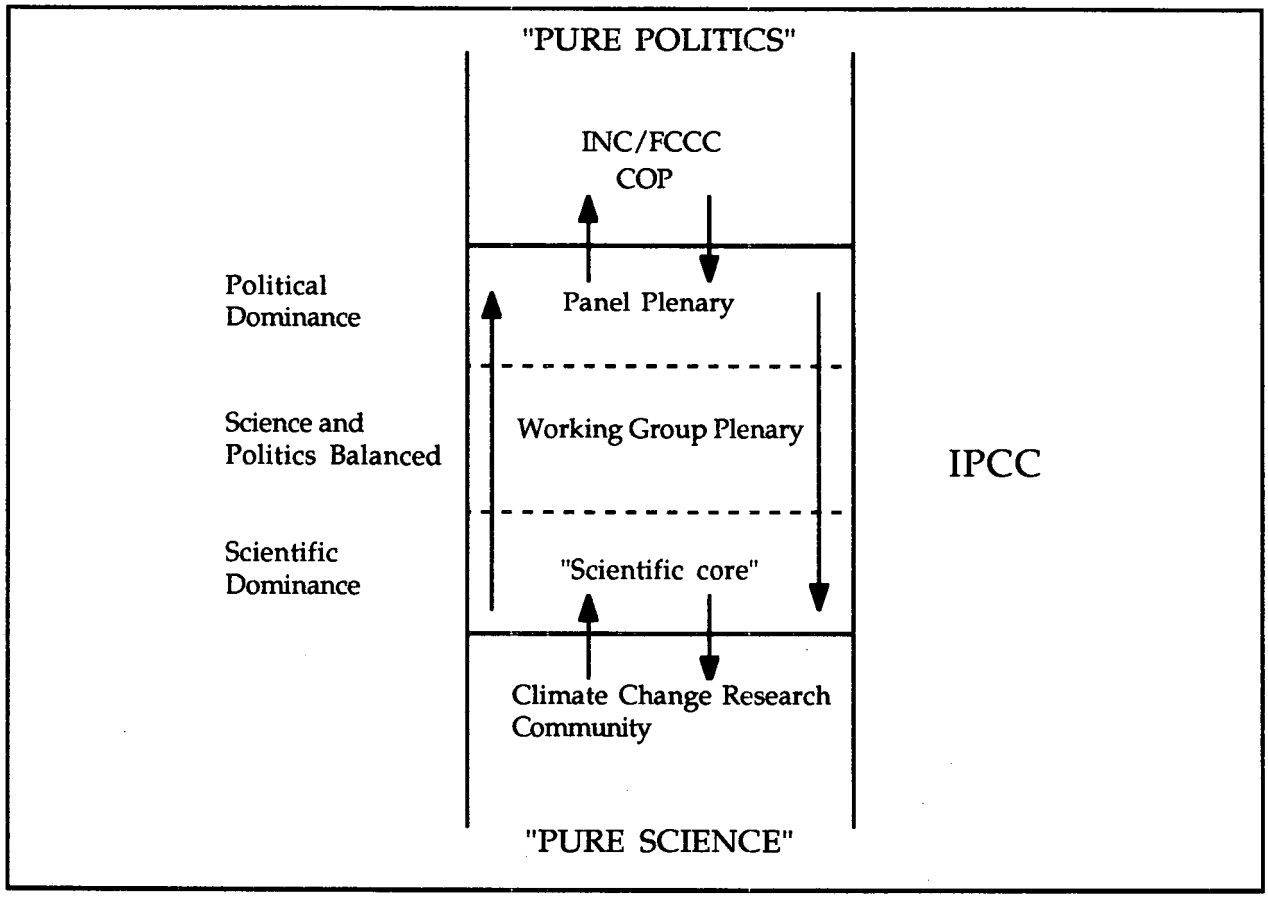

Fig. 1. The science-politics interface of the regime formation process on climate change. The arrows are rough illustrations of formal and informal channels of communication.

3.2.1. "Administrative Bodies": Rules of Procedure

The IPCC Plenary constitutes the main decision-making body of the IPCC; all subsidiary bodies (WGs and task forces) are constituted, and their mandates established and approved by the Plenary. Furthermore, scientific assessments provided by the WGs have to be discussed and accepted by the Plenary before they are considered to represent the official IPCC view. The Plenary also elects the leadership of each WG (chairs and vice-chairs). Lead authors on separate issues of each WG are decided by the leadership of the respective WG (the Bureau of the WG).

The significance of the Plenary as main decision-making body was further increased with the restructuring in 1992. In the preparatory work for the second assessment report, fairly detailed rules of procedure for preparation, review, acceptance, approval and publication of IPCC reports were adopted by the Plenary. Furthermore, although the actual election of lead authors still lies with the leadership of each WG, the Plenary has to a larger extent become involved in this election in that the geo-political representativity of the groups of lead authors has been given more emphasis. Thus, the geographical balance of the groups

has to a larger extent become an issue for discussion and approval in the Plenary. Before 1992, the election of lead authors was left entirely to the leadership of each WG.

Entry to the IPCC (at all levels) is in *principle* open, although the operational autonomy of the WGs implies some potential for restricting participation within the scientific core and in the actual drafting of the assessments (see below). Formally, invitations are extended by the Chairman of the IPCC to Governments "and other bodies" (WMO, UNEP, scientific/ environmental IGOs and NGOs). Thus, any expert nominated by Governments can participate. Experts can also be invited to contribute in their own right, but their respective Governments "should be informed in advance". In the administrative bodies there seems to be a marked tendency towards "political nominations" of participants in that these bodies are largely dominated by government officials, rather than scientists active in research.

In all IPCC bodies, participation shall reflect a balanced geographic representation. With regard to WGs and task forces, the modification "with due consideration for scientific and technical requirements" has been added. In the Plenary report from the eighth session (after the restructuring), this point is further emphasized: "it is highly desirable that governments ensure the scientific-technical integrity and credibility of the IPCC by nominating relevant experts for participation in the work of the Panel. Thus, the Chairman will request that a government indicate the relevant expertise of the candidate it nominates." (item 6.16).

The aspect of geographic representativity is, however, important. In the initial phases of the process, the IPCC was heavily dominated by western industrialized countries. Only 11 developing countries were represented during the first sessions, and usually only by one delegate. Lunde points out that "[t]his bias was early recognised, ... , as an important challenge to the success of the IPCC effort."[55]. Thus, a Special Committee on Developing Countries was set up early 1989. Its main function was to "...promote, as rapidly as possible, full participation of developing countries in IPCC activities."[56]. Their work seems to have been relatively successful, as developing country participation was increased from 10 in early 1989, to 40 by August 1990 [57].

With the restructuring in 1992, it was agreed that instead of a Special Committee on developing country participation, "the special situation of the developing countries should ... be given attention as part and parcel of all the work carried out by the Panel and its groups (working groups/subgroups/task forces)."[58] Thus, this issue is now integrated at all levels of IPCC activities, and the Special Committee on Developing Countries is dissolved. The

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[55]Leiv Lunde (1991), Science or Politics in the Global Greenhouse? EED Report 1991/8, The Fridtjof Nansen Institute, Oslo, p. 84.
[56]ibid.
[57]ibid.
[58]IPCC/TF/3rd/Doc.2 (7.x.1992).

organizational change implied, first, a restructured Bureau in order to achieve better geographical balance, and second, increased impact of the recruitment principle of geo-political representativity in all IPCC bodies and at all levels.

3.2.2. The "scientific core": The Provision of Assessments

Before the restructuring in 1992, there seems to be relatively few *formally* decided rules of procedure for the development of assessments, apart from the requirement of geopolitical representativity in participation. This seems to have implied that the WGs, and the task forces and subgroups in which the actual development of the assessments took place, enjoyed a relatively high *operational autonomy* in their work. Within their mandate, they could organize their own agendas, decide upon the division of labour within the group as well as the allocation of specific roles, such as lead authors, contributers and reviewers.

There are significant variations between the WGs in terms of how they went about to perform their task. Not surprisingly, the main distinction lies between WGs 1 and 3. Thus, we will concentrate our discussion on these two WGs.

In WG 1 "pure" research activities were primarily carried out by the task forces established on separate issues under the mandate of the WG and their individually appointed contributors. Input to the assessments was generated through a series of workshops, and lead authors were appointed to write the respective assessements following each issue. The assessments were then peer reviewed by scientists who had not taken part in the preceding process[59]. Due to the "scientific" nature of the task, these two phases of the process seem to have been well "shielded" against political pressures. The procedures adopted were to a large extent consistent with the norms and standards "governing" research science in general, and the level of scientific complexity in itself, served to "exclude" non-experts from participating actively in the process. The result of this process was then discussed and negotiated in the WG Plenary, where non-experts and governmentally appointed experts also participated. At the final stage, the assessment as well as the executive summaries (the summary of the assessment and the "policymaker summary") were to be accepted and approved by the Panel Plenary as the "official IPCC view". The required "approval" of the policymaker summary, implied that the document was submitted to paragraph-by-paragraph negotiations. New Scientist reported of difficult negotiations at the fourth Plenary session where the first assessment report was approved:

> "Following marathon negotiations that several times came close to breaking down, the Intergovernmental Panel on Climate Change approved its final report last week in Sundsvall, Sweden. ... Until 4 am on the last night of negotiations, however, it

[59]The 1992 Supplementary Report from WG 1 involved 115 lead authors/contributers from 21 countries, and 341 reviewers from 60 countries and NGOs (App. B, Report from the eighth Plenary Session).

> appeared that the IPCC would be unable to agree on a report. ... A summary [of the three reports], written by Bert Bolin, the Swedish scientist who chairs the IPCC, was abandoned last week as national representatives wrote hundreds of contradictory amendments into the draft. In the end, the final document was put together from earlier reports by the three working groups."[60]

Still, actors central throughout the process maintain that the scientific assessment (provided by WG 1) was not significantly changed *in substance* on its way through the decision-making levels of the IPCC.[61] All the way, however, attempts from governments as well as lobbyists (industry and environmental NGOs) were made to "water down" or "strengthen" (respectively) the conclusions of WG 1, particularly with regard to formulations concerning uncertainty. Lunde maintains that, in the Plenary, although political actors played the main roles, "it seems to have been agreed, ..., that the conclusions of the scientists (from WG 1 and 2) should remain untouched, and that the executive summaries wrapped up by, or under the scrutiny of, politicians in August 1990 [the fourth Plenary Session of the IPCC], should as genuinely as possible reflect the wording and spirit of the assessments that they were based on."[62] With regard to the First Assessment Report, however, criticism has been raised for discrepancies between the summaries and the underlying report.

With the restructuring in 1992, the distinction between "adoption" and "acceptance" was made explicit. While adoption refers to a line-by-line approval, acceptance refers to approval of the respective reports *en bloc*. The Summary for Policymakers (SPM) is *adopted* by the WG Plenary and *accepted* by the full Panel Plenary. Since the same *governmental* actors are represented in both fora, a report adopted by the WG Plenary can not be amended by the full Plenary. This institutional device is important for ensuring that the summaries actually reflect the reports on which they are based, since the lead authors are present in the WG Plenary, while they usually do *not* participate in the full Plenary. In the discussion of the reports (in WG 1) the lead authors' scientific authority is used to prevent changes in the summaries that would distort the relationship between the summaries and the underlying report. Thus, suggestions of *substantial* changes are not adopted without the approval of the respective lead authors. This implies that the rules of procedure, at least when handled with "care" (i.e., "diplomatic skills"), gives the lead authors a veto against changes that cannot be substantiated by the underlying report.

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[60]Debora MacKenzie and Joe Kerwin, "Model-makers defend consensus on climate", New Scientist, 8 September, 1990.

[61]Participating scientists in personal communication with the author. The proposition is, however, also based on the findings and interviews carried out by Leiv Lunde. See Leiv Lunde, 1991, op.cit.

[62]ibid., p. 88. For a detailed analysis of the manners in which "non-epistemic" pressures were exerted and met by scientists, see Chapter 4 of the same publication.

In WG 3 the proceedings and final outcome seem to have been significantly different than in WG 1. Particularly due to the political nature of the task ascribed to this WG[63] and the complexity implied in terms of drawing a definite line between science and politics, it should come as no surprise that WG 3 experienced a significantly more *substantial* politicisation of their work as compared to the other WGs.[64] The members were to a lesser extent scientists active in research and could to a lesser extent rely on existing networks and review mechanisms. Boehmer-Christiansen maintains that, "...the third WG became a forum mainly for government people and lobbyists..."[65] and, "... the main forum for pre-negotiations and conflicts."[66] In contrast to the situation in WG 1, where the negotiations taking place in the more "politicised" bodies were *structured* by the texts developed in the scientific core, the output from WG 3 seems to have been actually *developed* in these bodies. The political intricacies of the task ascribed to WG 3 is also reflected in their relatively greater difficulties in reaching consensus and the "diplomatic" formulations adopted in the final texts. This is, for instance, illustrated in the Norwegian Report of the final meeting in the WG 3 Plenary in June 1990, where it is reported that the meeting was "difficult and marked by bargaining" ("tautrekking"), and that the meeting showed less willingness to draw unambiguous conclusions than in the other WGs:

> "Årsaken til denne tilbakeholdenheten må søkes i det forhold at konklusjonene vil få innvirkninger på viktige områder av nasjonal politikk, vil, spesielt på sikt, kunne medføre store kostnader, og berøre policyspørsmål de fleste land ennå ikke har tatt stilling til." (to be translated).

Thus, the potential policy implications of their assessment seems to have been one factor significantly influencing their conclusions and choice of formulations.

3.3. Structure and Agent in the Scientific Diplomacy of Climate Change

Did the institutional framework serve to combine, or "balance" scientific "involvement" and scientific "autonomy", or was the one achieved at the price of the other? And, which role did performances of leadership functions serve towards this end? In other words, was the science-politics integration characterizing the IPCC at all levels achieved at the price of its scientific "autonomy" or legitimacy? And; were performances of leadership functions instrumental towards balancing the two dimensions?

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[63]Formulating response strategies inevitably involves value judgements and can, at best, be described as a "trans-scientific" question.
[64]See Sonja Boehmer-Christiansen [1993a-sett inn ref.]; Lunde, op.cit.
[65]1993a, ibid., p. 19.
[66]ibid., p. 22.

As the preceding discussion shows, scientists certainly were "involved" in the policy-making process. At several junctures in the process, scientists seem to have been confronted by the "politics" of the issue, and they also seem to have discussed their formulations with this in mind. This was clearly the case, for instance, with regard to the 1992 Supplementary Report, where WG 1 devoted considerable time to discuss the question of how their findings on the *negative* warming potential of SO2 should be presented due to its possible policy implications.[67]So the major question does not seem to be whether they were "involved" or not, but rather whether this level of involvement was achieved at the price of their scientific legitimacy.

The source to IPCC's scientific legitimacy seems to lie in the perceived autonomy of its scientific core; the task force/subgroup level of each WG, in which the assessment were drafted. As also is reflected in the preceding discussion, we have traced significant variations in scientific autonomy between the WGs at this level - particularly between WGs 1 and 3. Occurring within the same institutional framework (and thus not explained solely by this factor), we have to investigate the impact of other factors to explain the variation. Two crucial questions structures the discussion: i) Which "effect" (the politicisation of WG 3 versus the scientific legitimacy of WG 1) can be ascribed to the institutional framework? ii) Which "other factors" seem most important for explaining the variation?

The institutional framework of the organization does, in fact, seem to have contributed to both the politicisation of WG 3 and the scientific legitimacy of WG 1, implying therefore, that there are intermediate variables here, determining the effect of institutional arrangements. One important factor in this regard seems to have been the functions served by the WG leadership.

The political institutional framework of the IPCC left the process "vulnerable" to a substantial politicisation (implying a decreased scientific legitimacy) of the work taking place within its scientific core. The "politicised" recruitment principles seem to have been the most important institutional device in this respect. The principle of open entry (governmental nomination) implied that politically nominated participants, in principle, had access to bodies at all levels, including the scientific core. On the other hand, the operational autonomy of the WGs was significant. The leadership of each WG could decide on aspects such as the division of labour within the group, selections of lead authors on the respective areas and nominations of contributors and reviewers. Thus, the institutional framework provided the leadership of each WG with significant leeway for "controlling" the process, at least as far as participation in the development of assessments is concerned. In WG 1, this

[67]Personal communication with participants. Other examples that could be mentioned in this context, is the development of emission scenarios both in 1990 and 1992, and the related controversy on the appropriateness of paleo-analogs as a technique for climate forecasts.

potential was used to "control" the process *scientifically*. In WG 3, before 1992, this potential was to a much larger extent used to "control" the process *politically*. The political pressure towards WG 3 was much stronger than in the other WGs, due to the nature of their task. This WG, therefore, also probably had a much harded time doing the "boundary work" of negotiating the boundaries between science and politics, and maintaining an acknowledged "scientific" authority. In a comment to New Scientist, Bolin himself said; "It might have been over-ambitious for the IPCC to hope that the response strategies recommendations would be able to avoid the politics ... and stick to the science."[68] Things were not made easier by the WG leadership, however, since the chairman of this WG also served as the head delegate of the US delegation to the INC, one of the "least enthusiastic parties", at the time, in the climate change negotiations. This combination of functions was probably in itself a barrier towards maintaining this group's scientific credibility.

The increasingly politicised nature of WG 3 may, however, also have served to facilitate the "boundary work" in WG 1. That is, the politicisation of WG 3 may have contributed to the accepted scientific legitimacy of WG 1. During the first phase of the IPCC process (from 1988 to 1990) there did not exist a political forum where the "politics of the science" could be discussed. This function seems, however, to have been served by WG 3. The incorporation of this body may have served to provide a forum, within the IPCC context, to which discussions of the political aspects of all issues under the IPCC mandate could be channeled, and thus also served to reduce the political pressures directed towards the other WGs. With the political nature of the institutional framework of the IPCC, the WG 3 establishment may thus have served as a facilitating condition for preventing ("pure") politics from penetrating and probably obstructing the whole process. There was a need for a political forum, but this could not be formally established before the phenomenon in question (human induced climate change) was politically accepted as actually representing a problem requiring some sort of political response. Thus, political deliberations on interpretations of the science were "pooled" in WG 3[69].

In the event, the institutional structure of the IPCC effort, also implied a significant potential for the WG leadership to serve a "communicative" and "mediator" function between the various bodies of the IPCC. As is emphasised above, the outcome of the IPCC effort was thoroughly negotiated at all levels between different groups of participants. While politicians dominated the plenaries, scientists (although to a lesser extent in WG 3) dominated at the task force/subgoup level. The leadership of the WGs seem to have served an important function as the major "linking mechanism" between decision-making levels. As far as I have

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[68]MacKenzie and Kerwin, 1990, op.cit.

[69]This interpretation has been supported by several participants, although they also have emphasised that this effect most probably was not intentional when the WGs were set up.

been able to bring to my knowledge, apart from the IPCC Chairman himself, the leadership of the WGs were the only actors represented at all levels. It seems as if some of these actors (associated to WG 1 in particular) have served an important communicative or "mediator" function, by "negotiating science" to politicians at the upper levels and "politics to scientists" at the lower levels of the organization.[70] Particularly the former, has been commented upon, in writings about this process: "Besides learning how to talk science to politicians, scientists have learnt not to play at being politicians."[71]; "The real news ... is that some scientists have learnt how to talk to politicians."[72]; "Fred Bernthal, head of the US delegation [and chair of WG 3], tried in Sundsvall to amend the scientific report to stress its uncertainty. He was opposed successfully by John Houghton [now Sir], the head of Britain's Meteorological Office and the chairman of the scientific working group."[73]

This also brings in the aspect of learning. Several representatives of the leadership in WG 1 (including lead authors) had some experience with this kind of process, from 10-15 years of ozone "bargaining". Thus, prior to the IPCC process, they had experience in "talking to politicians". One key actor in this respect, is Robert Watson of NASA. It has been said about Robert Watson that, "...if [he] had been hit by a bus in 1980, we would not now have a treaty to save the ozone layer."[74] Robert Watson has been involved in the IPCC process since its start as a lead author in WG 1, and serves now also as chair of WG 2 (since 1992). Moreover, the organizers of the IPCC themselves, the Executive Council of WMO and the Governing Council of UNEP, were the same as in the ozone process[75]. In this respect, it seems reasonable to argue that the IPCC process constitutes the *continuation* of the ozone process, and to assume that the experience made in the negotiations of the science on ozone also had implications for the organizational model adopted for the IPCC framework. One main difference between the ozone and the climate processes, is the level of institutional integration between science and politics characterizing the *initial* phases of the climate process. Within the ozone context the intitutional integration between science and politics evolved over time[76]. Although hypothetical, it seems reasonable to assume that the climate

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[70]This is a *preliminary* conclusion which only can be tested by interviews with participating actors. Interviews on a broad scale have not yet been carried out.
[71]Comment, New Scientist, 8 September 1990.
[72]Editorial, New Scientist, 19 November 1988.
[73]Debora MacKenzie and Joe Kerwin, 1990, op.cit.
[74]Debora MacKenzie, "How to use science and influence people", New Scientist, 29 April, 1989.
[75]For a detailed account of the networks involved in these processes, see for instance Sonja Boehmer-Christiansen (1994), Global Climate Protection Policy: The Limits of Scientific Advice. *Global Environmental Change*, 4 (2), 140-159.
[76]Although intimately linked, the scientific process on ozone was not *formally* integrated in the policy-making process until 1988; three years after the adoption of the Vienna Convention, and after the agreement on the Montreal Protocol (agreed upon in 1987, in force since 1989).

process would have been organized differently without the experience made in the preceding ozone process.

Scientifically, the climate and ozone issues are intimately related. This linkage also seems to have had implications for the negotiations of the science on climate change in terms of providing forums for discussing the science of climate change outside the formal framework of the IPCC process. When WG 1 scientists started their work on the first assessment report, there existed a scientific network external to the IPCC framework without open entry, that also could serve as arenas for scientific debate. In this manner, the scientific linkage between climate change and ozone depletion research seems to have been utilized as a "tool" for moving scientific deliberations to other, external forums, to which the politically nominated participants of the climate process did not have access. Thus, "informal" forums for "negotiating" scientific controversies outside the political context of the IPCC were provided. This may, however, also have served as an instrument for increasing the "zone" of politically accepted conclusions within the IPCC context. The science on ozone is to a much larger extent politically *accepted* than the science on human induced climate change, and has resulted in substantive actions on the part of governments. By coordinating the two processes (or "ensuring consistency"), scientific conclusions, agreed upon and politically accepted within the ozone context, could to a larger extent be presented within the climate context as accepted "facts", despite their increasingly politically controversial nature within this context.[77]

This discussion shows the complexity and the inter-related nature of institutional structure and actor behaviour. In this particular context, the effect of the institutional framework seems to have been *contingent* upon the performance of leadership functions. The institutional feature which served to provide "room" for performances of leadership roles, the operational autonomy of the scientific core, also left the process "vulnerable" to politicisation and decreased scientific legitimacy. Still, this institutional device, giving "room" for a skilful performance of "scientific diplomacy" by the WG 1 leadership, was instrumental towards combining scientific autonomy and scientific involvement. Scientific involvement was achieved through an institutional integration between science and politics, while scientific autonomy was maintained through skilful leadership performance. The performance of leadership was, however, in its turn, made possible through institutional devices such as the operational autonomy of the scientific core.

[77]In personal communication with the author, one participating scientist has suggested that this was a concious "strategy" on the part of the scientists engaged in both processes, and constituted part of the motivation for the linkage.

4. Concluding Remarks

It is not hard to find explanations as to why the outcome of the IPCC process not yet has resulted in regulatory commitments on the part of governments. The political setting of the issue is extremely complicated and conflictual, not least because the "traditional", and largely ideological, north-south conflict is activated. Moreover, there are severe conflicts within the "blocs" of north and south, implying rigorous positions in the negotiations as the positions already at the outset, are compromises. This counts for the south in particular, as they to a larger extent than the north have tried to act as one bloc. It is obvious that a coalition consisting of both the oil producing Saudi Arabia and the poorest, oil importing, countries in Africa is bound to have problems in coordinating their positions. The northern "bloc", or the industrialized countries, have generally accepted that the main responsibility for bearing the burdens of regulatory measures to mitigate global warming is theirs. There are, however, severe conflicts over what constitutes a "fair" burden-sharing among these countries, due to large variations in energy structures, energy-efficiency etc. Thus, the opportunities and risks that CO_2 emission reductions may offer, are not at all shared equally among the industrialized countries. Major actors, therefore, have differing incentives to use both scientific uncertainties and scientific findings as tools for energy policy.

The science on a human induced climate change has certainly been "transformed" to politics, although the extent to which it eventually will serve as a decision premisse remains to be seen. In some respects, it already has served as a decision premisse, however, since the perception of human induced climate change as a political problem rests entirely on a scientific foundation.