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**«Scientific»  
management of  
the environment?  
Science, politics and  
institutional design**

*Steinar Andresen, Tora Skodvin,  
Arild Underdal and Jørgen Wetterstad*

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by

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## PREFACE

This report is a product of a research project undertaken jointly by Tora Skodvin and Arild Underdal - both at the Center for International Climate and Energy Research, Oslo (CICERO) - and Steinar Andresen and Jørgen Wettestad - both at the Fridtjof Nansen Institute (FNI).

We have divided the work as follows: Arild Underdal has had the main responsibility for developing the conceptual framework and the research design (chapter 1). He has also written the final version of chapters 2 and 8, on the basis of drafts by Skodvin (chapter 2) and Andresen and Wettestad (chapter 8). Tora Skodvin has been responsible for the case studies on ozone and climate change (chapters 6 and 7), Jørgen Wettestad for the case studies on the North Sea (chapter 4) and acid rain (chapter 5), while Steinar Andresen has written the chapter on whaling (3).

In writing this report we have benefited greatly from inputs in the form of information, comments and suggestions from a number of people. The most important providers of information are listed in footnotes to the case study chapters. In addition, we want to acknowledge the editorial assistance of Ms Ann Skarstad. Needless to say, the full responsibility for the present report rests with the four authors.

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Oslo and Lysaker, July 1994

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# "Scientific" Management of the Environment? Science, Politics and Institutional Design

## I Purpose and Scope

### 1.1. The question to be addressed

Rational policy-making requires that decisions be based on the best knowledge available about the problem itself and possible "cures". Knowledge is clearly a *necessary*, though not a sufficient, condition for rational management of natural resources and the environment. Scientific research is at least one major supplier of "advanced" knowledge. Hence, rational management of environmental resources requires that findings from relevant research be made available to decision-makers, and also that they be "translated" into, and actively utilized as, premisses for policy decisions.

This science-politics relationship seems, however, to be very sensitive to stress and several kinds of disturbances. For one thing, research findings rarely "speak for themselves", and inferring policy "implications" from findings or hypotheses is by no means a straightforward operation.<sup>1</sup> Nor is it one for which the research community itself can be assumed to be well prepared or command particular skills. Moreover, forging an interactive relationship between science and politics involves a significant risk that research itself become "contaminated" by political controversy, and that scientists be "exploited" as mere mouthpieces of their political masters. The ideal relationship seems to be one where the research community respond to *questions* raised by decision-makers, but *not* to any preferences they might have with regard to the substance of the *answers*. Constructive use of inputs from scientific research in the making of environmental policy decisions seems to require that we find some way to combine or balance the integrity and autonomy of the scientific undertaking with "involvement" in the practical problems faced by decision-makers and responsiveness to the "needs" of decision-makers for "diagnostic" and "therapeutic" knowledge. Conversely, it also requires that decision-makers find some way not only to utilize the knowledge and insights that science has to offer, but also to ask policy-relevant questions without "ordering" particular answers or in other ways interfering with the professional work of the scientific community itself.

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1 Strictly speaking, we can, at best, talk about *conditional* "implications", i.e. prescriptions that can be derived *given* certain values or goals.

This report explores *how* such a relationship may be accomplished. More precisely, we explore to *what extent and how the use of findings from relevant research as inputs into decision-making processes is affected by the way the science-politics "dialogue" is organized.* Our dependent variable is the extent to which conclusions from scientific research are *adopted* (undistorted) as (consensual) premisses for policy decisions. Our independent variables are certain aspects of the institutional setting, i.e. organizational arrangements and procedures. Our reason for focusing on institutional arrangements is not an assumption that these are necessarily the most important determinants of the rational use of knowledge, or account for most of the variance that we can in fact observe in a given sample of cases. Rather, we focus on organizational aspects mainly because they are social constructs that can, at least in principle, be deliberately designed and manipulated by the actors themselves. Organizational arrangements and procedures constitute one class of instruments that decision-makers can use in order to achieve a particular goal.<sup>2</sup> In order to tap the potential of institutions-as-instruments, we need to understand *how* different institutional arrangements actually work, and *what* makes one work differently from another. The purpose of this study is to contribute to answering these basic questions.

In what follows we shall first try to be somewhat more precise about our dependent variable; what do we mean by the "adoption" of conclusions from research as decision premisses, and how do we determine to what extent such adoption has in fact occurred? Then we shall identify those aspects of the institutional setting that we are going to examine in this study (i.e. our independent variables), and introduce three "non-institutional" factors that we believe can affect the extent to which inputs from science are adopted by decision-makers; the latter factors are treated as control variables in our design.

## 1.2. The dependent variable

We propose to conceive of the "adoption" of findings and hypotheses from research in terms of a cumulative scale with three levels. At the first and lowest level decision-makers "tune in" to science; i.e. they recognize the relevance and usefulness of the kinds of knowledge that scientists produce, and look to the scientific community for information, models, and theories. Scientists are considered competent experts, and decision-makers have a substantial amount of confidence in science as "project". At the second level decision-makers also accept as valid

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2 We do realize that this instrumental perspective is open to challenges. Thus, Scott (1981) distinguishes three basic conceptions of organizations; viz. organizations as "rational", "natural", and "open" systems. In his words, "Rational systems are designed, but natural systems evolve; the former develop by conscious design, the latter by natural growth; rational systems are characterized by calculation, natural by spontaneity" (Scott 1981:101). The description of "natural" systems seems to apply quite well to the scientific community. Even so, we assume that when it comes to organizing the science-politics interaction, there is *some* scope for conscious design and calculation.

or tenable the *substantive conclusions* that meet the standards of the scientific community itself. Whatever the transnational community of scientists considers, by general consensus, to be the best knowledge so far available is accepted as such also by decision-makers. At the third level decision-makers accept not only "factual" conclusions but also what might be called the "policy implications" of these conclusions, and respond "positively" to more explicit advice offered by the scientific community. At this level inputs from science are "adopted" even in a quite literal sense by decision-makers.

This scale can be considered cumulative in the sense that higher levels can be reached only through the preceding steps. It is hard to imagine that decision-makers will accept the curative implications of a diagnosis they reject,<sup>3</sup> and it is equally hard to imagine that they will accept the substantive conclusions offered by the scientific community unless they place at least a certain minimum of confidence in the professional competence and moral integrity of that community.

Even though we conceive of these as distinct levels of "adoption", we do recognize that at each level "adoption" is a matter of degree. Clearly, the amount of confidence in the validity of a certain proposition or diagnosis can vary significantly. Similarly, decision-makers may very well "respond positively" to a certain diagnosis or piece of advice without implementing "die Endlösung" of the problem. In fact, we shall consider even a small step to cure or alleviate the problem as a "positive" — although not necessarily as the "optimal" — response. In the empirical analysis we will not be able to measure the degree to which inputs from science are adopted precisely at any of these levels. We nevertheless believe that this crude scale provides a useful "conceptual grid" for empirical research, enabling us to compare cases in "soft" terms that makes sense also to the practitioners concerned.

Finally, we should point out that the concept of 'adoption' refers to an intellectual operation that cannot be observed from the outside. We shall therefore have to base our scores on indirect evidence, based on what actors say and do.

### 1.3. Independent variables

We have already suggested that a key to a constructive relationship between science and politics is to combine — or strike an optimal balance between — the autonomy and integrity of science on the one hand and involvement and responsiveness to questions and concerns of decision-makers on the other. Confidence in scientists and their findings seems to rest on two basic assumptions: competence and integrity. The principal reason why decision-makers and the attentive public look to science for information and guidance is confidence in the

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3 It is, though, conceivable that they will accept a piece of advice *for some other reason*, e.g. if it happens to coincide with the implications of another "diagnosis" to which they subscribe.



competence of scientists as producers and managers of "advanced" and reliable knowledge. Similarly, one main reason why they often accept the conclusions produced by research as valid is confidence in scientists as "truth-seekers", strongly committed to the logic and professional norms of scientific inquiry, and collecting and analysing evidence independent of all substantive interests that any government or other external actor may pursue. Although some individual researchers may fail to meet these high standards, decision-makers may have confidence in the ability of the *community* of scholars — through its pluralism and procedures of critical peer review — to correct such aberrations.<sup>4</sup> All this suggests that confidence in the autonomy and integrity of science are, or at least come close to being, necessary conditions for "adoption" at levels 1 and 2 above. For decision-makers to respond positively to policy "implications" or explicit advice (level 3 adoption), a certain amount of responsiveness to the questions and concerns of decision-makers and a minimum of active involvement — at least in the form of formulating and communicating "implications" — is likely to help.

The opening formulation of this section hints at one important question: to what extent are autonomy and integrity compatible with involvement and responsiveness? Can autonomy and integrity be preserved only at the expense of isolation in the "ivory tower" of Academia? This question is a complex one, and calls for a more thorough analysis than we can offer here. We shall return to this question in the concluding section. Suffice it here to indicate that our initial assumption is that up to a certain level involvement and responsiveness are indeed compatible with autonomy and integrity. However, we also believe that there is a price to be paid for strong and direct involvement in the policy-making process, and that some sacrifice of autonomy will be part of that price. Looking at the science-politics interface in this perspective, the optimal relationship seems to be one where science enjoys great autonomy and engages itself only to a moderate extent and perhaps only indirectly in the policy-making process.

### ***1.3.1. Factors affecting autonomy/integrity***

The autonomy and integrity of science may be conceived of as a function of multiple variables. The list below is by no means exhaustive, but it is offered as a check-list for the empirical case-studies to follow.

- 1) Who select(s)/appoint(s) and fund(s) the scientists involved?
  - (Non-governmental) scientific organization(s), independent of the *regulatory* body.
  - National governments
  - Intergovernmental organization, i.e. the regulatory body.
- 2) What are the principal criteria for selection of participants:

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4 In fact, of course, the scientific community may to some extent have its own particular interests, pertaining to funding of research, formalization of status etc. (see e.g. Boehmer Christiansen 1993a).

- Is entry open to any participant nominated by a member country, or subject to (firm) restrictions?
- To the extent that entry is restricted, what are the principal criteria for appointment: scholarly merits vs. other criteria (nationality, ideology, formal position, etc.)

3) Operational autonomy:

- To what extent are the scientists, considered as a group, free to organize their own work? Can they, for example, set their own agendas and decide on the division of labour and the allocation of specific roles such as principal investigators, rapporteurs etc?

4) Main function:

- Production of new knowledge; participants are actually conducting research
- *Coordinating* (and supervising) research
- Translating existing knowledge into decision premisses; main contribution is to elaborate "policy implications" of existing knowledge.

5) Unity and homogeneity of scientific body/network:

- Does the network have its own "institutional basis", i.e. some internal coordinating device?
- Are all members scientists (i.e. people presumed to spend a substantial portion of their time in active research), or does it also include administrators and other "non-scientists"?
- Do all or at least most members have a similar disciplinary background?

These dimensions may be combined in an index, as indicated in table 1:

*Table 1: Index of autonomy/integrity.*

<b>HIGH autonomy/integrity</b>	<b>LOW autonomy/integrity</b>
Selection by and funding from scientific organization or from IGO itself	Selected and funded by national governments
Recruitment based on scholarly merits or role in scientific community	Recruitment based on political position or administrative office
High operational autonomy; sets its own agenda and organizes its own work.	Under effective instruction and control by governments
Participants engaged in active research, or at least in coordination of active research	Mainly concerned with policy implications
Independent internal coordination; homogeneous	No independent institutional basis; heterogeneous

### 1.3.2. Factors affecting involvement

Similarly, the *involvement* of the research community in the political process can be seen as a function of dimension 4 above, and the variables indicated below. (*Responsiveness* is an attitudinal variable, but two of the subdimensions under 7 capture what may be the most important organizational determinants or indicators.)

#### 6) Functional differentiation: coupling between research and advice

- To the extent that the framing of policy advice is part of the scientists' agenda, do the same scientists that are supposed to *produce* knowledge also formulate explicit advice, or are the functions separated in some way?
- To the extent that the functions are separated, how great is the "organizational distance" between the actual research activities and the formulation of policy advice?

#### 7) Formal links to decision-making body

- Does the "scientific network/body" have a regular channel for communicating findings and advice to the decision-making body?
- Is there a regular channel whereby decision-makers can put questions to the scientists involved?
- Is there a regular dialogue about conclusions between scientists and decision-makers, or at least some kind of feedback from decision-makers (in the form of requests for more information, follow-up questions, criticism or support, etc.)?

Table 2: Index of involvement

HIGH involvement	LOW involvement
Scientists formulate policy advice	Scientists engage in research and research management only
No or little organizational "distance" between research and advice functions	Great organizational "distance"
Regular channels, working both ways	No regular channels

Clearly, tables 1 and 2 outline "ideal type" constructs. For most of the actual cases to be analyzed here intermediate scores will probably apply. For example, the specificity of policy advice provided may vary considerably. In some cases, scientists would only indicate what would be required to achieve a certain hypothetical goal — e.g. the stabilization of CO<sub>2</sub> concentrations in the atmosphere — without thereby *prescribing* the goal of stabilization itself. In other cases scientists may take steps towards recommending a certain target and/or

prescribing one particular strategy for achieving this target. Other things being equal, we would consider scientists in the latter category to be "more involved" in the policy process than the former.

Suggesting that the factors listed can be combined to form indexes as we have done in tables 1 and 2, immediately raises the question whether or not all factors can be considered equally important. In the comparative analysis we shall have to rely heavily on aggregate assessments of each case, but in the individual case studies we shall look also for evidence that might indicate that one particular factor or mechanism was particularly important. An even more intriguing question is whether two or more of these factors can *interact* to produce effects that cannot be attributed to any of them individually. Can we, for example, point to one particular combination of high *and* low scores on different dimensions of autonomy that seems particularly productive? What if we have a strong core of "independent" scientists and add a set of administrators or scientists nominated by governments as their representatives? Would the former educate or "socialize" the latter, or would the political agents "contaminate" the scientific process? To the extent that the core of independent scientists dominates the interaction, we might see the development of a particularly potent "epistemic community" (Haas 1990; 1992), where the "supervisors" become converted into de facto agents of a transnational coalition, and — by virtue of their links to their respective governments — also exercise more influence on their domestic arenas. These are very complex questions that we cannot explore in depth here. The point we wish to make at this stage is simply that the indexes outlined in tables 1 and 2 should be considered primarily as crude check-lists for comparative analysis, and not used mechanically as calibrated instruments capable of providing exact measurements of autonomy and involvement.

#### 1.4. Control variables

We have already explained that our interest in *institutional* arrangements is not based on the assumption that these are necessarily the most important determinants of the adoption of inputs from science as premisses for policy decisions. A wide range of other factors may be equally or even more important. In the analysis we have decided to pay particular attention to three of these:

- 1) The "state of knowledge"; in particular the conclusiveness of available scientific evidence and the scope of uncertainty and ambiguity of findings reported in previous research. The general assumption is that — other things being equal — the less conclusive the evidence, the less likely that it will be utilized as a basis for joint policy decisions.

- 2) The political "malignancy" of the problem itself. Other things being equal, the more "malign" the problem, the more conflict it will generate, and the less likely that even fairly conclusive evidence will be generally accepted as a basis for collective action.
- 3) Public saliency of the problem. High saliency probably tends to increase the demand for "more" knowledge and inputs from science, at least in so far as politically "benign" problems are concerned. For the most "malign" problems, however, high public saliency may serve to "politicize" the issue even more strongly, thereby creating a climate in which the rational and systematic use of inputs from science becomes very difficult.

### **1.5. Outline of the empirical analysis**

After a brief survey of conclusions and hypotheses that may be extracted from previous research in this area (section 2), we present — in summary fashion — five case studies, all focusing on decision-making within international regimes for the management of resources and the environment. We do believe that this small sample of cases will offer a sufficiently wide range of variance in terms of our dependent as well as our independent variables (and, for that matter, also control variables) to allow us to shed some light on our basic question. However, the reader should be advised that what follows is essentially an exploratory exercise into a poorly charted terrain. We embark upon this exercise without a well specified model from which precise hypotheses can be systematically derived, and also without a well-developed set of methodological tools enabling us to make exact measurement. The conclusions we have to offer at the end should therefore be seen as tentative, indicating focused questions and promising hypotheses calling for further research.

## II Do Institutions Matter? A Brief Survey of Previous Research

### 2.1. Introduction

Before examining our cases, it seems appropriate to explore what may be learnt from previous studies about the impact of institutional arrangements upon the adoption of inputs from science as decision premisses. Unfortunately, the question of how *specific* institutional variables affects the transformation of scientific evidence into inputs for environmental policy-making has not been studied in great depth and detail. Some conclusions may, however, be drawn from the literature on the science-politics interface more generally, and from some case-studies focusing on environmental protection and resource management more specifically.

In this "state-of-knowledge" survey we shall first offer a set of propositions identifying some of the circumstances under which findings from scientific research can be expected to influence policy decisions. From there we shall move on to focus on the impact of some of the specific institutional variables listed above. As the focus narrows, we shall have to look for implicit and tentative suggestions as much as explicit and definite conclusions.

### 2.2. Circumstances under which science is likely to affect policy

The most obvious conclusion that we can extract from previous studies is that decision-makers *sometimes* respond to new scientific evidence, and sometimes not. Accordingly, a good starting point seems to be to search for the *conditions* under which scientific evidence is likely to serve as premisses for policy decisions.

In concluding an international symposium organized by the Fridtjof Nansen Institute on the role of science in international resource management, Underdal (1989) suggested nine *ceteris paribus* propositions referring to this question (table 3, next page). More specifically, he suggested that the nature and quality of the input (propositions 1-2), the substantive content of the input (propositions 3-6), and the political setting into which the input is fed (propositions 7-9), all may have decisive impact on the extent to which science influences the outcome of international negotiations.

Now, our primary concern in this study is not the conditions under which conclusions from research are likely to serve as inputs into policy-making processes, but rather what difference the institutional setting can make in this respect. In order to examine the latter question, we

need to distinguish the impact of institutional variables from the impact of other relevant factors.

Table 3: Conditions affecting the impact of scientific inputs

Impact likely to be	
Strong	Weak
"Definite" or at least consensual conclusion	Tentative or contested hypothesis
Feasible "cure" available	"Cure" unclear or not feasible
Effects close in time	Effects remote
Problem affecting "social centre" of society	Problem affecting "periphery" only
Problem developing rapidly and surprisingly	Problem developing slowly and according to expectations
Effects experienced by or at least visible to the public	Effects not (yet) experienced by or visible to the public
Political conflict: low	Political conflict: high
Issue linkage: none or on substantive merits only	Tactical issue linkage: issue "contamination"
Institutionalized setting, iterative decision-making	Not yet institutionalized, <i>ad hoc</i> decision-making

Source: Underdal, The Politics of Science in International Resource Management: A Summary, in Andresen & Østrem (eds.), *International Resource Management* (1989), p. 259.

### 2.3. The policy-making process

For this purpose, it seems useful to split the policy process into three consecutive stages (see also Boehmer-Christiansen, 1989). Breaking the process down into phases can help point us towards the different *functions* that inputs from research may serve and institutions be

designed to facilitate. Moreover, the role of inputs from science can be expected to vary from one stage to another (Parson, 1991):

- I Problem identification and diagnosis; agenda-setting. At this stage science may — at least under the circumstances identified in table 3 — be expected to play a major role.
- II Selection of policy response; decision-making. This is the stage of bargaining, and the role of research as a supplier of inputs is likely to be marginal.
- III Implementation and enforcement, evaluation — possibly leading to renegotiation and adjustment. Here science may be expected to be a more important supplier of inputs than at stage II, but probably less important than at stage I.

Problem identification is obviously a precondition for problem-solving. Science usually plays an important role in identifying and diagnosing problems and in setting agendas. In most cases, research is the principal supplier of the knowledge required for problem identification and problem diagnosis. Science clearly serves as a major supplier of inputs also for developing effective "cures" and "enabling" actors to adjust (essentially by developing new technologies). However, in the actual *selection* of specific policy measures, the critical criteria seem to be normative and "political" rather than "factual". Consensual knowledge may certainly restrict the range of legitimate positions and arguments, and diverging findings or hypotheses may be invoked in support of conflicting interests. Moreover, models developed by scientists are sometimes used as tools for predicting the consequences of alternative solutions (see e.g. Sebenius 1984). But rarely if ever can the specific policy measures adopted be "derived" in any straightforward sense from the scientists' conclusions. Once a decision is made, science may again serve an important function in providing a knowledge base for implementation and also in evaluate the effectiveness of existing policies and in facilitating "learning".

## 2.4. Problem identification and "diagnosis"

### 2.4.1. *Developing consensual knowledge*

A major objective in the pre-negotiating phase is to develop a platform of consensual knowledge that can be accepted by all (pivotal) parties as the best knowledge available for developing and evaluating policy options. Underdal suggests that "at least in international negotiations, *consensual* knowledge seems to be more important than 'advanced' knowledge." (1989:259). This is so because "...substantial uncertainty about cause-effect relationships may serve as the political equivalent of live ammunition in the hands of actors opposed to (new) regulatory measures." The implication of this statement is not that we should aim at *eliminating* uncertainty; perfect information is usually beyond reach in areas such as environmental management. Moreover, we can most often make perfectly sensible choices



without it. Rather, the critical minimum of knowledge required is that which is needed for (pivotal) actors to agree on what qualifies as "sensible" action.

Empirical studies indicate several ways in which the institutional framework may affect the prospects of developing consensual knowledge. Three factors in particular seem to merit attention: recruitment principles, "organizational distance" to the regulatory (decision-making) body, and the structuring of the agenda.

One important aspect with regard to *recruitment and participation* concerns the extent to which entry is open or subject to firm restrictions. If entry is open, there are no instruments by which non-scientists or politically appointed actors may be excluded from the process. Open access therefore seems to increase the risk that the scientific process itself will become politicized, meaning that conflicting interests will penetrate and "contaminate" the professional exchanges that go on within the research community itself. In her studies of the international management of whaling, Peterson found that this may be the case with the Scientific Committee of the International Whaling Commission: "Participating governments are supposed to send qualified experts, but these experts can be drawn from the ranks of whaling industry employees or environmentalists rather than from any scientific epistemic community." (1992:183). This conclusion is supported also by the studies conducted by Fløistad (1989, 1990). On the other hand, Fløistad found that another scientific organization (The International Council for the Exploration of the Sea, ICES) — operating under similar criteria for appointment — has suffered less from such "contamination" (1989). One important part of the explanation seems to be that the regulation of whaling turned out to be a much more conflict-ridden issue than fisheries management. But the fact that ICES already had a long tradition of regular "business" probably also served to imbue the organization with a distinct culture of professionalism, making it more resistant to politicization.

Furthermore, several case-studies indicate that scientific consensus-building in international resource management calls for broad participation, involving experts not only from the countries championing joint action. As Timberlake points out: "Though science is theoretically value-free, scientists are not" (1989:120). The fact that scientific dissent tends to reflect political conflict at the governmental level (Andresen, 1990, 1991, Litfin, 1991) indicates that for science to play an independent and significant role, consensus must prevail not only within national networks of scientists, but within the transnational scientific community at large. Boehmer-Christiansen argues that scientists act not only for the benefit of the environment, but also to promote their own professional interests: "Organised science consists of powerful, often competing institutions dependent upon public or industrial funding" (1989:149; see also 1993). Control over the fortune of research institutions provides a basis for various forms of intervention into national research efforts. By contrast, "...governments cannot, ... determine the outcome of scientific research based on international cooperation and thus, when using this strategy, leave themselves open to persuasion and pressure" (Boehmer-Christiansen 1989:158).

Thus, for consensual knowledge -defined as "knowledge which is accepted as a basis for policy by groups and individuals professing differing political ideologies" (Miles, 1989:49) - to emerge, the scientific process itself must include a sufficiently wide range of participants that it can not easily be dismissed as a partisan "clique" operating as advocates-in-disguise for particular governments or interests.

This leads us to the aspect of *organizational distance* between the scientific and the regulatory bodies of a regime. Miles found that one of the most important factors in scientific consensus-building is the presence or absence of direct distributive or regulatory links, and that "indirect rather than direct links to management decisions will facilitate the emergence of consensual knowledge" (1989:49). Particularly in situations where the level of political conflict is high and research findings are seen as having immediate implications for actor interests, direct links to the regulatory body may "contaminate" scientific research. As Underdal points out: "...political conflict tends to exploit and even reinforce uncertainty by infusing political energy into diverging interpretations of available evidence." (1989:262). Thus, some sort of organizational "buffers" between the scientific and the political processes may prove instrumental in order to make sure that the former can proceed according to its own professional standards and operating procedures, insulated from the disturbing stresses of whatever political controversy that may surround it.

In some cases the impact of political conflict may be reduced also by careful *organization of the agenda*. In his studies on scientific cooperation in the polar regions, Østreg found that the impact of political conflict, with regard to scientific *as well as* political cooperation in Antarctica, was significantly reduced as a consequence of the sequence in which issues were addressed by the scientists (1989). Scientific cooperation began in non-controversial issue areas and was only gradually expanded as confidence and trust grew among the parties. Because of this sequential and incremental approach, "conflict in one issue area (sovereignty and territorial claims) did not preclude cooperation in another (science)" (1989:98). In turn, success in establishing cooperation in the area of research also served to facilitate cooperation in other areas: "Political cooperation was ... developed primarily as a superstructure to, and a result of, scientific cooperation" (ibid.).

#### **2.4.2. Communicating "advice" to policy-makers**

Scientific consensus must not only be developed; relevant conclusions must also be communicated to policy-makers, and policy-makers must try to transform such information into premisses for policy decisions. Since communication is essential, the *channels* through which it occurs may themselves be important. Here scientists may face a real dilemma: by involving themselves in the process of inferring "policy implications" and advice, scientists may risk their reputation as scientists. As pointed out by Young, scientists involving themselves directly in the political process immediately run a risk of being "...exploited by

players whose grasp of this process far exceeds their own" (1989:14). The further scientists go in terms of formulating policy "implications" or offering advice, the further they go beyond the domain of science itself, and the more deeply they get involved in making value judgements and trade-offs, thereby trespassing the domain of politics. On the other hand, "if they remain aloof from the political process, they must accept the risk that their findings will be used in a seriously distorted manner by those desiring to promote partisan causes." (Young, 1989:14). Institutional arrangements can to some extent mitigate this dilemma. On the basis of careful empirical analysis, Edward L. Miles has suggested that the decision-making process should be "...deliberately designed to provide a buffer between research results and their utilization for regulation and - especially - for the distribution of benefits and/or apportionment of costs" (1989:50). Ruggie has argued that scientific evidence may serve as an important determinant to political responses when "...the issue is to *discover* or *understand* some process or situation. When, however, the issue is to *manage* some process or situation, the weight of political purposes becomes preponderant" (1975:558). In a similar vein, Larkin maintains that (with respect to management of marine resources) a distinction should be made between the *research* biologist and the *management* biologist: "The research biologist should be involved in advancing theory while advice on specific application should come from the management biologist" (1988:281).

These observations all suggest the establishment of some sort of "mediative function" between the scientific and political processes. As Underdal points out, "...the use of research findings in the making of management policy often seems to depend on some kind of mediating agent or amplifier other than the scientists or the ultimate decision-makers themselves" (1989:264). Furthermore, he notes that several of the case-studies examining this problem "...suggest that institutionalized links such as the former Liaison Committee of ICES/NEAFC and the Policy Working Group of the Second International Conference on the North Sea provide an optimal setting for utilizing inputs from scientific research" (ibid.). Empirical work seems to suggest — as an ideal solution — a threefold organizational structure: one strictly scientific body providing knowledge and producing evidence, a political body responsible for evaluating options and making policy decisions (determining the distribution of costs and benefits following regulatory measures), and a mixed body — possibly including "management scientists" and administrators — "mediating" between the former two.

## 2.5. Decision-making

Students of international regimes have paid scant attention to the role that inputs from research can play in the process of choosing among policy options. Science is clearly important in developing and identifying options, but not, it seems, when it comes to choosing among relevant policy alternatives.

Several scholars suggest that the kind of inputs needed at this stage would come mainly from the social sciences rather than the natural sciences (Boehmer-Christiansen, Young, 1989). As Young points out, "Whereas natural scientists can sound the alarm with regard to acid precipitation, ozone depletion, or global warming, social scientists can point to individual incentives and institutional pressures that, in combination with advances in science and technology, are likely to necessitate a restructuring of existing resource regimes or the creation of new regimes where none now exists" (1989:11). However, with the possible exception of the recent attempts to design regulations under the LRTAP regime on the basis of the "critical loads" concept, we know of no instance where social science inputs have been systematically incorporated into the process of making international regulatory decisions in the area of resource management.

## 2.6. Implementation

In the implementation of policy decisions, research-generated knowledge again seems to serve an important function. Previous studies indicate that science may serve also other functions in this phase; in addition to the regular provision of updated knowledge, a scientific body may serve the function of "supervising" or monitoring agent, in evaluating the effectiveness and possibly also the efficiency of the regulatory measures that are adopted. Monitoring and evaluation seem to call for institutional arrangements different from those that are recommended for stage I. In particular, both seem to call for a firmer institutional link between the scientific and the political bodies of the regime.

## 2.7. Concluding remarks

Summing up, we may say that institutional arrangements are considered relevant and even important factors influencing the extent to which outputs of scientific research are utilized as inputs into policy-making processes. This brief review also shows, however, that - with a few notable exceptions - little energy has so far been invested in analysing the impact of *specific* institutional variables on the science-politics relationship. Quite clearly, the general observation that institutions are important is not particularly helpful unless we can specify more precisely *how* different institutional arrangements work, and what determines the capacity of a particular institution to transform research findings into decision premisses.

The aim of this project is to take us one step further towards a better understanding of *why* and *how* institutions "matter".

### III Management or Protection of Whales: The International Convention for the Regulation of Whaling (ICRW)<sup>5</sup>

#### 3.1. Introduction: background and development of cooperation

Before looking more specifically at the role of science and institutional set-up within this regime, first a very brief sketch of the background and the broad lines of development. The first convention for the regulation of whaling was signed in 1931. The effect of this convention was limited and although some scientists had expressed concern about the effect of whaling on stocks already early in this century, "generally all sizes and species were fair game" prior to the Second World War (McHugh, 1974:322). The large-scale industrialized whaling was highly concentrated geographically and by the end of the 1930s the Antarctic seas were producing some 85% of the world catch.<sup>6</sup> The peak was reached in 1938 with nearly 55,000 animals of different whale species caught. Although some 8-10 countries undertook pelagic whaling in the 1930s, the industry was dominated completely by Norway and the UK, which between them accounted for more than 95% of the catches (Birnie, 1985, Tønneson & Johnson, 1982)

The goal of the present International Convention for the Regulation of Whaling (ICRW), adopted in 1946, is dual: "to provide for the proper conservation of whale stocks and thus make possible the orderly development of the whaling industry" (Preamble to the Convention). As Japan and Germany, having demonstrated the strongest resistance towards international regulation of whaling in the 1930s, were no longer active players in the game just after World War II, according to one observer, "the period following World War II marked a tremendous opportunity for whale conservation" (Scarff, 1977:351).

The ICRW was set up at an international conference in Washington in 1946, based mainly on a U.S. draft. It came into force in 1948 and by 1950 sixteen nations, including all major pelagic whaling nations, had ratified the Convention (Birnie, 1985). During the first stage<sup>7</sup>

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5 The results from the recently finished 1994 IWC meeting in Mexico is not included in this presentation. However, based upon the information conveyed by the media from this meeting, the upshot does not seem to change the main conclusions in this section, although a somewhat more nuanced US policy is important to observe.

6 Traditionally, whaling is a shore-based industry. The introduction of factory ships initiated the *pelagic* phase of the industry — processing was now carried out at sea.

7 The splitting up of the history of the IWC in three main phases, is based upon other studies of the IWC by the author of this section (Andresen in Wettstad & Andresen 1991 and Andresen, 1993) However, other students of the IWC have organized the description and analysis otherwise, see f.ex. Birnie, 1985.

of its history, (end 1940s to early 1960s) the International Whaling Commission (IWC), established to implement the ICRW, was for all practical purposes a "whaling club", completely dominated by the interests of the pelagic whaling nations operating in Antarctic waters, conducting what has been referred to as the "whaling olympics". Especially the larger whale species, most notably the blue whale, were severely depleted in this period and catches went dramatically down by the end of the period. In the next phase, from the mid 1960s to the end of the 1970s, the number of Antarctic whaling nations was greatly reduced not least as a result of the decimation of whale stocks. However, several measures to protect remaining stocks of whales were taken in this period. In the third phase in the history of the IWC (end of 1970s to the present), there has been a strong influx of new members opposed to whaling and commercial whaling was banned as a so-called moratorium on commercial whaling was adopted by the large majority of the IWC members in 1982 (Hoel, 1985, Andresen, 1993).

The last few years have been quite turbulent in the IWC. Previous whaling nations (most notably Iceland, Japan and Norway) have for a long time been deeply frustrated with what they see as the conservationist or protectionist bias of the IWC and have 'rebelled' against the non-whaling majority. For this reason Iceland left the IWC in 1993 and may come to start commercial whaling outside the IWC framework. Norway has not left the IWC, but started commercial whaling in 1993 despite strong objections from many IWC members as well as from the environmentalist lobby. Moreover, a North Atlantic Marine Mammals Commission (NAMMCO) has also been established (Hoel, 1993). It is still too early to know whether the IWC will become an organization primarily for *conservationist* countries while NAMMCO, where nations with previous whaling traditions are members, will become an alternative forum for international whale management.<sup>8</sup> In any case, based on the latest development, it seems that the whaling issue will continue to cause tension in the international scene in the years to come — regardless of its relatively marginal economic importance.<sup>9</sup>

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8 Norway has been able to resume commercial whaling without violating the Convention because she lodged objections to the Schedule amendments concerning the classification of the Northeast minke whales as a Protection Stock under the NMP; and to the moratorium. Thus, Norway has acted completely within its legal rights under the Convention, even if not according to the spirit of the majority of IWC members. Whether this policy will be feasible in the long run from a *political* point of view depends upon the development within the IWC. So far, however, Norway's main strategy seems to be to try to convince reluctant IWC members that commercial whaling shall be conducted within the realms of IWC on species that are not threatened. Iceland did not object to the moratorium, this may be an important reason for deciding to leave the IWC. For a presentation and evaluation of the *de facto* and potential role and history of NAMMCO, see Hoel (1993).

9 In the mid 1980s the economic value of Japanese whaling was calculated to be approximately US \$ 40 million. For more information about the economic importance of the whaling industry, see for ex. Hoel (1987). If the significance of whales is studied in a multi-species perspective, (see for ex. Beddington, Beverton and Lavigne, (1985) i.e. the level of consumption of fish by whales is included, this picture may be altered. However, as we understand it, too little precise knowledge exists about this relation to do calculations on what this may signify in economic

### 3.2. Scientific knowledge and political development: a brief overview

Have conclusions from scientific research been adopted as premisses for policy decisions in the IWC, and how has this correlation developed over time? Needless to say, considering the long and varied history of the IWC, only some main observations will be presented at this stage.

As scientists within the setting of the IWC provide *quantitative* advice on the size of the recommended TACs, it is in principle possible to measure the distance between *suggested* quota, *adopted* TACs, and the *actual catch*. This gives us a rough indicator of the extent to which scientific advice is adhered to — in contrast to many international environmental regulatory bodies which provide only more general "status-reports". In principle, this is an important distinction, presumably giving us a more precise measuring rod when quantitative advice is given. As the early history of the IWC will show, however, the difference between quantitative advice and more vague status reports may not amount to much in practice — when the scientific basis is very uncertain.<sup>10</sup>

As a part of the planning process in setting up the ICRW, at an international conference in 1944, a total catch of 16,000 Blue Whale Units (BWU) was suggested by some scientists for the Antarctic region (one BWU equals one blue whale, two fins, two and a half humpbacks or six sei whales).<sup>11</sup> This figure was hardly more than a very rough "guesstimate"; according to one of the three scientists behind this figure: "the two others were pleased that I had suggested this figure instead of 15,000 or 20,000. It looked more reliable" (Tønneson & Johnsen, 1982:157). It was up to the whalers to determine the species and stock composition of the total catch — which proved to be a very damaging management approach and the whalers switched to smaller species as the larger ones were successively depleted.

On the one hand, there were strong indications that the stocks of commercially important species like the blue whale and the humpback had been reduced, on the other hand there had been a de facto "whaling moratorium" due to World War II. Against this backdrop, the

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terms, although some attempts have been made. See for ex. Flåten (1988). As will be shown later, however, economics are not the important issue but rather competing values and principles.

10 As the knowledge base improves over time within the IWC, at least in certain periods, it makes sense to compare advice and quotas adopted.

11 The BWU was originally worked out to limit whale oil production in the 1930s; but now it became the standard regulating measure. As we shall see, when it was first adopted, it was not easy to abolish — although it did not take very long to find out its detrimental consequences for important whale stocks, see for ex. Tønneson & Johnson, 1982.

suggested quota of 16,000 BWU seemed rather modest compared to the average catches in the two years prior to World War II (29,876 and 24,830 BWU) (McHugh, 1974:322).<sup>12</sup>

This figure was adopted as a total quota by the IWC from 1946-1953. Moreover, for roughly the next 15 years in the history of the IWC (phase 1), quotas were in the range of 14,500-16,000 BWU. Beginning early in the 1950s however, members of the Scientific Committee (SciCom) that had been established, expressed serious concern that quotas were too high, but they had difficulty in quantifying the necessary reductions and not all the scientists were convinced that any reductions were necessary at all.<sup>13</sup> So behind the seemingly precise advice there was much uncertainty as well as disagreement among the scientists and the IWC chose not to pay attention to those scientists warning that catches were too high (Schweder, 1993).

In the first half of the 1960s, the IWC received more specific and detailed scientific advice on the necessity of quota reductions. A total ban on the catch of blue whales was recommended and it was suggested that the blue whale unit approach was abandoned and that individual quotas should be set by species. There was often a considerable time lag between scientific advice and adopted regulations and some of the advice was not adhered to. Still, the IWC gradually reduced quotas as suggested by the scientists. The catch of blue whales was banned and in 1967 "21 years after it had been established, the IWC had agreed on a quota that was below scientific estimates of the sustainable yield of the stock" (Scharff, 1977:366). By the end of the 1960s the quota was down to 2,700 BWU; that is about 10% of pre-World War II catches and less than 20 percent of catches during the first phase in the history of the IWC.

Quotas were set for areas and species that had previously been unregulated and by 1976 all whale stocks had their own quotas. Finally it was also agreed to abolish the blue whale unit and to set quotas by species. By the mid 1970s, the IWC adopted the so-called New Management Procedure (NMP). This consisted of a set of rules to be applied by the Commission on the basis of advice from the SciCom. The basic idea was that all whale stocks should be stabilized slightly above the level of maximum sustainable yield (MSY). According

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12 According to Schweder (1993) this level was chosen as 2/3 of the average catch 1933-1939, and was believed to be sufficiently low to give adequate protection to the blue whale stock. Later on, however, it has been argued that the quota should have been no more than 3000 units. (McHugh, 1974) Attempts, based on scientific advice, to reduce the BWU catch limit before World War II were resisted by the whaling companies because of their massive investment in factory ships and catcher vessels. As much of the whaling materiel was sunk during the War, it was easier in the post-War resumption to reduce the catch limit while still maintaining the BWU per operation at about the previous level.

13 For those interested in a very thorough presentation of the role of science as well as of the individual scientists within the IWC framework in the 1950s, Schweder (1993) offers an interesting contribution. Needless to say, the 'story' as presented here is very brief and very general.



to one observer, the NMP "marked the strongest and most specific commitment to conservation that the IWC had ever undertaken" (Scharff, 1977:370). The *practical* significance of this approach should not be exaggerated as uncertainties and deficiencies abounded (Hoel, 1985). Still, there is hardly any doubt that the *intention* of this approach was to further enhance the significance of scientific advice in the IWC decision-making system. Summing up, despite some disagreement in the SciCom, weaknesses in the NMP as well as considerable time-lags between advice and adopted regulations, there is no doubt that scientific advice was indeed adopted as decision premisses to a much higher extent than used to be the case in the first phase of the IWC history (Andresen, 1989).

For a layman, it seems that some thirty years after the IWC was established, scientists should have a fairly good idea as to the size of the commercially most important whale stocks. As the "scientific history" of the last phase of the IWC has shown, however, this has by no means been the case. Although the scientific effort was considerably increased, by the end of the 1970s it became increasingly difficult for the SciCom to agree on recommendations. Although we are obviously not in a position to judge the degree of scientific uncertainties and controversies, there is hardly any doubt that the high degree of *polarization* and *politicization* on the issue of whaling outside and inside the IWC constitutes a main reason for the problems of reaching scientific consensus on crucial issues (Andresen, 1989 and 1993).

It all started out with an understandable concern among scientists as well as environmental NGOs in the mid 1960s. Frustrated by the lack of progress within the IWC some scientists stated that: "...there is no justification for increasing the serious risk of extinction of the main stock of the largest living animal" (Røssum, 1965:161). Strong public concern and emotions over the whaling issue continued to grow and during the UN Conference on the Human Environment in Stockholm in 1972, a 10-year moratorium on commercial whaling was called for. The whale was about to become a *symbol* for the international environmental movement: "saving the whales is for millions of people a crucial test of their political ability to halt environmental destruction" (Holt, 1985:12).

As we have seen, the IWC was not left unaffected by the changed perception and values concerning the whales. The crucial question in this connection is what the role of science has been in this process? The SciCom rejected the first call for a blanket moratorium in 1972 and continued to oppose it throughout the 1970s when different proposals to the same effect were made. The SciCom maintained that there was no scientific justification for a blanket moratorium, the management approach should be more refined and selective (Birnie, 1985). Neither had the SciCom suggested a moratorium on commercial whaling when it was adopted in 1982. However, as a number of scientists in the SciCom supported the moratorium, the SciCom was not able to agree on the scientific justification of a moratorium. In fact, the disagreement was quite fundamental as the SciCom was unable to agree if the effect of a moratorium would increase or decrease the flow of information, whether whale biology would

be hastened or retarded. Under such circumstances it is almost impossible to assess the scientific impact upon IWC decisions; those in favour of the moratorium — for scientific and/or political reasons — will maintain that it is high, while those on the opposite pole will tend to claim that the Commission neglected the "best" scientific advice.

Elsewhere we have maintained that science has not been a major force in the adoption of the moratorium nor in the subsequent development within the IWC; politics has been the name of the game (Andresen, 1989 and 1993). However, considering the political temperature of the whaling debate, such evaluations are bound to be controversial.

What about the role of science in the most recent stage of IWC history? As a means of increasing knowledge and improving management procedures, the Commission decided in 1982 (at the same time as the moratorium was adopted) to carry out a Comprehensive Assessment of potentially exploitable whale stocks by 1990 at the latest. As part of the Comprehensive Assessment, research started to revise and improve the management procedure of 1976 (RMP). A Revised Management Procedure (RMP) was adopted in 1991 by the SciCom, and the Commission conditionally adopted the procedure that same year. Although the Commission accepted the RMP as a matter of *principle*, the majority of IWC nations were not willing to implement this procedure at the 1992 IWC meeting in Scotland, adopting a resolution that: "accepted the scientific engine that will calculate catch quotas, but added numerous features that need to be in place before the engine is used.." (Cherfas, 1992:11). This resolution was carried with a majority of 16 to 1 (Norway) with 11 abstentions.

At the 45 IWC meeting in Kyoto in 1993, the specifications for the RMP were now completed by the SciCom and were unanimously recommended by the SciCom to the Commission for adoption and endorsement, but they were not accepted by the IWC,<sup>14</sup> despite the fact that, according to the (former) Chairman of the SciCom, "The Commission could now put in place a mechanism for the safe management of commercial whaling.." (High North, 4 June 1993). In his letter of resignation to the IWC Secretariat (26 May 1993), he states that "the future of this unique piece of work, for which the Commission has been waiting for many years, was left in the air" (ibid.). After this he decided he could not continue as the chairman of the SciCom. However, all observers did not seem to be impressed with the report from the Scientific Committee. According to Cassandra Phillips of World Wide Fund for Nature: "It's a horribly obscure document, very difficult to interpret" (Hadfield, 1993:5).

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14 In Glasgow in 1992 the IWC accepted the so-called annex H, giving the technical specification for the RMP, although it was not quite finished. Thus an incomplete version was accepted in 1992, but not the complete version in 1993. This is stated by the (former) chairman of the Scientific Committee, Dr. Hammond, to the IWC 26 May 1993). See INRW Digest, September 1993 and High North News, (4 June 1993).

The core of the matter seems to be what role the majority of IWC nations want science to play in the future management policy of the IWC. How many countries are against whaling for moral or other reasons, *regardless of* what the scientists have to say about stock abundance? Some nations, like the US, the UK and New Zealand appear to take this position. For example, at the 1993 meeting in Kyoto, the US delegation stated that it would not support a resumption of commercial whaling, even if the requisite assessments and management procedures were in place. If such views are representative of the IWC majority, science has no 'real' role to play within the IWC.

To sum up: In the first period of the history of the IWC the impact of science was dubious. Science had a strong impact in the sense that the quotas suggested by scientists were generally followed by the IWC. However, the basis for the recommendations was highly uncertain and the quotas suggested were so high that it was very much in line with the interests of the whaling nations. Moreover, the IWC paid little or no attention to the concern from the majority of scientists that quotas and catch were too high. In the next period, scientific advice was more precise and implied far greater restrictions on whaling. Yet, gradually the recommendations were adopted as decision premisses. In the more recent history of the IWC, it is more difficult to answer this question due to the high politicization of the whaling issue and the blurring of lines between science and politics. However, we tend to conclude that in the 1980s, science generally had little impact on policy decisions. In the 1990s scientific advice has become both more precise and more consensual. Nevertheless, advice has been disregarded by the IWC. It appears that the "preservationist" approach adopted by the IWC will leave science at the margins, even as the scientists themselves improve their methods and estimates.

What role, if any, has institutional design had for the varying relations between science and politics within the development of the IWC? First a few words are needed to describe the general institutional set-up.

### **3.3. Institutional design: main features and evolution**

#### **3.3.1. Main institutional features**

The basic structure of the whaling regime is spelled out in art.III of the Convention: "The contracting governments agree to establish an International Whaling Commission..to be composed of *one member* from each Contracting Government. Each member shall have *one vote* and may be accompanied by one or more *experts and advisors*" (author's italics). As to decision-making procedures, decisions shall be taken by a simple majority of those voting. However, a three-fourths majority is needed to amend the Schedule (Art.V). The Schedule (Art.I and V) forms an integral part of the Convention. It is an instrument to secure its

flexibility in that it allows for the amending of the more detailed whaling regulations written down in the Convention. In addition, members have the right of objection to any amendment (Art. V.3) meaning that the amendment does not apply to the objecting party. It is also important to note that *any* state may become a member of the IWC, irrespective of its interest in whaling. Also, parties to the IWC may withdraw from the organization, provided certain procedures are followed. As we shall see later, several members have availed themselves of this opportunity.

As regards the role of science within the IWC, there is no reference to a Scientific Committee or to other scientific bodies in the Convention. Still, it is recognized that science has an important role to play. For example, amendments of the Schedule "shall be based on scientific findings" (Art.V.2.(b). Elsewhere (particularly Arts. V and VI) the need for studies and analyses of whales and whaling is underlined.

According to Art. III.4., the Commission may set up "such committees as it considers desirable.." and at the first IWC meeting in 1949, a (joint) standing Scientific and Technical Committee was set up. In addition a Subcommittee of Scientists was set up in 1949 and this group had separate meetings in the following years. In 1951 the "Rules of Procedure.." were changed and the three following standing committees were established: the Scientific Committee, a Technical Committee and a Committee of Finance and Administration. While the Finance Committee was restricted to five representatives, the member nations may have any number of representatives they want in the two other bodies, but each nation has only one vote.<sup>15</sup> From 1955 on a new standing Scientific Sub-Committee (SS-C) was appointed. Although there has been a vast expansion of the IWC agenda and in the number of working groups, sub-committees and special meetings on scientific issues etc, this is still the *basic* structure and organization of the IWC, with *one* Scientific Committee (no longer the SS-C) at the very heart of its activities. However, within this main structure a number of important institutional changes have taken place through continuous revisions of the Schedule and the "Rules of Procedures..."

### ***3.3.2. Development over time: gradual strengthening of the institutional capacity***

As the history of the IWC is so long compared to many of the relatively "new" environmental regimes and the fact that rather far-reaching institutional changes have been adopted —

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15 As to the Technical Committee, at least originally, it was intended to function as a bridge between the Scientific Committee and the Commission. While the SciCom should consider scientific criteria only, the Technical Committee had a broader mandate and puts forward proposals to the Commission with a simple majority. For details of the role of this Committee, see Birnie (1985).

although the basic structure has remained much the same — this warrants a description of the development over time in order to understand its possible impact on the decisions taken.

*The first phase: weak institutional basis*

Schweder (1993) has given a detailed account of the working of the scientific apparatus in the 1950s and to some extent also of its more recent developments, while Birnie (1985) has given an account of all IWC meetings from 1949-1983. We lean heavily on these two sources in the following.<sup>16</sup>

Each member nation had the right to one member on the SciCom, and he may also bring with him advisors. However, according to Schweder (1993): "The Composition of the SciCom was decided by the Commission..(and) national members were named by the Commission. However, the scientists were nominated by the respective governments, which for all practical purposes had the decisive say. The number of participating scientists was very limited in this initial phase. Although 14 countries were members of the Commission, at the 1949 meeting only 4 scientists are mentioned in the records, from Canada, Norway, the UK and the US (Schweder, 1993). In the Scientific Sub-Committee, where most of the basic work was being done, the mean attendance per meeting (1953-1959) was 7 scientists (ibid.). However, gradually participation increased and in 1959 altogether 11 nations were represented in either SciCom or the Scientific Sub Committee. According to Schweder, most of these were well qualified as scientists, although not formally required to be so according to Birnie (1985). Entry to the IWC SciCom was limited to scientists from member countries, no outside expertise could be invited and contact with other relevant scientific organizations was also limited. Thus, it took time to build up the scientific apparatus, and initially "it functioned somewhat irregularly" (Allen, 1980:27).

The Scientific Committee was explicitly subordinated to the Commission and: "It should not hold any meetings until after the first plenary session at which appropriate matters will be referred to it" (Schweder, 1993:13). However, this strict subordination of the scientific work was modified somewhat by the fact that the main scientific work was carried out in the Scientific Sub Committee *prior* to the SC meeting. Scientific work was undertaken and funded exclusively on a national basis. The Committee had no funds of its own for such purposes. In fact, the Commission itself was not empowered to conduct research. Art VI allows it only

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16 As to the more recent development, the author of this section has participated in parts of the IWC meetings in 1989 and 1990. Interviews have also been conducted with some participants to the last meetings; for ex. professor Tore Schweder, member of the Scientific Committee and Alf Håkon Hoel, member of the Norwegian delegation as well as with the Secretary of the IWC, Dr Ray Gamble.

to recommend that its members conduct research.<sup>17</sup> It could, however, request funding from the Commission for specific purposes, but this was rarely done. The only exception for many years to come was the whale marking scheme. Some UK £500 was spent on a yearly basis during the last half of the 1950s, but not without the "grumbling" of some member countries — as this meant higher fees on the part of the member countries. (Birnie, 1985) Publication procedures, or rather lack thereof also underlined that the IWC was still a very 'young' organization. In connection with the IWC meeting in 1952 it is stated that: "Its papers and proceedings remain private, apart from the publication of a brief annual report" (Birnie, 1985:213) and the report of the Scientific Sub Committee was not published until the 1954 meeting. The scientific papers presented to the SciCom were only partly included in the records of the IWC and the records of the SciCom were very brief. Moreover, there was no independent Secretariat associated with the IWC. In an overall assessment, the scientific institutional structure must be described as both 'immature' and weak in this initial period.

### *Bringing in independent scientists*

An important turning point in the institutional development of the IWC is the introduction of *external* and *independent* scientists to the work of the IWC. By the end of the 1950s, the IWC was on the verge of a break-down politically as key members were threatening to leave the organization and little progress was being made scientifically.<sup>18</sup> In an effort to break the impasse, an initiative was taken to have the Commission appoint outside scientists. Upon a formal suggestion of the United Kingdom, a group of three (later extended to four) scientists was appointed to undertake an assessment of the stocks. They had to be "qualified in population dynamics and drawn from countries not engaged in pelagic whaling in the Antarctic" (Gulland, 1988:42). Although a number of objections were made on different grounds by several members, the Committee of Three was appointed in 1960.<sup>19</sup> Still, the reluctance on the part of some members was demonstrated on a number of occasions during the years of its operation (1960-63) through their unwillingness to finance the necessary research activities proposed by the Group of Three. This delayed their work and it was less extensive than they had planned (Birnie, 1985). Still, this group of scientists, in close

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17 As we shall see later, this provision has not been regarded as a hindrance against the establishment of a small Research Fund in its more recent history.

18 In 1959 Japan, Norway and the Netherlands gave notice of their withdrawal from the Convention and Norway and the Netherlands ended up withdrawing. They withdrew due to disagreement over national quotas as well as total quota. To bring the Netherlands and Norway back into the Commission - and they re-entered shortly after - it was decided to remove the total quota so that in the 1960/1 and 1962/62 seasons, there were no restrictions on whaling.

19 Of particular interest is the initial objection raised by the U.S., maintaining that: "...the best way to conserve a living resource was through the operation of a Convention in which only nations directly concerned with the resources participated" (Birnie, 1985:257). Considering the more recent history of the IWC, this organization would obviously have been quite a different one had this principle been adopted. Based on the U.S. position in the IWC over the last two decades, she obviously no longer adheres to this principle.

collaboration with the SciCom, gave more detailed and precise advice than had previously been the case.

There was general agreement in the IWC that after three years this ad hoc committee should be abandoned. However, it was agreed that the IWC was still in need of outside scientific expertise in order to fulfil its functions. Alternative institutional models were discussed, and it was decided to use the FAO in such a capacity and this was done for a few years. The SciCom also considered the use of alternative outside expertise and in 1965 it suggested that also the bio-economics of the resources be studied "by an impartial specialist committee of economists, perhaps assisted by FAO" (Birnie, 1985:339). However, this line of thought was not followed up at the time and to our knowledge has never been pursued since.<sup>20</sup>

#### *Expanded agenda and increased administrative capacity*

By the end of the 1960s, the formal link to FAO was broken,<sup>21</sup> but gradually the SciCom received increased input from a number of special meetings and working groups reporting on specific areas and species. Not least as a result of outside pressure, the scientific agenda expanded continually with new questions raised, such as scientific permits for whaling and the question of including small cetaceans under IWC regulations. With every new issue, new working groups and new scientific sub-groups were established.

In light of the strongly increased activity, especially on the scientific side, the question of strengthening the administrative capacity was brought up in 1973. Until this time, the IWC had continued to operate from the UK Ministry of Agriculture and Fisheries, using part-time staff only. Now it was agreed that there was a need to strengthen the Commission by appointment of its own full time staff, including as its Secretary, appointment of a scientist and the provision of its own premisses. When new suggestions were tabled with the purpose of strengthening the IWC, many members were concerned about increasing costs, although the member contribution was still only £500 per year (Birnie, 1985:439-49). Thus, it took another two years before the Secretariat was in place, based on a modification of the previous flat contribution on part of the IWC members.<sup>22</sup> According to Birnie (1985:465): "backed by the

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20 This idea has been suggested more recently concerning the International Council of Exploration of the Sea (ICES), but has so far not been accepted. One reason may be that ICES is very much dominated by biologists; they may not willingly accept that other professions enter "their" domain. (Fløistad, 1990)

21 FAO had been represented in the IWC already in the 1950s. It stopped its direct involvement in whale stock assessment in 1970 because it took the view that sufficient expertise was now available within the SciCom. FAO's limited resources could then be reallocated to less well-resourced fisheries problems.

22 The new procedures called for a flat rate contribution of all member states of 50%, with additional contributions of 25% each based on areas in which each state had undertaken whaling in the previous 20 years and on catch the previous year. This meant that only 10 out of the 15

services of the improved Secretariat, both the meetings and the reports on them became longer and more detailed as the IWC expanded its role and tasks". Moreover, this strengthening of institutional capacity was particularly important to accommodate the additional scientific work done nationally in connection with the adoption of the New Management Procedure (NMP) described earlier.

Although the IWC's institutional basis had been strengthened, pressures were mounting in the 1970s as a result of the development *outside* the IWC for a *new* Convention stressing the need of an ecosystems approach, broader participation as well as a removal of the objection procedure. The ICRW was not regarded as a suitable instrument to protect the whales, as an increasing number of countries and environmental NGOs wanted to do.<sup>23</sup>

#### *New procedures; new organization?*

Although this proved not to be a successful strategy, important procedures were changed, probably at least in part as a result of this criticism. A main criticism was that most research was still done by some of the whaling nations, not always trusted to be impartial and not always supplying the information that was called for. Thus, there was increasing concern about the lack of international scrutiny and participation in the IWC's work by other independent scientists as well as independent scientific bodies. Research conducted by the IWC was still very modest; the whale marking program being the only concrete project, now increased to a modest UK £1000 a year. The many special meetings arranged were usually sponsored by national sources.

To strengthen the independent scientific input, in 1974 the US proposed that FAO observers and UNEP observers should be allowed to participate in the Commission's discussions. This was accepted, upon the discretion of the Chairman, and in 1977 for the first time scientifically qualified observers were permitted to attend the SciCom. International non-governmental organizations (NGOs) and media were also admitted to the opening and closing Plenary Session of the Commission. As to the observer status of international organizations, no criteria were laid down concerning the subject interests of the organization, apart from the fact that the organization had to have offices in more than three nations. This rule was taken over from the Union of International Associations (UNESCO) as the definition of an international organization. The intention was to restrict NGO observer status to international rather than

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members paid for this organizational reform.

23 We will not deal with the process of trying to revise the Convention as it was a very long process and taking place at least partly outside the formal IWC meetings. Moreover the process did not result in a new Convention. As is known from the more recent history of the IWC, the proponents behind this strategy, succeeded in accomplishing their main goal, cessation of commercial whaling, with basis in the existing Convention. For a detailed account of the discussions regarding a revision in the 1970s and early 1980s, see Birnie (1985) Vol.1, pp.549-572.



purely national organizations. NGOs were quick to make use of the new and liberal procedures for participation and within a few years, more than 50 NGOs participated as observers.<sup>24</sup>

What about the criteria for participation in the SciCom? FAO and UNEP were given a special status as *intergovernmental* organizations as they were explicitly mentioned in the Revised Rules of Procedure. They participated "as scientists" with the status of advisors to the SciCom. Subsequently similar status has been given to CCAMLR, IATCC, CMS and similar intergovernmental organizations. Other *international* organizations (i.e. NGOs) may send observers, with sufficient scientific expertise to understand what is going on.<sup>25</sup>

The problem of data and information became apparent in connection with the establishment of the NMP. This more ambitious management procedure with the more sophisticated categorization needed more detailed data in order to work. The main sources of information necessary to implement this procedure had to come from the whaling nations. However, according to Birnie (1985: 472-73): "Member governments' obligations under the ICRW to supply the necessary information are, however, weak and confused". In short, member governments themselves decide both what information is available and whether it is practicable to pass it on. Of course, the members were welcome to interpret their treaty obligations broadly and supply the maximum information, but according to Birnie, this was not always the case. Thus, at the 31st IWC meeting in 1979 a resolution was proposed requiring members to submit data, and the Commission should decide whether they were adequate. The resolution, however, was defeated and Birnie (1985:507) concluded after this meeting that the failure of members to do this persisted, undermining the effectiveness of the NMP.<sup>26</sup> The question of what is a *necessary* and legitimate need of information vs. what is

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- 24 As late as 1965 only five international organizations participated, all with an obvious interest in marine mammals management. In 1978 the number had increased to 24. At the last IWC meeting in Kyoto, Japan 1993, more than 80 NGOs participated. However, it is believed that some major environmental NGOs control quite a few of these.
- 25 Other international organizations sending an observer to the IWC might also nominate a "scientifically qualified observer" to take part in the SciCom meetings. However, his or her scientific qualifications were to be determined in advance by the chairman of the SciCom. With regard to FAO and UNEP etc. however, this seemed more ambiguous as they participated *as* scientists, it did not seem required that they *were* actually scientists. Furthermore, a 'quota' on such observers in the SciCom could be imposed, but normally a minimum of five would be admitted. (Birnie, 1985:475/6) IUCN occupies a curious intermediate position, as it is not wholly intergovernmental in make-up and has some NGO characteristics. Qualified scientists not nominated by the Commission may also be invited by the Chairman of the SciCom to these meetings, presenting papers etc. The SciCom may also receive and consider unpublished documents from non-members as well as scientists who are not observers. For further details, see Birnie (1985) and 'Rules of Procedures.'
- 26 In a comment, professor Tore Schweder maintains that this conclusion probably is too simple as the main discussion to a larger extent dealt with *how* data should be interpreted rather than lack of data. Moreover, catch data were regularly reported, but there was a greater lack of effort-

an *excessive* demand for information as a (political) means to stall commercial whaling, is still a bone of contention within IWC.

Procedures concerning whaling for so-called scientific purposes also came more to the forefront in the latter half of the 1970s. This question had a bearing upon the need for more and better information as well as on the question of the competence between the IWC and its members. According to Art. VIII of the ICRW, the IWC members may grant permits to: "kill..whales for the purpose of scientific research . . . as the Contracting Government sees fit". This was seen by some as a 'loophole' that the whaling nations exploited to increase catches when quotas went down. In order to avoid this, the SciCom in 1976 recommended that it should review permits prior to issue and make reports and recommendations on the proposed permits to the IWC. The Commission agreed to change the SciCom's Procedures to this effect.<sup>27</sup> Not until the end of the 1980s, however, was this made compulsory. Still, the IWC can only give recommendations on this matter. It has no means of rejecting the issuance of such permits, but practice has shown that the opinion of the SciCom carries much weight. More recently, the Commission has been given a broader mandate to evaluate the scientific criteria in relation to the SciCom, strengthening the role of the Commission vs the SciCom (Andresen, 1989:113).

In 1970 the Secretariat was also gradually strengthened through new financial arrangements, including the establishment of a Research Fund. However, by the end of the 1970s the agenda adopted was so long and complex that it was difficult for the IWC to complete their business irrespective of the much strengthened institutional capacity (Birmie, 1985).

Although minor adjustments are continuously made, by the early 1980s the IWC's main procedural features regarding the organization between science and politics were in place. In contrast to many other international agreements, the massive increase in the level of activities within the IWC has *not* resulted in a number of new bodies established or protocols adopted. Those most eager for institutional reform found that it was possible to change the workings of the IWC through continuously amending the Schedule and "The Rules of Procedures..", contributing to the de facto making of a new IWC.

However, there was no need for any procedural changes to allow for maybe the most important change in the IWC: the massive influx of new members that started in 1979. As will be recalled, the IWC is open to any country, but the number of contracting governments had

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data.

27 Some members protested and said this was a violation of the Convention. However, this was rejected by an expert legal opinion. A proposal to make this *mandatory* in 1979 instead of *recommendatory*, however, failed to pass.

been very stable (some 15-16 members) throughout the course of its 30 year history. Within the span of 4 years (1979-1983), this increased to 42 member countries.<sup>28</sup>

There has been an even stronger increase in the number of participants to the Scientific Committee. Recall that the mean attendance of scientists in the mid 1950s were 7 and as late as 1974, there were 24 representatives of the SciCom (Scarff, 1977: 355). In the 1980s and 1990s, however, more than a 100 scientists usually participated in the deliberations of the SciCom. Although much of the more basic work takes place in the various scientific sub-committees, the general discussions take place in the plenary and all meetings are open to those accredited to meet. However, as the countries are free to send as many (or few) participants as they want, there are considerable variations as to the size of the different delegations. Based upon experience from some of the last SciCom meetings, only about half of the 37 contracting governments are represented at the SciCom at all. Of those represented, a large number of countries send one or maybe two scientists, while states like Japan and the US may send as many as 10-15, with Japan usually at the top.

Summing up, the most important procedural change has been the changing of the IWC into a much more *open* organization with more transparency as well as increased participation by outside experts as well as NGOs. Thus, in a sense IWC has become 'modernized' more in line with many new environmental regimes, e.g. the ozone-regime. Generally such a development is welcomed by most analysts of international environmental regimes. However, in the following we shall try to demonstrate that this development may also have been a contributing factor to the increased polarization in the work of the IWC. From *describing* the procedural changes, let us therefore take a closer look at how this system appears to work in *practice*. Needless to say, this is bound to be somewhat speculative as we rely on relatively few sources and opinions and interpretations may vary both among 'insiders' as well as 'outsiders' — especially considering the polarized nature of the whaling issue.

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28 As to the question of *why* all these new nations wanted to become members of the IWC and their links - or rather lack thereof - to whaling, see Hoel (1985) and Andresen (1989). Suffice it to say that a large number of them appear to have been recruited by activist - and wealthy NGOs like Greenpeace - who also paid their membership fees in order to secure the necessary majority for the moratorium. More recently, a somewhat similar strategy is said to have been applied by 'the other side'. Japan has been accused of using some of the same measures, that is 'trading' votes favourable to Japan, with extensive help in other areas. More recently the number of participating countries has been reduced significantly as quite a few of the newcomers failed to show up at the meetings as well as pay their membership fees.

### 3.3.3. 'Internal organization'

#### *Autonomy, participation and funding*

The SciCom is directly subordinated to the Commission. In this formal sense, the degree of autonomy enjoyed by the SciCom is low. According to the present "Rules of Procedure.." the members of the SciCom shall be scientists, but in practice that decision is up to the preferences of each country. The question, however, is not only whether those appointed are scientists or not, but whether they are independent of their respective governments. It is obviously *possible* for governments, so inclined, to appoint scientists they are sure will support their official views on controversial matters while others who may also be highly qualified are avoided. However, according to information we have obtained both from participating scientists as well as from the Secretariat, the SciCom enjoys a considerable *autonomy* vis-a-vis the Commission, and the large majority of the members of the SciCom are well qualified scientists as well as quite autonomous in relation to their appointee, although there are said to be some "political people", strongly contributing to the polarization and politicization of science.<sup>29</sup>

The SciCom is autonomous in the sense that it defines its own agenda and it is free to take up any question it wants, without any kind of approval from the Commission. The SciCom also elects its Chairman and Vice-Chairman independently of the Commission and it establishes -, organizes - and elects the chairmen of the sub-committees it finds necessary to accomplish its work.<sup>30</sup> Although quite autonomous from the Commission, it has been maintained that in the election of chairmen of various scientific groups as well as other important positions, there tends to be an "inner circle" of key people, most often with an Anglo-Saxon background, who tend to decide among themselves on these matters.<sup>31</sup> Conversely, the Japanese, despite their very large delegations as well as considerable research programs and scientific expertise appear to have far less influence on the conduct of the work of the Scientific Committee. This may partly be due to language difficulties. However,

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29 Some of the scientific 'activists' on the anti-whaling side have close connections to the environmental movement, such as Greenpeace and International Fund for Animal Welfare. For an elaboration of this point, see for ex. Andresen (1989), Schweder (1993) The scientists on this side, however will of course claim that there are similarly close ties between the whaling nations and 'their' scientists.

30 Some directions as to the sub-committees are given; According to the Rules of Procedure of the Scientific Committee, the SciCom shall include standing sub-committees by area, or species etc. However, this can hardly be seen as a limitation upon its freedom in this regard.

31 There has been a strong IWC link to the UK throughout its history through the location of the Secretariat and the fact that the working language is English, the code of conduct is also said to be Anglo-Saxon and the strong US position in a number of ways accentuates this tendency. However, this does not mean that the persons elected to the different positions are not qualified, but that it is easier for some scientists to get such positions than for others. Most chair-persons are elected by acclamation.

considering the polarization also within the SciCom with Japan on the 'whaling side' and the UK and the US on the other side, it *may* also be a result of a rather deliberate use of procedures for more political purposes.

Still, it should be pointed out that the scientists in the SciCom do *not* represent their countries or for that matter the organizations that have nominated them; they act in their capacity as scientists. Nevertheless, in order to make effective the decision-making process, there is a legitimate *need* for close communication between the scientists from a given country and their respective Commissioner. The line between the obvious need for mutual communication and the wish to have scientific conclusions in line with national preferences is bound to be a fine one. This potential problem may be strengthened by the fact that the scientific body is subordinated to the Commission.<sup>32</sup>

The change in procedures, paving the way for the permanent presence of scientists *not* linked to any specific nation was meant to be a remedy for this problem. It has been maintained that some of these "independent" scientists serve this function and have made very useful contributions (pers.comm). However, experience has also shown that being independent of national interests does not necessarily mean that the scientists are independent of other types of interests or neutral to controversial political issues. Considering some of these scientists' strong anti-whaling views, maybe this can be seen as a 'scientific counterweight' to the dominance of research from the whaling nations but this is obviously more of a political argument than a scientific one.

The strong increase in the number of participants to the SciCom has also created problems of its own. Its large size makes it somewhat unmanageable, a problem that was pointed out by an international group of scientists appointed by the Norwegian government in 1986 (Anderson et.al, 1987). It has been pointed out however, that in a sense this problem is alleviated by the fact that smaller 'core' groups play a quite decisive role, not surprisingly within such a large group (pers.comm.).

The communication *within* the SciCom between different groups of scientists has also posed certain problems. Traditionally, the SciCom was quite dominated by whale biologists. Gradually, however, more quantitatively oriented scientists (statisticians and experts on population dynamics) have become more dominating. Considering the increased focus on population dynamics and on statistical problems connected to the abundance estimation and stock assessment, this is not surprising. Uncertainty present in the interpretation of data and

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32 By "truly" independent we mean that scientists are appointed by for example independent scientific organizations according to qualifications only. As we shall see from this report, however, this is more of an ideal than a practical reality as regards international management of resources and the environment.

the complexity of this task require extensive statistical competence. However, this has caused certain problems of communication within the SciCom between the biologists and the more quantitatively oriented scientists (Andresen, 1989).

The increasing dominance of the statisticians has also raised the question of what emphasis should be placed on uncertainty in interpreting available data for whale management. When uncertainty is high enough with respect to stock identity and abundance, it might be argued that whaling should not occur. In many ways this seems to be consistent with the *precautionary principle*, which has been adopted as a principle in connection with certain environmental agreements.<sup>33</sup> It has been argued that the uncertainty has from time to time been overemphasized in the Scientific Committee in recent years, and that certain quantitative scientists have employed the disguised method of 'maximum uncertainty'. This is opposed to the statistical method of 'maximum certainty' used to extract as much information from the data as possible (Schweder, 1993). If this is correct, it may have served as a means of delaying the scientific as well as the political process. In 1987 outside observers noted that they: "Could not fail to detect the continuing existence of deep division and lack of mutual confidence between some of those involved in the affairs of the Scientific Committee of the IWC..(and) it would have been difficult for any scientific advisory body to have maintained a ..strictly impartial approach in such emotive circumstances."<sup>34</sup>

Maybe seen as a way to get around this polarization of science, an interesting concept was introduced in connection with the development of the revised management procedure. Five separate teams were set up and they met twice a year over a five year period to discuss their work. Some teams consisted of scientists from whaling nations, others from the group known to have close connections to the anti-whaling movement. According to Schweder (1993: 29): "The process of mutual learning and competition have turned out to be productive (and) despite the great differences in technique..the five candidate procedures improved and ended up with broadly similar performance". Thus, as a point of departure, this procedure seemed productive towards reaching increased scientific consensus.<sup>35</sup>

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33 In general terms the precautionary principle means that one should act with caution and not wait till there is conclusive scientific evidence between cause and effect before action is taken. What this means in more *practical* terms is of course bound to quite uncertain.

34 This was the group of scientists appointed by the Norwegian government in 1986. Although the scientific standing of this group may have been no less than the 'Group of Three/Four' (Schweder, 1993), the fact that it was appointed not by the IWC as the former independent group but by the whaling nation Norway, may have contributed to reduce the influence and legitimacy of this report.

35 Finally the SciCom decided to choose Justin Cook's procedure, who is known to belong to the 'anti-whaling' side. In the final discussion he and some other scientists objected to this choice, and he proposed to forward all the candidate procedures to the Commission for it to make the choice, thus contributing to weakening the emerging scientific consensus.

As regards funding of research, this is mainly provided by the member states and the bulk of the primary research is conducted by the (previous) whaling nations, most notably Japan, Iceland, Norway and in addition, the US.<sup>36</sup> However, at the SciCom meetings, researchers from countries like Australia and New Zealand also play a very active part. In addition, a fair amount of theoretical work and statistical analysis is carried out by invited participants. Thus, although independent IWC research is limited, it provides the important framework for the research, arranges the necessary procedures for peer review and publications and it seems that practically all the relevant scientific expertise internationally on the various issues related to management of whaling is assembled in the SciCom.<sup>37</sup>

#### ***3.3.4. "Communication between the Scientific Committee and the Commission"***

As the SciCom is an integral part of the IWC, the main function is to give advice on the management of whales based on available scientific data. Data are provided primarily by member states, but coordinated and discussed by the SciCom.

The communication between the SciCom and the Commission goes both ways. The SciCom gives advice, partly on its own initiative and partly as a result of questions and demands from the Commission. No particular body within the SciCom is designed especially for giving advice. Thus, in principle, the same scientists that do the research also give advice. However a particularly important mediating function is played by the lead person of the scientific

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36 What countries are previous and present whaling nations is a matter of definition. The US is still a whaling nation in the sense that it conducts aboriginal bowhead whaling (approved by the IWC majority but not by the scientists..). No commercial whaling was conducted between 1988-1992, but Norway, Japan and Iceland were conducting whaling for (so-called) scientific purposes. ) In 1993 Norway conducted both 'scientific' whaling and commercial whaling, but the total quota of minke whale was less than the Japanese quota (300 animals) taken for scientific purposes.

37 As to research done by the IWC, the Secretariat has some coordination functions of ongoing national research programs and occasionally they may hire some scientists to do specific work. Of a total staff of some 15 people, about 1/3 are professionals. The main function of the Secretary is of a procedural nature, having the main responsibility for organizing the IWC meetings as well as managing the Secretariat on a daily basis. Editing and refereeing the scientific publications is also a key function of the Secretariat. Needless to say, it is extremely important for the Secretariat to be neutral to scientific and political conflicts in the IWC. The Secretariat now has a budget of £ UK 930 000, a very strong increase over time. Still, as the administrative core budget amounts to £ UK 730 000, there is not much left for scientific activities. Parts of the Research Fund have been used in connection with the revision of the management procedure. The scientific work has, however, been carried out in research groups outside the Secretariat. A fair amount of routine computing has been carried out in the IWC Secretariat, but the extra costs incurred in connection with the many extra meetings needed in connection with the elaboration of the Revised Management Procedure, were paid by the host countries. The financial problems of the IWC have also been aggravated by lacking member contributions, especially among the newcomers.

delegations from various countries as they accompany their respective Commissioner to the Commission meeting.

The most important piece of information from the SciCom to the Commission is the Report of the Scientific Committee, produced as a result of their two week meeting prior to the meetings of the Working Groups, or Sub-committees, the Technical Committee and the Commission meeting. The recent reorganization of the IWC meeting procedures was designed to allow major agenda items to be discussed twice — most extensively in a Working Group or Sub-committee, and then in the plenary. The Technical Committee, now meeting the same week as the plenary, now only has two or three key items of the agenda for discussion before they go to plenary. The functions of the Technical Committee have varied somewhat over the years. For some time it served a quite useful function as a "screening " mechanism between the SciCom meeting and the Commission meeting, but over the last years its importance has been reduced.<sup>38</sup>

As a point of departure it seems to be a tall order to produce this key document, assembling the status quo of research as well as giving it a form suitable for advice, in such a short space of time. Although most of the basic scientific work is done in between sessions, this has often not been circulated to the wider group before the SciCom meets. Current practice is for the Chairman of the Scientific Committee, in consultation with the convenors of the various sub-committees, to send out a Workplan for the Committee after the IWC meeting and take account of the decisions of that meeting. Still, the process is bound to be a very hectic and demanding task for those key persons having the responsibility to put this together in one report.<sup>39</sup>

The Report of the Scientific Committee shall be completed and available to all Commissioners by the opening date of the annual Commission meeting. According to Birnie (1985:179): "(this) rule is not always adhered to because of the infrequent meetings of the SciCom, the growing expense of convening them and the immediate proximity of its meetings prior to that of the IWC annual meeting". The argument about the infrequent meetings of the Scicom no

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38 It has also been pointed out that 4 weeks, the time it takes from the start of the SciCom to the end of the Commission meeting was too long for those having to stay for the duration of these meetings (interview in the Secretariat).

39 The fact that this is a very hectic process again brings up the point about the Anglo-Saxon "bias"; as scientists with such background often have key positions and a mastery of procedures as well as language they may often have a decisive impact on the wording of the report. This tendency is strengthened by the fact that rapporteurs from the various bodies are often Anglo-Saxons. This may be perfectly legitimate due to their mastery of the language, but it may be a delicate balance considering the politicization of the work in the SciCom. It is possible to amend/change the report in the final meetings, but this requires a mastery of the English language which only few non-Anglo-Saxons muster..



longer holds true, but closeness in time between the Scicom and the Commission meeting may still be a very real problem. Even *if* the procedures are followed, the demands upon the recipient as well as the messenger are bound to be very high given the complexity of the matter.

The most important direct and informal channel for communication is between the scientists and the respective Commissioner and delegation from the same country. The scientists outline and explain what conclusions have been reached in the SciCom. Most Commissioners will not be experts *per se* on the whaling issue; it will usually be one of many issues they are dealing with at a rather general level in their respective Foreign (or other) Ministries. In addition, however good the scientists are at *their* scientific work, the conveying of complex scientific work to (more or less) laymen, is not usually a part of their scientific training.

Apart from this informal communication at the more personal level, there is of course also formal communication at the Commission level'. The Chairman of the SciCom presents the main results of the Scientific Report to the Commission as such. In addition, scientific presentations are given on a number of more specific points during the Commission meeting by other scientists.<sup>40</sup> This may result in further questions and demands from the Commission, wanting more and new scientific questions dealt with before the next annual Commission meeting. Thus, there is a true dialogue between the two bodies. For communication to be truly effective, we would expect that there is a need for more continuous dialogue between the scientists and the 'managers' between sessions. We have limited information on the extent to which this takes place. Recall, however, that only about half of the IWC members send scientists to the SciCom at all. Thus, quite a few of the Commissioners must be assumed to base their decisions on a rather slim scientific basis.

### 3.3.5. *How far from the 'optimal' combination?*

Assuming that the 'ideal' is to combine scientific integrity/autonomy on the one hand and involvement in the political process and responsiveness to decision-makers' questions and 'needs' on the other, how does this system 'score'? In a formal sense the autonomy of the SciCom is low as it is directly subordinated to the Commission while the score on responsiveness should be expected to be high as its aim is to 'serve' the Commission's need for management advice. Thus, we would assume that the greatest challenge for the SciCom would be to obtain a high score on the autonomy dimension. Having looked at the development of practice on these dimensions over time, in many ways a rather nuanced picture stands forth. However, a main conclusion and a fact that has 'upset' this 'ideal'

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40 Some of the scientists who also act as NGO observers at the Commission meeting often also play an important part.

balance, is the polarization and politicization both within the Scientific Committee, in the Commission and in the relation between the two.

This has not only characterized the IWC over the last 15 years, there were strong tendencies to the same effect in the 1950s as well and to some (but less significant) degree also in the 1960s. Thus, this feature has been more of a rule than an exception in the history of the IWC. To a large extent this has reduced the integrity of the SciCom and also made the process of giving impartial advice very difficult, as we have accounted for previously. In other words, too much involvement in the political process by science has been the problem rather than the 'independent ivory tower ideal'.

This being said, in the same manner as it was mostly a small minority contributing to the blurring of the lines between science and policy in the 1950s, the same can be said of the more recent history of the IWC. A number of institutional and procedural measures have been introduced, at least partly as a means of getting around the politicization of science, for example the idea of involving more independent scientists. These have had some positive effects, but it has not been possible to eliminate politicization altogether. More generally, it is doubtful whether this is possible by means of institutional design. Thus, in many ways a reasonably good balance between the need for autonomy and involvement has been attained, but the value of this balance has been discounted due to the polarization over the whaling issue politically and scientifically. Thus, although *asking* for advice, on key issues, the majority in the Commission has not always been ready either to accept it or to act upon it, however consensual and however thorough the process has been scientifically. This being the case, a high degree of responsiveness on the part of SciCom to the decision-makers' questions and needs makes little difference.

### 3.4. Institutional impact — 'real' or 'spurious'?

Initially, we gave a general overview of the history of the IWC, before zooming in on the relations between science and politics and especially the significance of institutional features within this setting. As discussed in the introductory chapter, institutional design and development can only be expected to explain parts of the weight attributed to scientific premises. Running briefly through the history of the IWC, how much of a difference did institutional design make?

First, did institutional design matter for the acceptance and weight of scientific evidence in the process of depleting the large whales in Antarctic waters? Birnie answers this question in the affirmative. According to her analysis, parts of the reason for the failure of the IWC in this initial period was of an institutional nature: "its determination to have a minimal budget

limited to small members' contributions and its limitation of scientific advice and research to *national* efforts, without the addition of international *independent* research, or research conducted by the IWC" (Birnie, 1985:203). She also maintains that "the SciCom itself, being composed only of *government* scientific advisers, was over-willing to temper its recommendations to conform to so-called "practicalities" (Birnie, 1985: 261).<sup>41</sup> Schweder (1993) has a somewhat different perspective in his description and analysis of the work of the scientists until 1960. He claims that "It is probably safe to say that from a point of view of whale biology, the SciCom mustered the main competence available at the time. However, in other fields, like statistics and population dynamics, the competence . . . was much weaker than it could have been at the time". This was partly due to the fact that links to other relevant scientific organizations, having such competence, were weak or non-existent. On the question of the *independence* or *autonomy* of the SciCom, according to Schweder, this was primarily a problem in relation to a small minority of scientists.<sup>42</sup>

Thus, Schweder does not seem to stress these assumed institutional weaknesses as strongly. Nevertheless, the *immaturity* of the organization, characterized by low administrative capacity and irregularity of formal procedures probably also weakened the institutional basis. A stronger and maybe also more independent scientific apparatus may have made a difference for the acceptance and weight attributed to scientific advice. However, we agree with Birnie (1985) that institutional 'defects' were not the *main* reason for the policies adopted, but the fact that IWC did not restrict entry into the whaling operations and the use of an overall quota system set in the indiscriminating BWUs, reflecting the philosophy of liberalism and competition inherent in the ICRW. Thus, the problem-structure of the whaling issue in this period was of a *malign* nature; the common property nature of whale is known to generate the so-called "tragedy of the commons" within a system of free competition and the IWC was not set up to reduce this competitive element, rather the contrary. Considering the strength of this industry in this period, any serious attempt to constrain it would probably have been met with fierce opposition. Some whaling nations simply did not want a stronger science based organization, which could be a way to curtail their activities. On the other hand, the relatively weak scientific input can probably also be attributed simply to the fact that it takes *time* to develop a more effective institutional structure. One should also be careful in judging the past with the values of the present; whaling was considered a blessing in a period characterized by unlimited need for food and fat.

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41 In 1956 New Zealand claimed that the SciCom took into account extra-scientific considerations such as the effects of catch reductions on the industry and the possibility that governments might not accept them, instead of presenting the facts, however unpleasant. The Chairman of the Scicom accepted that they were influenced by such 'other' factors when giving advice (Birnie, 1985:236).

42 The thrust of his argument is captured in the title of his article: " Intransigence, incompetence or political expediency? Dutch scientists in the IWC in the 1950's: injection of uncertainty".

To sum up: we do think that a stronger institutional basis might have made it more difficult to neglect scientific advice, but other forces were more decisive for the depletion of the large Antarctic whales.

Did institutional design matter for the acceptance and weight of scientific evidence in the more conservationist-oriented management of the 1960s and 70s? Again, as a point of departure, we can answer this question in the affirmative. The main reason being the institutional innovation of bringing in external and independent scientists through The Committee of Three/Four and the FAO (in the 1960s) which also brought with them new and necessary scientific qualifications. According to Birnie (1985:304): "The scientific advice given to the IWC at last began to point in the right direction with the authority of independent scientific opinion". It seems to be a general opinion among a number of different analysts of the history of the IWC that this institutional innovation, however constrained by economic difficulties, represented a turning point in the history of the IWC.<sup>43</sup>

More generally, there was also a considerable strengthening of the scientific administrative capacity through the establishment of the new Secretariat in the 1970's and the increased frequency of special (scientific) meetings etc. Budgetary constraints did serve as a limiting factor on the scientific effort throughout this period also, not least as a result of the many new demands made upon IWC due to external developments and pressure in the latter part of this period. Nevertheless, there seems to be a close link between the strengthened institutional basis and the weight attributed to science. Thus, much more often than previously, scientific advice was accepted as legitimate and consequently also acted upon.

However, by looking at the change in the nature of the problem associated with whaling, the link between institutional strength and adherence to scientific advice may well be a spurious one. As we move into the 1960s, the whaling issue is gradually becoming much more 'benign' because the number of main actors is strongly reduced and so are the economic stakes involved.<sup>44</sup> It is also a fact that quotas and catch were not reduced in accordance with

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43 In a personal comment Schweder has added an interesting 'footnote' to this general interpretation. He maintains that key members of the Scientific Committee in the 1950s and the 1960s (Ruud, Laws, Otterstad) were just as able scientists as the Group of Three, and had pointed to the same problems for a long time without being heard. The real difference was that in this case, the IWC decided that these scientists were given a role as independent 'referees', who should be paid attention to, underlining the fact that institutional design might make a difference.

44 By the end of the 1960s only two nations continue their Antarctic whaling operations, Japan and the Soviet Union. Norway, United Kingdom, the Netherlands and South Africa have all left the scene. Moreover, due to the strong depletion of major stocks and the subsequent small quotas, Antarctic whaling lost much of its economic interest (Tønnesen and Johnsen, 1982).

scientific evidence until the whaling nations were unable to catch their quotas.<sup>45</sup> These changes implied that it was much easier to follow scientific advice since the 'costs' were reduced. Nevertheless, we think the process of turning the IWC into a more conservation oriented body was smoothed by the institutional change and development within IWC.

Moving to the more recent development within the IWC; did institutional arrangements matter for the weight of the scientific evidence in connection with the adoption of the moratorium and the subsequent policy of banning commercial whaling? Again, we can quite confidently answer this question in the affirmative. However, maybe the most important procedure to this effect was the fact that the IWC is open to *all* nations. There would hardly have been any moratorium on whales, had not the ICRW been equipped with this provision, considering that a 3/4 majority was needed to have it adopted.<sup>46</sup> This general provision has no *direct* bearing upon the scientific/political complex, but *indirectly* it does as this rule in combination with procedural changes paving the way for an influx of new scientists and (NGO) observers created an entirely new basis for IWC with profound effects regarding the role of science. We agree with Birnie (1985:476) that this *did* broaden the scientific basis as well as the basis for decisions more generally: "its rather narrow scientific basis, closely linked to the research and knowledge of the major whaling nations was broadened...(and) ..this procedure greatly increases the scientific knowledge, advice and criticism now available to the SciCom from independent scientists and it is particularly welcome as the complexities of stock assessments have increased". Nevertheless, the net effect was not necessarily instrumental for enhancing the role of science in the decision-making process as it *also* seemed to increase the tendency towards stronger polarization and politicization. It seems to have been a conscious strategy by those opposed to whaling to use procedural changes to strengthen their position. In this sense IWC offers an interesting example in illustrating that changing procedures may indeed affect the policies adopted. As will be recalled from the previous description, the change in key procedures *preceded* the gradual change in policy.

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45 During the 1962/63 season less than three quarters of the quota of 15,000 BWU was caught. Although the quota was reduced to 10,000 BWU the subsequent season, less than 9,000 BWU were taken and during the latter half of the 1960s, the whaling nations were not able to fulfil their highly reduced quotas. (McHugh, 1974: 310 Table 13-2. Blue whale unit catch limits established for the Antarctic since the 1945/46 season compared with actual catches.)

46 In many international cooperative ventures it is quite self evident who and how many shall participate, like the truly regional agreements as well as the truly global ones. The IWC represents a border-line case, in the same manner as for example the Antarctic Treaty System (ATS). However, within the ATS a much more *exclusive* approach has been chosen, based on for example direct and manifest interests in the area and historical connections. Had similar provisions been adopted for the IWC, there would have been no room for the large number of newcomers in the 1980s. For an elaboration of this point, see for example Andresen & Wettestad 1992.

However, the drive for these procedural changes also reflects the development outside the IWC. The massive pressure of an active public opinion in key countries, channelled through an aggressive environmental movement, completely changed the value system in relation to whales; for the majority of IWC members, management was no longer the goal, but protection. Against this group stood some of the (previous) whaling nations still regarding the whales as exploitable resources. Thus, the issue over the whales once again had turned into a much more 'malign' issue as competing value systems stood in opposition to each other (harvesting vs protection).

### 3.5. Concluding remarks

Although the formal bodies and main rules have been unaltered during the 45 years of history of the IWC, *de facto* the IWC has become a new organization. A quite small and exclusive 'whalers club' with a rather weak and somewhat ad hoc oriented institutional apparatus has been transformed into a professional organization, with a sizable Secretariat, more than twice the number of contracting parties and in excess of a hundred scientists participating in the SciCom meeting and its large number of working groups. This development is probably partly the result of organizations' general inclination to grow over time, if so allowed. In this case, there has been both inside and outside forces pressing for growth and institutional strengthening.

The relative simplicity of the institutional 'science-policy set-up' is in itself a virtue; to assemble all relevant scientific expertise internationally beneath one scientific umbrella is in itself no small accomplishment. Thus, the advice given is an expression of the 'best' scientific advice there is. There are small chances that diverging scientific opinions with much weight will be voiced from other relevant scientific bodies — as used to be the case in its earlier history. The idea of bringing in experts other than those associated with the member countries, is also in accordance with the importance of securing the more independent element. The fact that there are both positive and negative experiences associated with this procedure, does not necessarily reduce the value of this concept. It simply reminds us that when issues are highly politicized, this will surface whatever procedures you choose.

Although almost all research is nationally funded and the SciCom is directly subordinated to the Commission, in *general* the autonomy of the SciCom is fairly high. Concerning the responsiveness of the SciCom to the demands of the Commission, again the *general* picture is fairly positive. However, the fact that the SciCom work directly precedes the Commission meeting may make the process of communication less effective than it could have been if organized differently. Also, the fact that the scientific work takes place in close conjunction with the political work, may also contribute to a blurring of the lines between science and

politics. Budgetary constraints have also hampered the working of the scientific apparatus throughout its history, although the administrative capacity has been strongly increased over time, a reflection not least of the paradox that the less whales that have been taken, the longer and more complex the agenda has become.

However, these are fairly minor issues compared to the problems caused by the politicization of the issue, seriously affecting the rather positive 'score' we have assigned above. It is questionable whether it is possible to remove such deep conflicts over values by changing institutional design. However, this question has a bearing upon the effect of institutional features upon decisions taken. In all three phases institutional design does seem to make a difference. At least to some extent it may also have some independent explanatory power on the policies adopted, although other forces are usually more decisive. To some extent the institutional development is driven by its own internal logic; it tends to grow and expand over time as a result of learning, maturing and perceived organizational needs. On the other hand, the institutional basis also seems to be a reflection of 'outside' forces in the sense that institutional changes do not seem to run counter to the preferences of the dominating power coalitions, if any. Initially, the weak institutional basis may be seen as a reflection of the dominating pelagic whaling nations. In the middle period, no such dominating coalition existed, maybe allowing for more independent institutional growth. Finally, rules and procedures are created and used as a reflection of the perceived interests of the dominant anti-whaling coalition. Thus, the institutional development of IWC cannot be studied unless the interaction with 'outside' factors is re-considered.

## IV Combating Land-based Marine Pollution in the North-East Atlantic: The Paris Convention (PARCON) and The North Sea Conferences\*

### 4.1. Introduction

It was the ocean dumping issue that gave the main impetus to the establishment of international conventions to regulate marine pollution in the North Sea and the North-East Atlantic in the early 70s. The Oslo Dumping Convention (OSCON) was signed in 1972. The OSPARCOM 10th Anniversary book notes that: "After the signing of the Oslo Convention, international opinion in environmental matters was favourably disposed towards the conclusion of agreements to establish rules for the prevention of pollution" (p.6). At the initiative of the French Government, a first diplomatic conference was held in 1972 to establish a convention on land-based marine pollution.

The Convention for the Prevention of Marine Pollution from Land-based Sources (hereafter: PARCON) was signed in Paris in 1974 and came into force in 1978. It originally covered marine pollution from land-based sources such as emissions via watercourses, directly from the coast and from offshore installations under the jurisdiction of the coastal states. It was amended in 1986 to include pollution of the sea from atmospheric sources.

The main goals of the cooperation were stated in Articles 4, 5 and 6 of the Convention text. The main provisions of these Articles were: a) the elimination of pollution by "blacklisted" substances (organohalogen compounds; mercury and mercury compounds; cadmium and cadmium compounds; persistent synthetic floating materials; persistent oil and hydrocarbons); b) strict limitation and, as appropriate, elimination of pollution by "greylisted" substances (organic compounds of phosphorus, silicon and tin; elemental phosphorus; non-persistent oils and hydrocarbons; the following elements and their compounds: arsenic, chromium, copper, lead, nickel and zinc); c) prevention and, as appropriate, elimination of pollution by radioactive substances.

To attain these objectives the Convention specified that the Contracting Parties must, in respect of substances in the black and grey lists, "jointly or individually as appropriate" implement programmes and measures taking into account the latest technical developments, fixing time limits for their completion and including "as appropriate, specific regulations or standards governing the quality of the environment, discharges into the maritime area, and the composition and use of substances and products". Moreover, Article 11 called for the



establishment of a monitoring system allowing an assessment of the effectiveness of reduction measures.

As PARCON covered the North-East Atlantic, the participating states in the annual Commission (PARCOM) meetings have been, in addition to the North Sea states, Ireland, Portugal, Spain and Iceland. Moreover, it should be noted that the EC has been a separate participating member of PARCON.

Dumping and land-based pollution matters have also been discussed at three Ministerial North Sea Conferences (NSCs): Bremen, 1984; London, 1987; and the Hague, 1990. Naturally, participation has here been limited to the North Sea states.

As the work of these two entities - i.e. PARCON and the Ministerial Conferences - has been so closely interwoven, we choose to regard the various institutions connected to these entities as a single 'case'. Given this point of departure, on the *scientific/technical* side, the case has in the most recent years contained four main (groups of) bodies, three on the "inside" and one "independent". The three "insider" bodies have been: 1) the scientific/technical working groups connected to PARCON, i.e. the Technical Working Group (TWG), with subsidiary working groups. There are presently five working groups and two ad hoc working groups under the TWG (see appendix); 2) the Joint Monitoring Group (JMG), connected to both of the conventions; and 3) the North Sea Task Force (NSTF), established in the wake of the London 1987 North Sea Conference. Membership of the NSTF includes the eight North Sea states, representatives of the Commission of European Communities, under the co-sponsorship of OSPARCOM and ICES. The "independent" fourth body within this structure, is the International Council for the Exploration of the Sea (ICES). Traditionally, the input of ICES into the work of the commissions has consisted of some limited advice on monitoring questions and a general briefing on the work of ICES at commission meetings.

On the *decision-making* side, there are two main bodies: first, the annual Commission meetings, composed of the Parties to PARCON. These meetings make legally binding decisions and "morally binding" recommendations.<sup>47</sup> Second, there are Ministerial North Sea Conferences (NSCs). These Conferences produce political statements, normatively binding upon the participants.<sup>48</sup> In addition, the organizational structure consists of, first, the 'Group of Chairmen and Vice-Chairmen (CVC), carrying out some preparatory work in between Commission meetings. In addition, there is a London secretariat serving the conventions and the NSTF. The tasks of the secretariat are quite traditional: organise and prepare meetings of

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47 Hayward, Peter: "The Oslo and Paris Commissions", Special North Sea Issue of *International Journal of Coastal and Estuarine Law (IJCEL)*, London, 1990, p.93.

48 See Mensbrughe, Yves: "Legal Status of International North Sea Declarations", *IJCEL Special Issue*, *ibid.*

the commissions and other bodies, distribute and translate documentation, issue reports from the annual meetings of the commissions. Lately, the administrative capacity has increased somewhat, from 5 - 6 in the mid-80s to 11 today.

In September 1992, the Oslo and Paris Conventions were merged into one revised convention, the Paris Convention for the Protection of the Marine Environment of the North-East Atlantic,<sup>49</sup> which has not yet entered into force. Regarding land-based issues, the main new elements are the incorporation of the precautionary principle into the Convention, and also the obligation to use the best available technology (BAT) and best environmental practices (BEP). Institutionally, the two former Commissions will merge, and the technical working groups (SACSA and TWG) will merge into a "Programmes and Measures Committee" (PRAM). The Joint Monitoring Group will be reorganized into an "Environmental Assessment and Monitoring Committee (ASMO), incorporating the work of the North Sea Task Force.<sup>50</sup> We will come back to this reorganization under section 3, when institutional design questions are discussed in more detail.

## 4.2. Scientific knowledge and political development: a rough overview

### 4.2.1. Introduction

Have policymakers accepted conclusions from scientific research as legitimate advice, and given weight to and acted upon this advice? How has this relationship changed over time? These questions form the point of departure for this section.

For the sake of simplicity, "scientific knowledge" is here delimited to "knowledge of inputs, concentrations and effects of contaminating substances in the marine environment."<sup>51</sup>

We will first briefly assess the political/regulatory development of the cooperation, before we turn to the science-politics relationship.

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49 For a discussion of this Convention, see Hey/Ijlstra/Nollkaemper: "The 1992 Paris Convention for the Protection of the Marine Environment of the North-East Atlantic: A Critical Analysis", *The International Journal of Marine and Coastal Law*, vol.8, no.1, 1993.

50 The Terms of Reference for the new ASMO and PRAM Committees are outlined in the summary record of the fifteenth joint meeting of the Oslo and Paris Commissions (1993), Annex 23 and 24.

51 This is in line with the main sections and contents of the "Quality Status of the North Sea" reports.

#### 4.2.2. Political development: a brief introduction

The development of the cooperation on North-East Atlantic marine pollution can be divided into two main phases: a "low-key" and rather passive first decade, and some more "vigorous" recent years.

##### *The first decade*

The early years of PARCOM were marked by a conflict mainly between the United Kingdom and the Continental states over regulatory philosophies and instruments. The crucial question was: should PARCOM decisions be made as "Environmental Quality Standards", focusing on the marine environment's "absorptive capacity", up to which emissions freely could take place? This was favoured by the UK. Or should decisions be made as "Uniform Emission Standards", focusing more on emissions and technology than absorptive capacity. This approach was favoured by the Continental states. A compromise solution was reached in 1978, when it was decided that both approaches should be temporarily valid.<sup>52</sup> During the period 1978-87, PARCOM adopted six binding decisions and 18 recommendations. Many of these regulations contained emission standards and water quality standards, but some were cast in the form of recommendations to phase out entirely certain substances, for instance aldrin, dieldrin and endrin.

The 1984 Bremen North Sea Conference may be characterized as a 'necessary first step', producing few specific new measures.

##### *The recent years*

The 1987 London North Sea Conference can probably be characterized as a turning point in the protection of the North Sea environment. Regarding land-based issues, the most important measures were emission reductions of nutrients and toxic substances "of the order of 50%", to be achieved by 1995, 1985 being the baseline. Moreover, of special interest in a science-politics context, the "principle of precautionary action" was endorsed. The "correct" interpretation of this concept is still being discussed,<sup>53</sup> but a core notion is that abatement measures may be undertaken even if ecological damage cannot be "conclusively" scientifically proved. At the 1990 Hague North Sea Conference, it was decided to reduce emissions of

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52 For more information on the EQO-UES controversy, see Sætevik, Sunneva: *Environmental Cooperation between the North Sea States*, London, 1988, ch.5.

53 See for instance Gray, John: "Statistics and the Precautionary Principle", *Marine Pollution Bulletin*, vol.21, 1990, pp.174-76; Nollkaemper, Andre: "The Precautionary Principle in International Environmental Law", *Marine Pollution Bulletin*, vol.22, 1991, pp.107-110; Boehmer-Christiansen, Sonja: "Precautionary Principle", *Environment*, January/February, 1993, pp.42-44.

substances 'that cause a major threat to the marine environment' by 70% or more (i.e. dioxins, mercury, cadmium and lead).

In a similar manner, and partly directly related to the NSC measures, PARCOM activity has been 'vitalized'. In the period 1987-92, eight binding decisions and 24 recommendations have been agreed on. Moreover, work on the "operationalization" of the "Best Available Technology" (BAT) approach has been initiated.<sup>54</sup> A recommendation endorsing the "precautionary principle" has been adopted. In 1990, for the first time in the history of PARCOM, a binding decision was adopted by a three-quarters majority vote.<sup>55</sup>

Hence, the outcomes and general activity of PARCOM and the North Sea Conferences have clearly been strengthened in recent years.<sup>56</sup> What role has scientific evidence played in this process?

#### 4.2.3. *The acceptance and weight given to scientific evidence*

##### *A cautionary introduction*

Due to 'structural' differences between fishery/marine mammals management and pollution management,<sup>57</sup> in studies of pollution management, it is not possible to 'measure' the degree of acceptance/adoption of scientific conclusions quantitatively. In short, compared to fisheries management, there are no quota proposals on the scientific side, and consequently there are also no quota decisions on the management side. This means that assessments of the degree of acceptance/adoption of conclusions from scientific research have to be based mainly on qualitative interpretation of scientific reports and policy decisions, supplemented by interviews with scientific and political actors.<sup>58</sup> However, very few of the current decision-makers have

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54 An Industrial Sectors Working Group has been established, in order to review BAT in 10 industrial branches with a view to proposing process-based discharge standards. See Hayward, Peter, op.cit., p.95.

55 PARCOM Decision 90/1 "on the reduction of discharges of chlorinated organic substances from the production of bleached kraft pulp and sulphite pulp". See Pallemmaerts, Marc: "The North Sea Ministerial Declarations from Bremen to the Hague: Does the Process Generate any Substance?", *International Journal of Estuarine and Coastal Law*, vol.7, no.1., pp.22-26.

56 For a more thorough discussion of the effectiveness of cooperation to combat land-based marine pollution in the North-East Atlantic, see Wettestad, Jørgen: "The 'Effectiveness' of the Paris Convention on Marine pollution from Land-based Sources", *International Environmental Affairs*, vol.12, no.4, 1992.

57 Albert Weale introduces the useful distinction between "common-pool" resources (e.g. fisheries) and "common-sink" resources (e.g. environmental goods). See Weale: *The New Politics of Pollution*, Manchester, 1992, ch.7.

58 We have touched upon these issues in several rounds of interviews with participants in the North-East Atlantic/North Sea cooperation and researchers/observers over the last 5-6 years. Especially in connection with this article, we have had useful talks with Jens H. Kofoed and Lars

participated in the cooperation for more than the last 5-6 years. Hence, our interviews first and foremost cast light over the most recent decade of the North-East Atlantic/North Sea cooperation. Thus our interpretation of the first formative years — based on scattered written sources — must be seen as a modest first step in the assessment of the science - politics relationship in this period.

The discussion will be structured in the following manner: first we will establish a very rough 'scientific baseline', i.e. describe the scientific situation at the time the cooperation started. Second, the science - politics relationship in the first decade of the cooperation will be discussed, focusing on the scientific background for the decisions on emission limits and water quality standards, and the 1984 Bremen North Sea Conference. The third and final section will discuss the most recent development, with particular focus on the 1987 London and 1990 Hague North Sea Conferences.

#### *A rough scientific baseline*

If we take a brief look at other international environmental cooperative efforts such as the acid rain and the ozone layer regimes, these regimes were established due to a great extent to alarming scientific reports. Was this the case also with regard to the North-East Atlantic marine pollution cooperation? To some extent, yes. The Oslo dumping regime, the first North-East Atlantic marine pollution regime, was established due to concern over dumping activity. Part of the background for this was reports from the ICES in the late 60s, indicating that large amounts of waste were dumped in the North Sea.<sup>59</sup> However, the background for the establishment of the Paris land-based marine pollution regime seems to have been a quite general concern for the North Sea and North-East Atlantic environment, and not to have been rooted in specific, alarming reports about the quality of the marine environment.<sup>60</sup> On the one hand, the absence of 'challenging' reports meant that the level of scientific disagreement was quite low. On the other hand however, scientific uncertainty was high. For instance, the main international marine scientific organization - the ICES - had long been preoccupied chiefly

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Otto Reiersen, the Norwegian State Pollution Control Authority, John Gray, University of Oslo, Claire Nihoul, the OSPARCOM Secretariat, and Jean-Paul Ducrottoy, the North Sea Task Force Secretariat.

59 The Oslo and Paris Commissions: The First Decade, 1984, p.1. See also Jon B. Skjærseth: *Effektivitet, problem-typer og løsningskapasitet: en studie av Oslo-samarbeidets takling av dumping i Nordsjøen og Nordøstatlanteren*, the Fridtjof Nansen Institute, 1991.

60 In Sunneva Sætevik's PARCOM study, PARCOM delegates were asked to state the motives of their country for joining the regional cooperation. Sætevik summarizes the delegates' answers in the following manner: 1) a wish to obtain more information and a better knowledge concerning the connection between emissions and environmental damage; 2) a wish to obtain more control over the emissions, especially from industry; 3) none had any particular reason for not participating in the cooperation. See Sætevik, S.: *Environmental Cooperation between the North Sea States*, London, 1988, p.33.

with fishery science, its "Advisory Committee on Marine Pollution" (ACMP) having been established in 1972. This does not mean that a scientific input into the regimes' formation processes was entirely lacking. For instance with regard to the Oslo Convention process, it has been noted that "the cooperation between the experts in international law and the experts in marine pollution was really excellent".<sup>61</sup> There are no indications that would suggest the situation was very much different regarding the Paris Convention process.

### *The first decade*

Concerning the first decade of the cooperation, let us first briefly address the background for PARCOM decisions, mostly centred around emission limits and water quality standards. On the one hand, it is first important to keep in mind that the establishment of the "black"/"grey" lists was a scientifically based process. As these lists strongly influenced PARCOM's agenda in the following years, one may say that the priorities regarding PARCOM's agenda were fundamentally scientifically based. Second, the emission limits/water quality standards decisions were based upon recommendations from the "Technical Working Group" (TWG), something which ensured a certain scientific influence. On the other hand, the work of the "Technical Working Group" (TWG) had a fundamentally dual character: provide advice on technical *and* scientific questions.<sup>62</sup> In these first years, the "scientific part" of TWG's work was hampered by a "serious" shortage of data on usage, discharges, environmental effects and existing controls on 15 substances given priority (Boehmer-Christiansen, 1984). As commented by Boehmer-Christiansen: "Since not everybody agreed to assume a priori that all blacklisted emissions were harmful, a basis for concerted action did not exist."<sup>63</sup> A report on cadmium, mercury and PCB inputs, carried out by an Ad Hoc Working Group on Input Data in 1984, concluded that "there were large gaps in the geographical coverage of the Convention area as a whole, and for PCBs the data were either missing or sparse. There was a general lack of information about the methodologies used by the Contracting Parties in measuring inputs and the analytical limits of detection varied markedly".<sup>64</sup> Moreover, the progress of the JMG's monitoring work was slow.

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61 The Oslo and Paris Commissions: The First Decade, op.cit., p.4.

62 The terms of reference of the TWG is as follows. It 1) is open for membership for those Contracting Parties wishing to send representatives; 2) advises on technical and scientific questions submitted to it by the Commission, taking into account the information and advice received under the provisions of paragraphs 3) and 4) below; 3) makes suitable arrangements to keep under review the progress of scientific and technical knowledge insofar as this may assist the work of the Commission; 4) seeks advice on specific questions from the appropriate international or technical organizations. *First Annual Report of the Activities of the Paris Commission*, Annex II.

63 Boehmer-Christiansen, S.: "Marine Pollution Control in Europe - Regional Approaches", 1972-80, *Marine Policy*, January 1984, p.49.

64 Paris Commission, Sixth Annual report, p.2.

On the whole, it is thus reasonable to assume that this shortage and poor quality of data somewhat reduced both the acceptance and weight of TWG's recommendations, at least the "scientific parts". But this does not mean that scientific uncertainty was an especially important issue in PARCOM decision-making in this period. Looking through PARCOM's Annual Reports in these years, scientific uncertainty does occasionally turn up in the discussions. But this was related first and foremost to lacking national input data, i.e. reporting deficiencies, and not to scientifically underpinned assessments and suggestions introduced by TWG/JMG. In fact, these Annual Reports clearly indicate that, most often, "laggard" states used other arguments than data inadequacies and scientific uncertainty. Such arguments included economic difficulties, legal obstacles for the implementation of measures (for instance the discussions on PCBs and PCTs), the lack of available substitutes (for instance the discussions on aldrin, endrin and dieldrin), and EC-related problems (PCBs and PCTs) — to mention some.

Regarding the 1984 North Sea Conference in Bremen: the first thing to note is that part of the background for the German initiative to hold this conference was a 1980 report published by the German Council of Experts for Environmental Affairs, indicating North Sea pollution problems. Moreover, under the leadership of Germany, a specific "Quality Status of the North Sea" report (QSR) was produced just in time for the Conference. Was the content of the QSR accepted and given weight at this conference? Seemingly, to some extent it was. For instance it is claimed in ENDS Report just after the conference: "Demands by German environmentalists in 1984 for sweeping cleanup measures . . . were rejected by ministers, not least because of a major scientific effort by the UK which showed that serious pollution is mostly confined to the major estuaries and is not a threat to the North Sea as a whole".<sup>65</sup> However, as the report was completed just in time for the actual conference, and had not been discussed by the delegations in advance, parts of the report were disputed at the conference.<sup>66</sup>

### *The recent years*

Let us now turn to the most recent phase in the cooperation, with the 1987 London North Sea Conference as an important policy turning point. Has the acceptance and weight given to scientific evidence in the first decade changed in more recent years? Lacking to a large extent the decision-makers' own perceptions, we have to rely mainly on a more "indirect" line of reasoning. In our view, some factors indicate that the acceptance of scientific evidence has increased somewhat, while other factors make it hard to say if the weight given to scientific evidence has increased accordingly.

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65 ENDS Report 141, October 1986, "Opening skirmishes on the Health of the North Sea".

66 Interview with Norwegian civil servant.

The main factor supporting an "increased acceptance" assumption is the obvious improvement in quality of the available scientific evidence. According to the PARCOM Secretariat, data reporting has generally improved.<sup>67</sup> However, the most important factor in this connection is the establishment of the "North Sea Task Force" (NSTF). The NSTF was established as part of the outcome of the 1987 North Sea Conference, as a response to the scientific uncertainty communicated throughout the 1987 Quality Status Report. The work of the NSTF has undoubtedly meant a general "boost" for North Sea monitoring, modelling and research activities (more about the NSTF in the next, "institutional" section). It is of course true that improved quality of scientific evidence does not automatically increase its acceptance and weight. However, participants at the 1984 and 1987 North Sea Conferences have indicated that the longer and more extensive 1987 report was given more weight in the negotiations than the somewhat sparser 1984 QSR.<sup>68</sup>

Can the North Sea Conference reduction measures agreed to in 1987 and 1990 be interpreted as an effect of increased weight given to scientific evidence? In our view, the answer is no. Briefly stated: our impression is that many natural scientists regard the 50% North Sea Conference measures as strongly politically motivated and only loosely "scientifically justified". As the contents and main "message" of the 1984 and 1987 Quality Status Reports do not greatly differ,<sup>69</sup> such a view is understandable. The 1987 QSR painted a quite "moderate" picture of the pollution situation, emphasizing the heterogeneity of the situation. An important concluding passage goes like this: "In general, deleterious effects, at present, can only be seen in certain regions, in the coastal margins, or near identifiable pollution sources. There is as yet no evidence of pollution away from these areas".<sup>70</sup> In light of this "heterogeneous" assessment, the "homogeneous" common percentage-cuts approach was criticized.<sup>71</sup> Such a crude approach ignored variations in toxicity between substances, and variations in degree of pollution between the various parts of the North Sea. However, more as a footnote: the subsequent adoption at the 1990 Hague North Sea Conference of a common list of 36 priority hazardous substances and a 70% reduction target formulated for a smaller group of substances (e.g. dioxins, mercury, cadmium and lead), may be said to contribute to a somewhat more nuanced approach.<sup>72</sup>

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67 Interview with C.Nihoul, Secretary of the Paris Commission, April 1992.

68 Interviews with Norwegian civil servants.

69 See Wettestad, J.: *Uncertain Science and Matching Policies: Science, Politics and the Organization of North Sea Cooperation*, R:003-1989, the Fridtjof Nansen Institute.

70 "Quality status of the North Sea", Summary Report, 1987, p.1.

71 See for instance Thaulow, H.: "Nordsjøavtalen - for lite miljøvern for pengene!", Norsk institutt for vannforskning, Oslo, 1989.

72 However, according to Marine Pollution Bulletin, the scientific justification for this selection was also questioned: "The reason for the singling out of these four substances is not clear... Criticism was voiced to the effect that certain other substances are more hazardous by far than the four



In this connection, we feel that it is necessary to briefly comment upon the added weight given to the "precautionary principle" (PP) as a decision-making tool in this period. Does this signal a weakening of the weight given to scientific evidence within the cooperation? The scientific and political implications related to the PP is of course a long and complicated discussion, and we will only briefly touch upon the issue in this connection. Let us first quickly repeat the core of the PP: abatement measures may be decided upon and implemented even if there is no "conclusive evidence" of a causal relation between inputs and effects.<sup>73</sup> At first, this may look like a weakening of the weight of scientific evidence. However, in our view, the role of scientific evidence may change, but not necessarily be "weakened". Scientific evidence will still play an important part in pinpointing areas for regulation and, not least, providing crucial background material needed for setting decision-making priorities. Even if "conclusive causal proof" is no longer needed, this does not mean that "anything goes". Given that financial resources for environmental protection and decision-making capacity are limited, choices will have to be made. Scientific evidence will still be needed to assist in making such choices. In sum, it is only a very extreme interpretation of the PP that will imply a serious weakening of the weight of scientific evidence in decision-making. There is a change of function and role more than in "weight".

#### *Summing up*

Have conclusions from scientific research been adopted as premisses for policy decisions, and has this changed over time? The answers to these questions have to be qualified in several ways. As indicated earlier, an exhaustive answer to such questions would necessitate a far more detailed study of 15 years of PARCOM and North Sea Conference decision-making than we have been able to carry out in this connection. Thus, for the sake of simplicity, we have devoted much attention to the relatively few and more "visible" North Sea conferences, in relation to the more complex and "elusive" decision-making activities of the Paris Commission. Given this caveat, our answer is, first, that conclusions from scientific research have influenced decision-making from the establishment of the "black" and "grey" lists at the very beginning of the cooperation and onwards, but seemingly seldom acted as factors decisive for the outcomes. Second, it may be assumed that the acceptance of scientific evidence has increased over time, as the quality of scientific evidence has clearly improved. Third, it is hard to say if the weight given to scientific evidence has increased markedly over time. The North Sea Conference reduction measures cannot be utilized as "proof" of such an increase in weight, as these measures are probably far more politically than scientifically motivated. The increased weight given to the precautionary principle as a decision-making tool represents more a change of function and role of scientific evidence than a change in weight.

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mentioned" (*Marine Pollution Bulletin*, vol. 21, no. 5, p. 223).

73 For a discussion of the precautionary principle and its role within the new 1992 Paris Convention, see Hey/Ijlstra/Nollkaemper, 1993, op.cit.

How is this crudely sketched development related to the evolution of the institutional setting? The next section will describe the evolution of the institutional setting, before turning to the question of institutional impact in section 4.

### 4.3. Institutional design: main features and evolution

Also with regard to institutional design, we will distinguish between two main phases, with the 1987 North Sea Conference and the related establishment of the North Sea Task Force (NSTF) as the main turning point.

#### *The first decade*

Let us begin by taking a closer look at the "Commission model" (i.e. PARCOM and related bodies). As indicated in the introduction, the two main scientific/technical bodies were the "Technical Working Group" (TWG) and the "Joint Monitoring Group" (JMG). Let us first have a closer look at the TWG. TWG's "terms of reference" were laid out in the first annual PARCOM report. These terms included open entry for all Contracting Parties "which wish to send representatives". Hence, appointment and funding of delegates were up to the Contracting Parties. Moreover, TWG's main task was described as "giving advice on technical and scientific questions remitted to it by the Commission", taking into account various scientific and technical knowledge. In practice, regarding participation, the Contracting Parties have generally sent delegates considerably closer to the administrative than the scientific pole, but type of representation and disciplinary background have varied among the countries.<sup>74</sup>

In practice, the main tasks of the TWG have been 1) to produce more general technical inputs/reports to the discussions of the commission, 2) to recommend specific courses of action, and 3) to give technical assistance in the implementation process of measures. Regarding its place in the scientific-political communication chain, the role of the TWG can be described as an "interpretive buffer", i.e. it has translated and synthesized the work of the various, partly ad hoc, (sub) working groups into usable information and specific proposals for the commission to decide upon. Its "intermediate" position indicates a kind of "medium" autonomy: it has received specific tasks to carry out by the Commission, but also come up with information and advice more or less independently. TWG's main reporting has been in the form of an annual report which has been presented and discussed at the PARCOM part of the annual OSPARCOM meeting.

Shifting our attention to the "Joint Monitoring Group" (JMG), its mandate was given in article 11 in the Paris Convention. Here, the Contracting Parties agreed to set up a permanent

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74 Interview with C.Nihoul, Secretary of PARCOM, April, 1992.

monitoring system allowing "- the earliest possible assessment of the existing level of pollution; and - the assessment of the effectiveness of measures for the reduction of marine pollution from land-based sources taken under the terms of the present Convention". This was to be done by means of "pursuing individually or jointly systematic and ad hoc monitoring programmes". In practice, the work of the JMG has been based upon national monitoring programs. Like the TWG, entry has been open, and participants to the JMG have been appointed and funded by national governments. Its main function must be characterized as "coordinatory", although participants have clearly conducted research. Hence, JMG participants seem to have been considerably closer to the "scientific pole" than in the case of the TWG. The autonomy of the JMG seems also to have been higher than in the case of the TWG. Regarding the question of communication and formal links to the decision-making body, the JMG has reported both to the TWG and the annual Joint Commission meeting.

Both groups have reported to the Secretariat and the Commission about their election to office of chairman etc., but these have been mere formalities. Concerning the question of publication of reports, all material has been published with the permission of the Commission.

The role of the International Council for the Exploration of the Sea (ICES) within this structure should also briefly be commented upon. ICES is the oldest intergovernmental organization concerned with marine and fishery sciences (established in 1902). Up until quite recently, recruitment of scientists to its working groups on marine pollution has been based mainly on scientific merit.<sup>75</sup> On the whole, ICES played a very limited role within the PARCON scientific-political complex in this period, mainly giving some assistance to JMG's monitoring work. This was due to different factors: First, higher priority was given to fishery issues than marine pollution issues within ICES itself. Second, PARCON decision-makers found ICES reports too technical and little related to their practical decision-making needs.

The first North Sea Conference, Brëmen 1984, deserves some specific comments in this connection. As indicated earlier, prior to this conference, a specific "Quality Status of the North Sea" report was produced, under the leadership of the FRG. This was partly based on reports from TWG, SACSA and ICES, and partly on national reports.

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75 For more information on ICES and its research on marine pollution, see Jørgen Wettestad: *Uncertain Science and Matching Policies: Science, Politics and the Organization of North Sea Cooperation*, R:003-1989, the Fridtjof Nansen Institute, pp.13-15. With regard to ICES recruitment criteria, more weight has recently been given to national representation. For a brief discussion of this change within ICES, see Andresen/Skjærseth/Wettestad: "International Efforts to Combat Marine Pollution: Achievements of North Sea Co-operation and Challenges Ahead", *Green Globe Yearbook 1993*, p.22.

### *The recent years*

Regarding the "Commission model", few organizational changes in the scientific-political complex have seen the light of day until quite recently. Thus, until recently, the most interesting organizational changes were found in the North Sea Conference context.

First, the preparatory process for the 1987 London NSC was organized somewhat differently than the 1984 process. The work on the QSR not only commenced earlier but was also more thorough than in 1984, and the QSR was completed well ahead of the conference itself.<sup>76</sup>

Second, an important part of the outcome of the 1987 conference was the establishment of the "North Sea Task Force" (NSTF). The NSTF has included delegates from the eight North Sea states, the Commission of the European Communities, under the co-sponsorship of OSPARCOM and ICES. Its main objective was formulated in the declaration from the London North Sea Conference: "to carry out work leading, in a reasonable time scale, to a dependable and comprehensive statement of circulation patterns, inputs, and dispersion of contaminants, ecological conditions and effects of human activities in the North Sea". It has been funded by the participating states. The NSTF has had two main functions: 1) preparation of a new Quality Status Report, (QSR) of the North Sea in 1993, and 2) the coordination of monitoring, modelling and research activities.

Some of the background material for the new QSR comes from a specific Task Force monitoring programme ("Monitoring Master Plan"), utilizing newly developed measurement techniques.<sup>77</sup> Hence, the 1993 Quality Status Report will definitely come up with some new knowledge. The second task clearly indicates the coordinating aspect of the NSTF. For instance, several NSTF workshops have been organized in the field of modelling. Regarding disciplinary background and internal autonomy, the picture is mixed.

On the one hand, a considerable portion of the delegates have been scientific/technical "senior administrators".<sup>78</sup> Our guess is that when the final advisory sections were formulated, the autonomy of the Task Force delegates was limited. On the other hand, the scientific

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76 In addition to the Scientific and Technical Working Group producing the QSR, there was also a Policy working Group. Although the PWG may have helped to clarify the positions of the various participants prior to the conference, it was our impression that very little time was spent on discussing scientific matters.

77 The work of the NSTF is further described in the article "How polluted is the North Sea, and what are we doing about it?" (interview with Jean-Paul Ducrotoy, NSTF Secretary), *ICES Information*, September 1993.

78 According to Philip Reid, "The North Sea Task Force (NSTF) is comprised of delegates (scientists and senior administrators) from the eight coastal states of the North Sea...". "The Work of the North Sea Task Force", ch.7 in the 1990 IJCEL Special North Sea Issue, op.cit., p.80.

groundwork of the Task Force has seemingly enjoyed high autonomy. The NSTF Secretary, J.P. Ducrottoy, emphasizes the close cooperation with ICES as a crucial factor in strengthening the validity and reliability of the NSTF's work. For instance, monitoring data have been assessed by ICES expert groups, and ICES has also played a major part in reviewing the texts of the new Quality Status Report.<sup>79</sup> The main formal reporting will be the presentation of the new QSR in 1993, but the Task Force has also produced preliminary working papers, and reported to the most recent joint OSPARCOM meetings.

What about the organizational changes related to the establishment of the new Paris Convention, then, for instance the establishment of the new Environmental Assessment and Monitoring (ASMO) and Programmes and Measures (PRAM) committees? Judging by the terms of reference for the two groups, the changes in the functioning of the system initiated by the establishment of the NSTF will be strengthened. The terms of reference for ASMO clearly indicates that basic features of the NSTF organizational model and way of working will be carried on in close cooperation with ICES; the publication at regular intervals of reports on the quality status of the marine environment; the development of analytical tools such as modelling for assessment procedures etc. On the reporting side, in line with the more explicit advisory North Sea Task Force, it is probable that the work of the new ASMO will emphasize advisory and communication issues more than the old JMG. Regarding the new PRAM committee, which replaces the earlier technical working groups, a more action-initiating role may be signalled in its first obligation: "in accordance with the priorities established in the Action Plan of the Commissions, prepare proposals for Decisions and Recommendations for consideration by the Commissions to prevent and eliminate pollution of the maritime area..." (Annex 24, point 2.a). In comparison, the terms of reference for the TWG only contained what is now formulated as a second obligation for PRAM, namely to "advise the Commissions on technical and scientific questions remitted to it by the Commissions". However, as the TWG in practice also carried out action-initiating functions, the new terms of reference may only be a codification of practice so far. An aspect which is definitely new, is the obligation to "review reports for assessing compliance with, and the effectiveness of, agreed Decisions and Recommendations" (Annex 24, point 2.e). As this aspect will only marginally influence the science-politics interface, we do not speculate more about its possible practical implications in this connection.

#### *How far from the 'optimal' combination?*

If the "ideal combination" is to combine high scientific integrity/*autonomy* on the one hand, and *involvement* in the political process and responsiveness to decision-makers' questions and "needs" on the other, how does the quite complex system described in the preceding sections "score"? During the first decade, the TWG had a strong position within the regime, the TWG

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79 "How polluted is the North Sea...", *ibid.*, p.6.

being much of a "low autonomy/high involvement" type of body. The JMG, being much more of a "high autonomy/low involvement" type of body, played a far less significant role. Hence, on the whole, the dominating combination was one of high involvement and rather low autonomy.

With the establishment of the North Sea Task Force (NSTF) in 1987/88, the situation changed somewhat. The NSTF must be said to combine "quite high autonomy" with "quite high involvement", however operating mainly within the North Sea Conference system. As the PARCOM structure has remained unchanged until quite recently, the main impression is still that the system "scores higher" with regard to involvement in the decision-making process than regarding scientific autonomy. However, as the PARCOM system is changing, and seemingly building upon the mode of operation of the NSTF, the "integrity/autonomy" dimension may be strengthened in the coming years.

#### 4.4. Institutional impact — 'real' or 'spurious'?

##### *The first decade*

Did organizational factors influence the acceptance and weight of scientific evidence in this period? Turning first to the "Commission model", on the one hand, the PARCON organizational model seemingly worked fairly well. Participants in the cooperation emphasized monitoring, reporting and generally the scientific work carried out in TWG/ICES as positive aspects in this period.<sup>80</sup> On the other hand, it is possible to point to some weak points in the model. First, as indicated by Sætevik (1988), the marked national/administrative element in TWG recruitment spurred some complaints about 'politicisation' of the TWG. An example of this is that the European Community in 1985 pressured a member country to withdraw a proposal put forward for discussion in the TWG, and instead give this the status of an information document.<sup>81</sup> Second, the work of ICES was only to a slight extent integrated into the Commission context.<sup>82</sup> Third, as indicated earlier, the work of the JMG had several flaws. It covered only the major estuaries and coastal zones of the participating states and no open sea sites. Moreover, it was based on the already existing monitoring programmes of the Parties, and very little methodological coordination/adaptation took place.<sup>83</sup> According to

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80 Sætevik, op.cit., p.109-110.

81 Sætevik, op.cit., p. 116.

82 For further discussion of the "scientific-political complex" within the PARCOM context see Wettestad, J.: *Uncertain Science and Matching Policies: Science, Politics and the Organisation of North Sea Cooperation*, R:003-1989, the Fridtjof Nansen Institute.

83 This is based on the OSPARCOM Secretariat's own assessment in the paper "Measurement Campaigns of the Oslo and Paris Commissions", produced for the first meeting of the North Sea Task Force, The Hague December 7-9, 1988.

participants in the PARCOM cooperation, JMG's "action-initiating" function (related to its "continuous assessment of pollution levels" mandate) was virtually non-existent.<sup>84</sup> However, was the malfunctioning of the JMG primarily an organizational flaw? In our view, no. The lacklustre performance of the JMG was first and foremost a reflection of the generally low priority given to PARCON matters by the cooperating states in this period.

On the whole, if the acceptance and weight of scientific evidence was "limited" in this period, some weak points in the PARCON model may have contributed to this. But as the model overall was working fairly well, it seems reasonable to check some important "background" factors for a more comprehensive picture.

Turning first to the political malignancy of the problem, it is important to remember that this first cooperative phase was necessarily a 'getting-to-know-each-other' phase, where discussions on regulatory philosophies and emission limit values probably would have focused much on economic, technical and political issues regardless of a more specific and detailed scientific/monitoring input. This is partly due to the differences in environmental affectedness, regulatory philosophy and to some extent level of development among the North-East Atlantic states. Take environmental affectedness first: briefly, due to the "anti-clockwise" ocean current circulation in the North Sea, water discharged from rivers on the East Coast of Britain spreads throughout the central North Sea, while water from the Continental rivers tends to stay in the eastern part of the sea. So while Britain "exports" much of its pollution, Continental pollution remains largely in Continental waters. This rough asymmetry was exacerbated by an earlier mentioned controversy between the UK and the Continental states over regulatory philosophies, with the UK favouring a "scientific, assimilative capacity" type of approach, while the Continental states favoured a more technological, uniform emissions standards approach. This conflict was not made easier by being fought out on the European Community arena as well as at several environmental arenas simultaneously. Add to this rough picture a difference between the quite "wealthy" North Sea states, and the poorer Spain and Portugal, and it becomes clear that there were many basic economic and political issues to be clarified in these first years.

A second factor probably contributing to a limited interest for and weight given to scientific evidence was the public saliency factor. After the environmental enthusiasm of the early 1970s, our impression is that the public saliency of ocean pollution issues sank to a lower level in the late 70s and early 80s, at least in the most of the North-East Atlantic states.

Turning to the North Sea Conference context, did organizational factors influence the acceptance and weight of scientific evidence at the Bremen 1984 North Sea Conference? To

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84 Interviews with Norwegian civil servants in the State Pollution Control Authority and Ministry of Environment.

some extent, yes. According to participants at the conference, the late completion of the QSR reduced its value in the discussions. Few of the participants had read and discussed the report beforehand, leading to some confused debates over scientific matters at the Conference itself.<sup>85</sup> However, when taking into account the various background factors discussed in the previous section (however noting that the public saliency of the pollution problems was on the rise), it is questionable whether more efficient organization would have made *very* much of a difference to the outcome of the Conference.

### *The recent years*

Have organizational features influenced the acceptance and weight of scientific evidence in this phase? As can be recalled, we assumed a slight increase in the acceptance of scientific evidence. If we stick to this "slight increase" assessment, can organizational factors account for this development? On the surface, to a large extent, it seemingly can. First, although easily neglected, the continuous work (especially regarding inputs) within the TWG, the sub-groups and the JMG contributed to the improved quality of scientific evidence in the more recent years of the cooperation. Although the work and progress of some of these groups may have been slow, it does not mean that the accumulative product of their work over time is insignificant. According to the Secretariat, the report of the TWG has always been given a lot of attention at the PARCOM meetings. But it is also acknowledged that the Commission seems quite often to have insufficient time at its disposal to discuss the JMG report adequately.<sup>86</sup>

Moreover, as indicated earlier, the establishment and functioning of the North Sea Task Force seems to have vitalized North Sea research and monitoring considerably. However, as the new Quality Status report will not be published before late 1993, the main effect of the work of the NSTF still probably lies ahead of us, for instance in connection with the Copenhagen North Sea Conference in 1995.

Now, the crucial question becomes: did other "background" factors also change, influencing organizational factors or the acceptance and weight given to scientific evidence - or both? Regarding environmental affectedness patterns, regulatory philosophies etc., the picture was somewhat, if not dramatically, changed. The Parties' understanding of each others' positions had of course improved. Moreover, the fundamental schism between Britain and the Continent over regulatory philosophies was weakened in the last part of the 1980's, with Britain

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85 Interviews with Norwegian civil servants.

86 Interviews with PARCOM secretariat staff, April 1992.



increasingly accepting the "Uniform Emissions Standards"-approach dominant within the European Community.<sup>87</sup>

In addition, and probably more important, the public saliency of pollution problems changed considerably up through the 1980's. This was both on a "general" and on a "specific" level. On a "general" level, the quite dramatic "crisis-like" development of environmental issues such as the German "Waldsterben" related to acid rain, the Chernobyl nuclear accident, and the discovery of the "hole" in the ozone-layer over Antarctica contributed to a general heightening of environmental awareness from the mid-80's on. On a more "specific" level, the late 1980's saw several North Sea "headline-grabbing" events: among other things, fish diseases and mystic seal deaths linked to pollution, and plankton/algal blooms killing fish. These events no doubt, at least temporarily, increased the demand considerably for scientific knowledge on North Sea pollution. Norwegian newspapers, which usually barely mention the North Sea every third month or so, devoted whole pages almost daily to the state of the North Sea in the "algae" spring of 1988. It is likely that this atmosphere also influenced the decision-makers and increased their interest in and openness to new scientific findings.

#### 4.5. Concluding remarks

Based on the political history of the North-East Atlantic marine pollution cooperation, we distinguished between two main phases in the development of PARCOM and the North Sea Conferences, with the 1987 London North Sea Conference as a rough "turning-point".

Although conclusions from scientific research have influenced decision-making from the initial priority-setting of the "black" and "grey" lists and onwards, in the first decade, the acceptance and weight of scientific evidence were assessed as "limited". This *may* have had something to do with the organization of the science-politics interface. The advisory "Technical Working Group" was staffed with nationally appointed scientific/technical bureaucrats. Related to this, there were some scattered "politicization" complaints. Moreover, the progress of the "Joint Monitoring Group" was slow. However, several factors indicate that these institutional "flaws" had little independent impact on the acceptance and weight given to scientific evidence. First, despite the aforementioned "flaws", participants in the cooperation were quite satisfied with the scientific and technical aspects of the cooperation. Hence, it seems much more plausible to relate the limited acceptance and probably also weight given to scientific evidence to political background factors in this period. Although there were differences in marine pollution affectedness among the states, such issues were generally not high on the states' agendas in this period. Moreover, differences in regulatory philosophy and level of development among

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87 See Sonja Boehmer-Christiansen's article "Environmental Quality Objectives versus Uniform Emission Standards" in the IJCEL Special Issue on the North Sea, op.cit.

the North-East Atlantic states had to be discussed. Our thesis is that these factors reduced the decision-makers' interest in and need for scientific evidence. The malfunctioning of the JMG and to some extent the broader science-politics communication chain in this period can also be seen in this light as a result of the generally rather low priority given to marine pollution issues by the cooperating states.

In more recent years, the data reporting and the more general quality of the scientific evidence have improved, and we assume that this has been accompanied by an increase at least in the acceptance of the available scientific evidence. It is pretty clear that a gradually improving science-politics communication chain, and institutional innovations like the North Sea Task Force, have contributed to this increase in quality and acceptance. However, the basic organizational approach has not changed dramatically, as the NSTF has also been staffed mainly with nationally appointed scientific/technical bureaucrats. As the political background factors have changed considerably, the increased acceptance and possibly also weight of scientific evidence may again be just as much, or more, related to such factors "outside" the institutional setting. First and foremost we are thinking about the considerable increase in the public saliency of marine pollution issues. This increase in saliency has been brought about both by a general "greening" of the public opinion, related to dramatic events like the Chernobyl accident and the discovery of the "ozone hole", and more specific marine pollution events, like seal deaths and algal blooms. Our thesis is that this development inspired both organizational changes (i.e. the establishment and working of the NSTF) - *and* increased acceptance and weight given to sober reports on "chrysochromulina polylepsis" and other marine phenomena.<sup>88</sup>

A concluding reflection: the recent reorganization of PARCON scientific and technical advisory bodies indicates that the closer cooperation with ICES and the more "independent" marine scientific community, established by the NSTF, will be carried on. On the one hand, this is probably a reflection of the fact that ICES has changed, giving higher priority to marine pollution issues, and to accessibility of its work. On the other hand: although the PARCON organizational approach, with a strong national/administrative flavour in the scientific/technical work, has seemingly functioned quite well so far, the closer integration of ICES may also be interpreted as an awareness of the fundamental legitimacy problems related to such an "administrative" science-politics model.

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88 "Chrysochromulina polylepsis" is the latin name of the algae that caused damage to marine life in Norwegian waters in the spring of 1988.

\* I would like to express special thanks to Jens H. Kofoed, The Norwegian State Pollution Control Authority, for providing me with background information for this article. Interpretations and assessments are of course my responsibility alone. Thanks also to Ann Skarstad, FNI, for typing assistance.

## V International Acid Politics: The UN ECE-Convention on Long-range Transboundary Air Pollution (LRTAP)\*

### 5.1. Introduction: background and development of cooperation

In 1968 the Swedish scientist Svante Oden published a paper in which he argued that precipitation over Scandinavia was becoming increasingly acidic, thus inflicting damage to fish and lakes. Moreover, it was maintained that the acidic precipitation was to a large extent caused by sulphur compounds from British and Central European industrial emissions. This development aroused broader Scandinavian concern and diplomatic activity related to acid pollution, and played a part in the adoption of "Principle 21" at the 1972 Stockholm UN Conference on the Human Environment. This principle pointed out that states have an obligation to ensure that activities carried out in one country do not cause environmental damage in other countries, or to the global commons. The specific background for formal negotiations on an air pollution convention was the East-West detente process in the mid-70s, in which the environment was identified as one potential cooperation issue. Due to the East-West dimension, the United Nations Economic Commission of Europe (UNECE) was chosen as the institutional setting for the negotiations.<sup>89</sup>

The ECE Convention on "Long-range Transboundary Air Pollutants" (LRTAP) was signed by 35 Contracting Parties (34 countries and the EC Commission) in Geneva in November 1979. Four main aspects of the 1979 Convention may be discerned: a) the recognition that airborne pollutants were a major problem; b) the declaration that the Parties would "endeavour to limit and, as far as possible, gradually reduce and prevent air pollution, including long-range transboundary air pollution" (article 2); c) the commitment of Contracting Parties "by means of exchange of information, consultation, research and monitoring, develop without undue delay policies and strategies which should serve as a means of combating the discharge of air pollutants, taking into account efforts already made at the national and international levels" (article 3); and d) the intention to use "the best available technology which is economically feasible" to meet the objectives of the Convention.<sup>90</sup> The Convention did not specify any pollutants, but stated that measures should start with sulphur dioxide (SO<sub>2</sub>).

The Convention has been in force since 1983 and has a current membership of 34 parties. Moreover, the Convention was to be overseen by an "Executive Body" (EB), which included representatives of all the Parties to the Convention as well as the EC. Furthermore, the ECE

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89 For more information on these "formative" years, see for instance Park, C.: *Acid Rain - Rhetoric and Reality*, London, 1987; and McCormick, J.: *Acid Earth*, London, 1989.

90 Nordberg, L.: "Combating air pollution", LRTAP "non-paper", March 1993.

secretariat was given a coordinating function. The institutional structure has also included several Working Groups, Task Forces and "International Cooperative Programs". We come back to this "scientific-political complex" in more detail in section 3.

The first main regulatory step in the cooperation was the *1985 Protocol on the Reduction of Sulphur Emissions*. At the third meeting of the Executive Body of the Convention in Helsinki, July 1985, 21 countries and the EC signed this legally binding protocol. The Protocol stipulated a reduction of emissions/transboundary fluxes of sulphur dioxide (SO<sub>2</sub>) by at least 30% as soon as possible, and by 1993 at the latest, with 1980 levels as a baseline. As it was immediately signed by the 21 members of the already existing "30% Club", it obtained enough ratifications to bring it into force in October 1985. However, some major emitter states failed to join the agreement, the UK being the most important one.

In the 1988 *Sofia Protocol on Nitrogen Oxides (NO<sub>x</sub>)*, the signatories pledged to freeze NO<sub>x</sub> emissions at the 1987 level from 1994 onwards and to negotiate subsequent reductions. Twenty five countries signed the protocol, including the UK and the United States. Moreover, 12 European signatories went a step further and signed an additional (and separate) joint declaration committing them to a 30% reduction of emissions by 1998.

The latest step so far is the 1991 *Geneva Protocol on Volatile Organic Compounds (VOCs)*. VOCs are a group of chemicals which are precursors of ground level ozone. The protocol calls for a reduction of 30% in VOC emissions between 1988 and 1999, either at national levels or within specific "tropospheric ozone management areas". Some countries are allowed to opt for a freeze of 1988 emissions by 1999. 22 countries and the EC have signed the protocol.<sup>91</sup>

It should be mentioned that a revised sulphur protocol is currently being negotiated, to be based on the "critical loads" approach. The aim of this approach is that emission reductions should be negotiated on the basis of the (varying) effects of air pollutants, rather than by choosing an equal percentage reduction target for all countries involved (see section 5.2).

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91 For more information on the protocols, see for instance McCormick, op.cit.; Sand, P.: "Regional Approaches to Transboundary Air Pollution", in: *Energy: Production, Consumption and Consequences*, Washington, 1990.

## 5.2. Did scientific evidence count? A rough overview

Have conclusions from scientific research been adopted as premisses for policy decisions in this field? How has this relationship changed over time? These questions form the point of departure for this section. As indicated in the section on PARCON and the North Sea Conferences, due to the lack of quantitative, quota-like scientific inputs/advice and the related lack of quota-like decisions, it is very hard to "measure" the degree of acceptance/adoption of scientific conclusions. This means that assessments of the degree of weight given to scientific evidence also in this case have to be based on qualitative interpretation of scientific reports and policy decisions, supplemented by interviews with scientific and political actors. Although we have carried out several interviews with various relevant actors in recent years, given the demanding character of this research program, our assessments must really be regarded as initial interpretative steps.<sup>92</sup>

The discussion will be structured in the following manner: first we will establish a very rough "scientific baseline", i.e. describe the scientific situation at the time the cooperation started. Second we will discuss the weight given to scientific evidence in the process leading up to the 1985 SO<sub>2</sub> protocol. Third, the weight of scientific evidence in the processing leading up to the 1980 NO<sub>x</sub> protocol and the 1991 VOC protocol will be discussed, with special weight on the NO<sub>x</sub> process. The fourth section will briefly discuss the role of science in the current protocol revision processes, based on "critical loads".

### *A rough scientific 'baseline'*

Let us first turn to the scientific situation as it was at the time the cooperation commenced. As indicated in the introductory section, in the late 1960s and early 1970s, the main scientific input on acid rain and international air pollution came from Swedish and Norwegian researchers. However, in 1972, the OECD launched the "Cooperative Technical program to Measure the Long-Range Transport of Air Pollutants". 11 European states participated in this program. Results of the program were first published in July 1977. Concerning the question of long-range transportation, the report affirmed that such transportation was taking place. The report concluded that there was "strong evidence" that the sulphur dioxide pollution caused

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92 We have conducted several interviews with Per M. Bakken, the Norwegian Ministry of Environment, Harald Dovland, the Norwegian Institute of Air Research, and Erik Lykke, the Norwegian Ministry of Environment. Moreover, we have had talks with Anton Eliassen, the Norwegian Institute of Meteorology, Jan Thompson, the Norwegian Ministry of Environment, and Lars Nordberg, LRTAP Secretariat in Geneva. In addition, we draw on information gathered through a questionnaire on the "effectiveness" of LRTAP, carried out in the Spring of 1991. The questionnaire was initially sent out to all the LRTAP countries, and to a selected group of eight more independent "observers". We received eight answers from the "Contracting Parties" group, among them important countries like the UK, the FRG, and the Scandinavian countries; and four answers from the "observer" group.

the acid rain and snow that appeared to be killing salmon and trout, for instance in Norway and Sweden. However, the degree of scientific uncertainty related to all these aspects of the problem must also be characterized as quite high. For instance, the OECD stated explicitly that owing mainly to uncertainties surrounding emission data and dispersion models, the program's findings were only accurate to within plus/minus 50 percent.<sup>93</sup>

In addition to high uncertainty, there was also some scientific controversy, especially between Scandinavian and British researchers. However, according to researchers who participated in this debate, it was not as heated as the political rhetoric at the time may lead one to believe.<sup>94</sup>

Having established a rough scientific "baseline", let us then turn to the role played by scientific evidence in the process leading up to the 1985 SO<sub>2</sub> protocol.

### *The "sulphur process"*

First, compared to the situation when the cooperation started, there is no doubt that scientific evidence with regard to the transnational transportation of sulphur and nitrogen compounds was becoming much more accepted in the early 1980's than in the mid 1970s. A certain scientific "consolidation" was witnessed at the 1982 Stockholm Conference on Acidification of the Environment. The conference's final statement concluded that "...the acidification problem is serious and, even if deposition remains stable, deterioration of soil and water will continue and may increase unless additional control measures are implemented and existing control policies are strengthened".<sup>95</sup> Moreover, Park (1987) maintains that "...by this time (i.e.1983/84)...beyond doubt was the transfrontier character of the problem, in which some countries gained whilst others lost" (p.177). He further states that when the European Parliament Committee on the Environment, Public Health and Consumer Protection held a public hearing on acid deposition in 1983, "all agreed that the adverse effects of acid rain were clear, and the debate centred *not* on whether or not to reduce emissions but on how it should be done and who should pay for it" (p.174). Thus, we have good reason to assume that the existing scientific evidence was very much accepted and given considerable weight by the states choosing to sign the 1985 SO<sub>2</sub> protocol. Among the "important" non-signers (Poland, the UK, the United States), it was only the United States that seemingly gave very much weight to scientific uncertainty in its explanation for not joining the agreement.<sup>96</sup>

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93 Wetstone/Rosencrantz: *Acid Rain in Europe and North America*, Washington, 1983, p.136.

94 Interview with Harald Dovland, the Norwegian Institute for Air Research.

95 Cited in Wetstone, G.: "A History of the Acid Rain Issue", in Brooks/Cooper: *Science for Public Policy*, Oxford, 1987, p.186.

96 McCormick, J., *op.cit.*, p.85.

However, this does not mean that the 30% target was in itself directly derived from scientific evidence. Both the base data of 1980, the target date of 1993, and the 30% figure have been characterised as arbitrary.<sup>97</sup> But our own interviews have confirmed the impression that most scientists definitely saw the 30% reduction as a significant step in the right direction.

### *The "Nox process"*

Nitrogen oxide emissions stem from both power plants and automobile emissions. They are responsible for some acid deposition, and also contribute to the formation of ground-level ozone, a gas that is toxic to humans and harmful to vegetation.<sup>98</sup> The NOx negotiation story is somewhat different from the SO2 story. On the one hand, the process was scientifically underpinned in the sense that the initiative to formal negotiations on a NOx protocol was a reflection of the growing recognition that the causes of air pollution problems were multifaceted and complex - and henceforth that the problems were far from "solved" by the establishment of the sulphur protocol. On the other hand, at least in the first phase of the negotiations, scientific understanding of the NOx problem was less developed than in the case of the SO2 negotiations. For instance, a Norwegian observer claims: "Here the politicians decided to go ahead with actual negotiations before the necessary scientific basis had been established".<sup>99</sup> Our impression is that scientific knowledge improved a good deal during the negotiations. Thus over time, the acceptance of scientific evidence seems to have become quite high.<sup>100</sup> According to Levy (1993), "the scientific consensus now is that ozone is just as harmful to forests and agricultural crops as sulphur deposition. It is also a public health problem in many urban areas. All this was accepted during the negotiations of the nitrogen protocol. There was dispute over the magnitude of the damage, though" (p.94).

It is very hard to determine whether the weight given to scientific evidence was lower or higher than in the sulphur process. The final NOx negotiations seem to have focused almost exclusively on economic and technological issues, much related to the reduction of auto emissions. Germany, who supported a protocol, had a fleet of cars which, first, were already required under 1983 legislation to instal catalytic converters. Second, the cars had a large average engine size, which could accommodate catalytic converters with lower increases in marginal costs than cars with smaller engines. The UK, France and Italy, who opposed a

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97 Haigh, N.: "New Tools for European Air Pollution Control", *International Environmental Affairs*, vol.1, Winter 1989.

98 Levy, M.: "European Acid Rain: The Power of Tote-Board Diplomacy", in Haas, Keohane, Levy (eds.): *Institutions for the Earth*, Cambridge 1993, p.94.

99 Bakken, P.: "Science and politics in the protection of the ozone layer", in Andresen/Østreg (eds): *International Resource Management*, London 1989, p.202.

100 This is first and foremost based on Morten Stenstadvold's account of the negotiation process in his thesis "The evolution of cooperation. A case study of the Nox-protocol" (in Norwegian), University of Oslo, autumn 1991.



protocol, had no catalytic converters legislation in place, and an auto industry producing cars with smaller engines.<sup>101</sup>

*Some brief notes on the "VOC process"*

Regarding the VOC protocol, as indicated earlier, it has a comparatively quite complex structure, combining a 30% flat reduction target with a clause opening up for reductions only within specific TOMAs (tropospheric ozone management areas). This complex structure may be interpreted as a response to the patchy scientific evidence at hand. On the one hand, that VOCs contribute significantly to European ozone formation is not disputed. On the other hand, VOCs are comprised of a large number of chemicals, including fuels, solvents and cleaners. These chemicals interact differently according to meteorological and topographical conditions, in ways not yet fully understood, so that their ozone-creating potential varies widely (Levy, 1993). Furthermore, specific knowledge on national emissions seems scarce. As of mid-1991, only five countries had collected systematic knowledge on their VOC emissions.<sup>102</sup>

*Protocol revision and the question of critical loads*

Negotiations on a revised sulphur protocol are currently going on. As indicated in the introductory section, this protocol is to be based on the "critical loads approach". A "critical load" is defined as "a quantitative estimate of an exposure to one or more pollutants below which significantly harmful effects on specified sensitive elements of the environment do not occur according to present knowledge".<sup>103</sup> This approach has come about through developments in the field of modelling in the latter 1980s. An accepted element of the approach is the generation of so-called "target loads" from critical loads, target loads taking into account economic factors especially, but also considering technological, social and political factors. More specifically, to use the critical loads approach, the following information is needed: a) inventories of current emissions and projections of future emission rates; b) estimates of the potential for and costs of emission reduction; c) long-range transport models; d) maps of critical loads and target loads; d) integrated assessment modelling.<sup>104</sup>

Early in the negotiations, it was recognized that it would be impracticable to reduce sulphur depositions below critical loads by the end of the century. Instead, within this time frame, it has been agreed to reduce by 60% the gap between current levels of sulphur deposition and critical loads in most of Europe, except for the most acid-sensitive areas. But even with this more moderate ambition, an estimated annual cost of 30 billion DM has been suggested. Furthermore, the approach would require for instance a 89% reduction in the UK's SO<sub>2</sub>

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101 Levy, op.cit., p.95.

102 Levy, M., op.cit., p.99.

103 Ibid., p. 24

104 Nordberg, L., op.cit.

emissions in the years 1980-2000 — much more than its current EC-related programme to reduce SO<sub>2</sub> emissions from large combustion plants by 60% by 2003. Reports from the latest negotiating sessions in August/September indicate that several countries will have difficulties in agreeing to the emission targets calculated for them under the critical loads approach, especially the UK.<sup>105</sup> According to ENDS-Report, although the protocol was due to be ready for signature at the LRTAP Executive Body meeting in November, negotiations are now likely to continue into next summer.<sup>106</sup> On this background, it is possible that the critical loads approach becomes "watered down" in the final rounds of negotiations, and the weight of scientific evidence reduced. Our impression is that the acceptance of the fundamental scientific groundwork carried out is fairly high. Still, there have been some heated disputes over which of the computer models give the "correct" reduction targets. For instance, a model developed by the Stockholm Environment Institute indicates that the UK will need to cut its SO<sub>2</sub> emissions by 88% (if the 60% "gap closure" is to be achieved). The International Institute for Applied Systems Analysis' (IIASA) RAINS model suggests a target of 79%. In comparison, the UK's own ASAM model indicates that a reduction of "only" 76% will be necessary...But a clear majority of the countries seems to have confidence in IIASA's RAINS model.<sup>107</sup>

#### *Summing up*

Have conclusions from scientific research been adopted as premisses for policy decisions, and has this changed over time? As in the case of PARCON and the North Sea Conferences, we have to make a caveat: an exhaustive answer to these questions would necessitate a far more detailed study of 14 years of LRTAP decisionmaking than we have been able to carry out in this connection. Having said this, our assessment is that the weight of scientific evidence has varied somewhat: in the sulphur negotiations our impression is that both the acceptance and weight of scientific evidence were quite high — despite the fact that the 30% target itself was not derived from scientific evidence. In the NO<sub>x</sub> and VOC negotiations, scientific evidence seemingly played a somewhat less important role. In the case of the current SO<sub>2</sub> protocol revision negotiations, based on the critical loads approach, scientific evidence plays a very important role. With regard to the majority of the countries, the general acceptance of the scientific evidence seems to be quite high. However, as several countries will have problems in meeting the targets proposed for them, the final weight of scientific evidence remains to be seen.

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105 This is somewhat paradoxical, as the UK was in the forefront with regard to the initial development of the critical loads approach.

106 This section is based on the following ENDS-Report articles: "UK likely to be hard hit by revised SO<sub>2</sub> protocol" (218, March 1993); "Government protects British Coal in talks on new SO<sub>2</sub> target" (222, July 1993), "UK leads objections to new agreement on SO<sub>2</sub>" (224, September 1993).

107 ENDS-Report 224, September 1993, "UK leads objections to new agreement on SO<sub>2</sub>".

How, then, is this crudely sketched development related to main features of the institutional setting? This is the main theme of the next sections.

### **5.3. Institutional design: main features and evolution**

#### **5.3.1. Introduction**

There are two main phases in the development of LRTAP institutional design: before and after the reorganization that took place in 1991.

Let us first turn to the initial, "formative" phase. A first thing to note is that part of the LRTAP "scientific-political complex" lingered on from pre-Convention days, namely the Working Party on Air Pollution Problems" (WPAP). WPAP was initially under the ECE body 'Senior advisers to ECE Governments on Environmental and Water Problems.' Other parts of the scientific-political complex grew out of the aforementioned OECD monitoring program, namely the EMEP ("Cooperative Program for Monitoring and Evaluation of Long-range Transmissions of Air Pollutants in Europe") program. The rest of the complex was established in connection with the convention, with sub-groups gradually being added to the structure. Up until 1991, the structure was roughly like this: on the administrative, political side, the main bodies have been the "Executive Body" (EB) of the Convention, with the parties meeting annually since 1983, and the "Working Group on Abatement Strategies" (WGAS), an important forum for continuous negotiations. On the scientific/technical side, important subsidiary bodies under the EB have been the "Working Group on Effects" (WGE); the Working Group on Nox; the "Working Group on VOCs" (WGV); the EMEP Steering Body; the "Group of Economic Experts on Air Pollution" (GEAP); and the aforementioned "Working Party on air Pollution Problems" (WPAP). Under these bodies, several "International Cooperative Programs" (ICPs) and Task Forces have been established.

This set-up was reorganized in November 1991. The current organizational structure is somewhat simpler: on the administrative/ political side, in addition to the EB, there is now a "Working Group on Strategies" (WGS). On the scientific/technical side, in addition to the "Working Group on Effects" (WGE) and the EMEP Steering Body, there is now a "Working Group on Technology" (WGT). Current International Cooperative Programs under the WGE are forests (Germany), freshwaters (Norway), materials (Sweden), crops (UK), and integrated monitoring (Sweden). An overview of the whole new structure is given in appendix 2.

The EMEP monitoring program warrants some specific, introductory comments. It has been initiated by the ECE in cooperation with the United Nations Environment Program (UNEP) and the World Meteorological Organisation (WMO) as a part of UNEP's "Global Environment Monitoring System" (GEMS). The main objective of EMEP is to provide governments with

information on deposition and concentration of pollutants, as well as on the quantity and significance of long-range transmission of pollutants. The program has three main elements: emission data, measurements of air and precipitation quality, and atmospheric dispersion models. The EMEP sampling network consists of some 100 stations in 33 countries, and the work is coordinated by three international centres, two in Oslo and one in Moscow. In 1984, a specific EMEP financing protocol was established. Funding is now provided by all Parties to the EMEP Protocol, according to a cost sharing agreement developed by the Parties to the Convention on the basis of the UN "assessment scale" (based on GNP, population and geographic criteria).<sup>108</sup> The 1989 EMEP budget was around 1 million dollars.<sup>109</sup>

Finally, a few words about the Secretariat. As indicated earlier, the ECE Secretariat in Geneva coordinates the cooperation. More specifically, the Secretariat work is presently being carried out by five full-time professional posts and two secretaries in the air pollution section of the ECE Environment and Human Settlements Division.

### 5.3.2. *Internal organization*

#### *Autonomy, participation, and funding*

As indicated in the previous section, we are here talking about formal bodies. The EB discusses and approves the mandates of the various groups. However, overall, the Secretariat claims that the groups' work is very autonomous. Other sources indicate that the degree of autonomy varies somewhat between the groups, and also between the issues discussed by the groups. The general rule seems to be that groups discussing general features of problems enjoy a high degree of autonomy, while groups discussing effects and issues with more obvious political implications enjoy less autonomy.<sup>110</sup>

Formally, entry is open to all governmentally appointed experts in all the groups — "entirely in the hands of governments", as expressed by the Secretariat. More informally, the Secretariat sometimes indicates certain desirable qualifications with regard to rank, education etc. vis a vis the participating countries. All subsidiary bodies are comprised of government officials.<sup>111</sup> There is a mix between scientists and administrators in all the working groups, including the EMEP Steering Body. Delegations vary widely with regard to size. Some countries may choose to appoint delegations with many experts, each covering one topic, whereas others rely on fewer persons. The mode of appointment also varies among the

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108 Dovland; H.: "Monitoring European Transboundary Air Pollution", *Environment*, December 1987; personal communication with L.Nordberg, LRTAP Secretariat.

109 Levy, op.cit., p.88.

110 Interviews with P. Bakken, The Norwegian Ministry of Environment.

111 Levy, op.cit., p.84.

countries. However, there is a certain pattern, with some countries consistently closer to the scientific or the administrative "pole" than others.<sup>112</sup> In the Task Forces and ICPs, participation is far closer to the scientific than the administrative pole. Membership requires that research is conducted at the national level, and that this research is harmonized and shared with other participants. However, the regularity in participation should not be exaggerated, as this varies somewhat between meetings. This is partly issue related: some people participate only in meetings where certain issues are discussed. This also gives the secretariat the possibility of influencing participation indirectly: by framing issues in certain ways, they know that certain people get tempted to show up. . . However, the secretariat can also formally invite persons/organizations, but only those with ECOSOC credentials.<sup>113</sup> Given the multitude of bodies, disciplinary background is obviously heterogeneous.

Regarding funding, this is generally in the hands of the participating governments. However, there are some nuances to this picture. EMEP is as indicated above funded according to a specific protocol and cost-sharing agreement. The coordinating expenses for the ICPs are provided by the lead countries in each of the ICPs, and participating governments pay their own research costs.<sup>114</sup>

#### *Main functions*

The groups vary with regard to main functions. The Working Group on Effects (WGE) is responsible for assessing effects of air pollution on ecosystems, crops and materials. It addresses dose-response relationships, critical levels and loads and the ecological effects of reduction of air pollution, emissions, concentrations and depositions.<sup>115</sup> On the whole, the WGE first and foremost synthesizes and "translates" existing knowledge. It supplies important background material for revised and new protocols, by supplying information relevant for "critical loads". Actual research is mainly carried out in the various sub-groups.

The EMEP Steering body has been described as "the backbone of the Convention".<sup>116</sup> Our impression is that the Steering Body first and foremost has a coordinating function. It directs scientific work for the improvement of data and information on air pollution, and advises the international EMEP centres on appropriate development of routine measures and research projects.<sup>117</sup> But it also has a "translatory" function, for instance in cases where the Working Group on Strategies needs certain data as inputs to ongoing negotiation processes. It is likely

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112 Interview with L. Nordberg, LRTAP Secretariat.

113 Ibid.

114 Levy, op.cit., p.89.

115 Annual Report, LRTAP Executive Body, 1991.

116 Interview with L. Nordberg, LRTAP Secretariat.

117 Annual Report, LRTAP Executive Body, 1991.

that the Steering Body will increasingly be called upon to provide reliable data on pollution levels and loads as a basis for calculating excesses in relation to "critical loads".

The Working Group on Technology (WGT) came into operation in June 1992. It took over responsibility for the former WPAP and the earlier Task Force on Exchange of Technology. The WGT will probably first and foremost have a coordinating role, supervising the substantive work on technologies for air pollution abatement.

The principal task of the Working Group on Strategies (WGS) is to prepare protocols on specific air pollutants, or group of air pollutants. Thus, the WGS is the main negotiating body within the LRTAP organizational structure. The body has a coordinating and "translatory" role, as it cannot fulfil its negotiating role without inputs from all the other bodies. Regarding the economic dimension, it should be noted that there is now a specific Task Force on Economic Aspects of Abatement Strategies under the WGS. Another interesting body under the WGS is the Task Force on Integrated Assessment Modelling. This group has cooperated closely with the International Institute for Applied Systems Analysis (IIASA) to evaluate the cost effectiveness of different abatement strategies. IIASA has developed a system model, the "Regional Acidification Information and Simulation" (RAINS) model that combines information on emission generation with emission control technologies and abatement costs, also taking into account the long-range transport of pollutants and the environmental effects of acid deposition in different areas of Europe. On that basis, scenarios of different abatement strategies can be analysed.<sup>118</sup>

### *5.3.3. Communication between scientific and regulatory bodies*

All bodies produce annual reports to the EB. In addition to those reports, the Secretariat provides the EB annually with a document on policies and strategies as reported by parties, a document on progress in selected areas, a draft work plan for the forthcoming year, and a note on financial issues. According to the Secretariat, the EB reviews and responds to these reports, but the extent of this response varies a good deal. The standard response formulation in the EB report is that the EB has "taken note of" the reports produced. This may be regarded as a deliberately ambiguous formulation. "Take note of" is normally interpreted as an approval, but it may also be interpreted as expressing only that the EB has seen the reports.<sup>119</sup> Conclusions and recommendations for action, expressed in the reports submitted to the EB, have to be endorsed by the EB in order to be implemented. This is normally a very smooth procedure since the relevant subsidiary body has reviewed and approved all draft

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118 Wuster, H.: "The Convention on Long-range Transboundary Air Pollution: its Achievements and its Potential", in T.Schneider (ed.): *Acidification Research, Evaluation and Policy Applications*, 1992, p.235.

119 Interview with L. Nordberg, LRTAP Secretariat.

recommendations beforehand. The main formal response element lies in review of policies and strategies, and adoption of work-plan and EMEP budget. In addition, there are the discussions on and formulation of new or revised mandates for the subsidiary bodies, and, if necessary, the Task Forces. The most far-reaching decisions by the EB concern the adoption of international agreements, i.e. protocols and their amendments.

The question regarding the "organizational distance between research and policy-making" is also in this context a complex one. However, some broad features may be indicated: first, all research is reported to the Working Groups and the EMEP Steering Body before reaching the Executive Body. Reports from the Working Groups are presented to the EB, but this is more of a formality than a thorough advisory process. Second, given the central role of the WGS vis a vis the EB, it may be assumed that the work carried out in the three Task Forces directly under the WGS, providing basic information for the negotiations, stand an especially good chance of being communicated to the EB. Besides, given the weight attributed to the possible establishment of "critical loads" in revised protocols, the Task Force on Mapping is quite closely connected to the policy process. This is also explicitly indicated in the revised organization map.

#### ***5.3.4. "How far from the 'optimal' combination?"***

If the "ideal combination" is to combine scientific integrity/autonomy on the one hand, and involvement in the political process and responsiveness to decisionmakers' questions and "needs" on the other, how does the complex system described in the preceding questions "score"? Although our limited data does not allow a very precise answer, a couple of features stand out at this stage: first, much like other environmental scientific-political complexes like for instance the PARCON system, integrity/autonomy is highest in the sub-groups (i.e. ICPs and TFs). Autonomy is lower in the working groups, although a significant range of variation exists among the groups, and even with regard to different issues within the groups. Second, the prominent place of the EMEP monitoring system within this organizational structure ensures a certain core of independent scientists. Third, having a permanent negotiating working group - the WGS - "computing" inputs from the other groups, provides a continuous formal forum for discussions on science-politics interface issues. Fourth, the gradual "rise" of the critical loads approach in the late 1980s means that various types of scientists are drawn into the LRTAP decisionmaking process in a perhaps hitherto unprecedented manner within the realm of international environmental cooperation.

## 5.4. Institutional impact — 'real' or 'spurious'?

### 5.4.1. *Did institutional design matter for the weight of scientific evidence in the sulphur protocol?*

As will be recalled, our assessment was that the acceptance and weight of scientific evidence were seemingly quite high. Is it likely that the weight of scientific evidence was related to the functioning of the LRTAP "scientific-political complex", with the EMEP system as a driving force? To some extent, yes. Regarding EMEP, the high priority given to coordination of national research programs and standardisation of data collection strengthened the international and general credibility of national research programs.<sup>120</sup> This in turn contributed to increased scientific consensus on transport mechanisms and patterns of transboundary air pollution. As maintained by Park (1987): "By this time (i.e. 1983/84), the scale and significance of the acid rain problem in Europe was clear to all, even those countries reluctant to act directly in reducing oxide emissions. Also beyond doubt was the trans-frontier character of the problem, in which some countries gained, whilst others lost" (p.177). Levy (1993) indicates that countries like Austria, Finland, Switzerland, and Austria were influenced primarily by LRTAP's knowledge-creating activities: "All of the governments were active participants in [LRTAP] collaborative programs, even though their official positions, initially, were that acidification was not a problem. Once these governments became aware of the extent of damage their countries suffered, they adopted positions favouring reductions in emissions" (p.121).

Hence, several factors indicate that institutional features connected to the LRTAP system contributed to the weight of scientific evidence. The question then becomes: how does this assumed causal relationship hold up when we control for background factors like "political malignancy" and "public saliency"?

Regarding degree of "political malignancy", the acid rain issue was of course a quite malign issue when the cooperation started in 1979;<sup>121</sup> emissions stemmed from important economic and societal sectors like power stations, industrial plants and transport, hence abatement efforts would affect "core" national economic activities, and potentially give rise to "malign" competitive effects between the countries. Moreover, although several countries were probably both emitters/"exporters" and receivers/"importers" of transboundary polluting substances, there were also substantial asymmetries: major emitters like the UK were seemingly able to "export" significant portions of their emissions to small Scandinavian emitters with vulnerable soil

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120 Viz Levy 'op.cit., '...Swedish and Norwegian research is better received in Britain today, for example, than it was before LRTAP (p.12).

121 For a further description of the acid rain problem, see Ch. 2.5 in Wettestad/Andresen: *The Effectiveness of International Resource Cooperation*, FNI, 1991.



characteristics. Last, but not least, it was only the Scandinavians who had experienced visible acidification damages. It has been maintained that when LRTAP was created, only two of its thirty members thought acid rain was a serious environmental problem.<sup>122</sup>

But this initial low responsiveness to scientific evidence started changing from 1982 on. Although most of the "fundamental" malign features were still there, the pattern of environmental affectedness and related damages started changing. Things started happening both at the national and international level. At the national level, the most important development was of course the sudden political about-turn of Germany (BRD) in 1982. Like most others, Germany had been a reluctant and passive party in the early LRTAP days. In 1982, extensive acid rain-related domestic "Waldsterben" made big headlines, contributed to a stepped-up domestic abatement program, and for obvious competitive reasons, a new "entrepreneurial" role for Germany on the international stage - be it LRTAP or the European Community. However: according to Levy (1993), the various effects of the German forest death "incident" would not have been as strong in the absence of the LRTAP network. The Scandinavian aquatic acidification "incidents" in the late 1960's occurred in the absence an international scientific and diplomatic network, and generated virtually no scientific and political reorientations abroad.<sup>123</sup>

At the international level, attention-raising meetings were held from 1982 on. First, there was the arrangement of the aforementioned 1982 Stockholm Conference on Acidification of the Environment. This conference has been hailed as a "success model" in shepherding an international consensus on scientific information concerning transboundary sulphur pollution: "At the expert meetings, over one hundred of the leading scientists, engineers and pollution control officials from 20 nations gathered to produce a detailed summary of the current state-of-the-art knowledge of the scientific, technical, and policy issues surrounding the acidification problem".<sup>124</sup> Second, international political meetings like the 1984 Ottawa Ministerial Conference and Munich Multilateral Conference on the Environment also contributed to a generally heightened public saliency of the transboundary air pollution issue.

Now, to sum up, where does all this leave us with regard to the impact of institutional design? Our overall assessment has to be that the LRTAP institutional setting, and especially the EMEP program, contributed somewhat to the weight given to scientific evidence. However, changes in the political malignancy and the increased public saliency of the acid rain issue in the first part of the 1980's are probably more important causal factors in this picture. Such an

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122 Levy (1993), op.cit., p.75.

123 Levy, op.cit., p.122.

124 Wetstone, G: "A History of the Acid Rain Issue", pp 185-186. This is Ch.12 in Brooks/Cooper: *Science for Public Policy*, Oxford 1987.

assessment is in line with other authors who have written on the science-politics relationship within the acid rain context. According to Wetstone (1987), "the level of media interest, the extent of public concern, and external factors, such as economics and international politics, played a key role in determining how governments responded to research data" (p.192). Levy (1993) observes that in June 1983 nine government were in favour of 30% cuts; the number had grown to eleven in March 1984, eighteen in June, and twenty by the year's end. He then maintains: "In effect, the eleven governments that did not consider sulphur dioxide reductions to be in their interest in 1983 changed their minds once the question became highly public and connected to normative principles" (p.94).

#### *5.4.2. Did institutional factors matter for the weight of scientific evidence in the NOx protocol?*

In the case of Nox, although acceptance of scientific evidence increased over time, we indicated that the weight of scientific evidence was perhaps somewhat lower than in the sulphur process. Does this mean that the scientific-political institutional apparatus functioned less effectively than in the sulphur process? As far as we can see, the answer here is negative. In this case, we are in the lucky position of being able to base our reasoning on a recent, quite detailed study of the Nox negotiation process, covering among other things the structure and impact of the institutional apparatus (Stenstadvold, 1991). In order to establish the necessary scientific basis for negotiations, the Working Group on Nox (WGN) was established in July 1985. Soon it became clear that both scientific uncertainty was high and consensus was quite low. In this situation, a successful knowledge-gathering process was organized. This process included, first, the systematic incorporation of existing knowledge in bodies like EMEP, Economic Commission for Europe (ECE), World Health Organization (WHO), and the OECD. Second, the establishment of several sub-groups (Task Forces, Working Groups, Working Parties) helped make a complex problem more manageable. Third, the use of specific "Governmentally Designated Expert Groups", comprised of governmental and "independent" experts, was an institutional innovation which was characterized as "a very effective organizational tool which speeded up the process".<sup>125</sup> According to Stenstadvold, the outcome of this well-organized knowledge-gathering process influenced the negotiating Parties' preferences considerably in favour of the establishment of an international Nox protocol.<sup>126</sup>

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125 Interviews with Per Bakken, Norwegian Ministry of Environment, and Anton Eliassen, Norwegian Meteorological Institute, cited in Stenstadvold, op.cit., p.92.

126 Stenstadvold, op.cit., maintains: "The information gathering and diffusion process, and the way this was organized, included several institutional innovations regarded as very successful. This organizational approach involved the Parties in a manner which stimulated a common problem understanding, and paved the way for more constructive negotiations than would otherwise have been the case" (p.102, my translation).

Hence, the question then arises: was the seemingly lower weight given to scientific evidence caused by a politically malignant problem? This seems more likely. First, like in the case of SO<sub>2</sub>, Nox emissions also stem from "vital" societal activities, especially related to power plants and automobile emissions. As indicated earlier, the competitive aspects related to automobile emissions regulations played an important role in the negotiations. It has generally been maintained that the regulation of such emissions has "enormous industrial and commercial implications" (Boehmer-Christiansen/Skea, 1991).<sup>127</sup> In the sulphur process, a strong coalition supporting international regulations emerged over time, including prominent Continental states like Germany and France, in addition to the Scandinavians. This coalition broke down in the Nox negotiations, as countries like France and Italy joined forces with the "traditional" LRTAP "laggard", the UK. Furthermore, countries like Norway and Finland became much more passive, as they during the negotiations "discovered" costly emissions.<sup>128</sup> Hence, the drive to forge a scientific and political consensus was weakened compared to the sulphur process.

Was the political malignancy accompanied by a lower public saliency than in the sulphur process? Possibly, but the question is difficult to assess. On the one hand, the sulphur process and growing, visible evidence of forest damages in an increasing number of European countries up through the 1980's ensured a certain public saliency related to air pollution problems that benefited later negotiation processes like NO<sub>x</sub> and VOCs. On the other hand, the Nox process period, i.e. 1985-88, lacked both the occurrence of national "acid dramas" comparable to the German "Waldsterben" events in early 1980's, and the frequent and high-level meetings of the sulphur process. Hence, on the whole, our impression is that the public saliency related to the Nox problem was somewhat lower than was the case in the sulphur process.

To sum up: LRTAP institutions seemingly performed quite well in this case, but the weight of scientific evidence was reduced first and foremost by the economic and political malignancy of the problem, and possibly also by a somewhat lower public saliency related to the Nox problem.

#### ***5.4.3. Institutions and the critical loads approach***

As indicated earlier, the SO<sub>2</sub> protocol revision process has so far witnessed a very interesting close collaboration between various types of scientists and administrators. Not least the economic input has been considerably stronger than witnessed in international environmental cooperation so far. It seems reasonable to maintain that the systematic preparatory work in

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127 Boehmer-Christiansen/Skea, op.cit., p.22.

128 Stenstadvold, op.cit., p.112.

various LRTAP subgroups has been a pivotal factor in the critical loads process, especially the "Task Force on Integrated Assessment Modelling", with its close collaboration with the International Institute for Applied Systems Analysis (IIASA). This collaboration seems to have been generally successful, as IIASA's suggested reduction targets seem to have been accepted by most of the negotiating parties, even if several of them face considerably tougher targets than under the current regime. Here, the "international" dimension of IIASA's work is probably of crucial importance in securing its legitimacy.

A less important, but still relevant LRTAP institutional feature which *may* play a role in these negotiations, is the verification capability represented by the EMEP monitoring system. It has for instance been maintained that "few other international agreements can be said to come equipped with verification instruments of this calibre".<sup>129</sup> A differentiated and complex agreement based on critical loads will need as much "independent" verification as it can get.

On the whole it is important to bear in mind that the outcome of these negotiations is still uncertain. Accordingly, it is too early to determine precisely the impact of this quite unique institutional setting. As indicated earlier, it is reasonable to assume that various economic and political issues will come in heavily in the final negotiating stages. Cash-strapped countries like (the former) Czechoslovakia, Poland and Hungary face around 70% additional abatement costs under a critical loads-based protocol, and some form of "burden-sharing" mechanism has already been discussed. It should also be mentioned that an alternative "Best Available Technology" (BAT) approach has been discussed. This technology-based approach is precautionary in nature, and implies that emissions should be reduced as far as is technically possible.<sup>130</sup> However, the critical loads-approach has dominated the negotiations so far.

## 5.5. Concluding remarks

Judged on its own, the LRTAP scientific-political complex has seemingly achieved a nice combination of a quite high (and increasing!) involvement in the political process and a lower, but still significant amount of scientific autonomy/integrity. This does not mean that the institutional arrangements can be labelled "perfect". As indicated by the recent organizational restructuring, the system had probably grown a bit too complicated, with overlap of work and inadequate communication between some bodies. Moreover, the role of the Secretariat warrants some specific comments. Both observers and insiders seem to agree that funding is far from adequate, considering the workload. In fact, personnel resources have remained unchanged in the period after 1985 - a period in which, among other things, two new

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129 Sand, P., *op.cit.*, p.259.

130 Nordberg, *op.cit.*, p.10,

protocols have been concluded and a revision process regarding the sulphur protocol has been started. This situation obviously constrains the Secretariat's possibilities of fulfilling its coordinating and synthesizing function in the internal communication chain.

Moving on to the crucial impact question, in our view, the LRTAP case is a good example of the complex relationship between institutional design and the weight given to scientific evidence. Although, as indicated above, the institutional machinery has been functioning quite well on its own premisses, its impact with regard to the acceptance and weight given to scientific evidence has been variable. True, institutional features have mattered in various ways. The EMEP system contributed somewhat to the wide recognition of the long-range transportation thesis in the SO<sub>2</sub> case. Moreover, as stated by Levy (1993): "[LRTAP] established an expanding range of scientific working groups which harmonized data collection, evaluated competing explanations of damage, and assessed environmental threats. This process was crucial for keeping alive political momentum after political crises faded from view, and for enabling the institution to enter its current stage, in which the regulatory agenda is determined more by scientific consensus than political shocks"(p.132). However, in both the sulphur and Nox processes, the weight given to scientific evidence is more compellingly explained by background factors like political malignancy and public saliency. Finally, in the sulphur/critical loads negotiations so far, institutional features like the close collaboration with IIASA on the development of the revised reduction targets seem to have been quite successful. But the ultimate weight of scientific evidence will probably be considerably influenced by economic and political background factors, given the size of some of the emissions reductions envisaged and various allocations aspects.

One final reflection: the most important challenge that this system is now facing, may be that with the adoption of the critical loads approach, direct scientific involvement in the decisionmaking process is increasing considerably. The "administrative" participation model, with scientific working groups composed of government officials, has seemingly not spurred complaints about "politicization" so far. Whether an even higher involvement by scientific experts in the political process can be accomplished without seriously threatening the fundamental scientific legitimacy of the system, remains to be seen.<sup>131</sup>

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131 For some further notes on the relationship between "usability" and "credibility" in relation to the role of science in international environmental regimes, see Andresen/Skjærseth/Wettstad: "International Efforts to Combat Marine Pollution: Achievements of North Sea Co-operation and Challenges Ahead", *Green Globe Yearbook 1993*, Oxford University Press.

## VI The Ozone Regime<sup>132</sup>

### 6.1. Introduction

#### 6.1.1. *The history of science*

In 1974 Richard Stolarski and Ralph Cicerone, two Michigan University scientists, published a theory indicating that chlorine released in the stratosphere could destroy ozone. At the same time, but independent of the Michigan findings, Mario Molina and Sherwood Rowland discovered that chlorofluorocarbons (CFCs) are not broken down in the lower atmosphere, as most other gases, but persist and migrate slowly up to the stratosphere where they eventually are broken down by solar radiation. In this process, large quantities of chlorine is released in the stratosphere. The combination of the two theories therefore, implied a link between CFCs and ozone depletion. The Rowland-Molina hypothesis set off a major research programme conducted by the National Academy of Science (NAS), the National Aeronautics and Space Administration (NASA), the National Oceanic and Atmospheric Administration (NOAA) and leading Universities. In the following years the validity of the chlorine-ozone linkage was confirmed, but the hypothesis could not be definitively proved.

The first international initiative on the ozone issue, was the UNEP-sponsored Washington Meeting held in March 1977. Representatives of 33 nations and the EC Commission participated. The meeting drafted the "World Plan of Action on the Ozone Layer", and recommended the establishment of a "Coordinating Committee on the Ozone Layer" (CCOL). In May 1977, UNEP's Governing Council created the CCOL and adopted the Plan of Action. Until 1982 the CCOL was the only formal international body concerned with the ozone issue. The main task of the CCOL was to coordinate research undertaken by national and international agencies (see for instance Parson, 1991).

In an effort to "...provide governments around the world with the best scientific information currently available on whether human activities represent a substantial threat to the ozone layer", a major cooperative scientific venture was launched. (Benedick, 1991:6). The project

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was cosponsored by three US scientific agencies (NASA, NOAA, FAA), three international organizations (WMO, UNEP, EC) and a West German scientific agency (The German Federal Ministry for Research and Technology) and resulted in a scientific assessment report published by WMO and UNEP in 1986. The assessment was generated through a series of workshops held in 1984 and 1985, with the participation of approximately 150 scientists from 11 countries (Benedick, 1991, Litfin, 1991). The report was "...the most comprehensive study of the stratosphere ever undertaken" (Benedick, 1991:14), and may be seen as a first landmark of the evolving scientific consensus on the ozone issue.

In 1988, the Ozone Trends Panel (OTP), initiated by NASA in 1986, presented its report; the result of "...a 16 month scientific exercise involving more than 100 scientists from 10 countries." (ibid.:110). With this report, the scientific consensus on the ozone issue was confirmed: "Ozone layer depletion was no longer a theory; at last it had been substantiated by hard evidence. And CFCs and halons were implicated beyond reasonable doubt." (ibid.).

In October 1988 scientific input to the process was institutionalized in four Assessment Panels (scientific, environmental, technological and economic). The integrated summary of the panels' conclusions, the Synthesis Report, was presented in 1989.

### *6.1.2. The history of politics*

In 1981 UNEP's Governing Council approved a Swedish initiative to establish a working group to start negotiations on an international convention on the ozone layer. The first meeting of the group took place in Stockholm in January 1982. The CCOL was designated to be its scientific and technical advisory body. The Working Group met seven times in the following three years, when the Vienna Convention was adopted. In the 1985 Vienna Convention the signatories agreed to take "appropriate measures" to protect the ozone layer, but "...it made no effort to define such measures..." (Benedick, 1991:45). The convention did, however, empower UNEP immediately to convene Working Group negotiations for a "follow-up" protocol. Under the leadership of UNEP's Executive Director Mustafa Tolba (from 1986), these meetings eventually led to agreement on the Montreal Protocol two years later, in 1987. In the Montreal Protocol the Parties agreed to a 20% cut, from 1986 levels, of production and consumption of the five major CFCs by end 1994, and a 50% cut by end 1999 (WMO, 1992). It was further agreed to freeze, at 1986 levels, production and consumption of halons by 1992. By December 1988, 29 countries plus the European Commission had ratified the treaty, and the Protocol entered into force January 1 1989. The First Meeting of the Parties took place in Helsinki in May 1989. The meeting resulted in the Helsinki Declaration approved by consensus. This was a nonbinding document, calling for phase out of CFCs "...as soon as possible but no later than the year 2000. Furthermore, somewhat more vaguely, it also called for a phase out of halons and "other ozone depleting substances" not covered by the Montreal Protocol "as soon as feasible". (Benedick, 1991:125). At the Second Meeting of the Parties

in London, in June 1990, the London Amendment was adopted. The parties agreed to increase the scope of the Montreal Protocol by including several gases not covered by the 1987 agreement (several CFCs, carbon tetrachloride and methyl chloroform), accelerate CFC phase out (a 20% cut of 1989 levels by 1993, an 85% cut by 1997, and 100% phase out by 2000), and to phase out halons by the year 2000 (with a 50% cut of 1986 levels by 1995). (Benedick, 1991, WMO, 1992).

In November 1992, the parties of the Montreal agreement met in Copenhagen. At this meeting it was agreed to accelerate phase out further; a 100% phase out by 1996 for CFCs, and by 1994 for halons. In the Copenhagen adjustment the Parties also agreed to regulate HCFCs; a consumption cap beginning 1996, and gradual phase out between 1996 and 2020. A 0.5% production "tail" will extend for 10 years (GECR, 4 December 1992).

## **6.2. Did scientific evidence count?**

The ozone treaty has been highly praised as one of the most successful efforts of international environmental cooperation to date (see for instance Parson, 1991; Benedick, 1991). One explanation of this, it has been claimed, rests in the cooperation and dialogue that prevailed between scientists concerned with the ozone issue and decision-makers concerned with negotiations on a convention to protect the ozone layer. Before we look into the institutional variables of this case, we will examine which indications we have that this holds true. That is, before we can study the impact of institutional aspects, we need to have some indications of the "score" on our dependent variable; the extent to which scientific recommendations were recognized as legitimate advice, and the extent to which the advice was acted upon.

In order to trace the impact of scientific input in this case, we will start by investigating the correlation between some main events in the scientific process (such as the publication of major findings, important meetings etc.) and agreements and/or altered positions in the corresponding political process. Secondly, we will proceed by studying the extent to which scientific recommendations are reflected in the agreed outcomes of the political process.

The political process on ozone depletion is characterized by a sequential approach; first adopting a framework convention with few obligations in terms of specific control measures, proceeding with agreement on a protocol and eventually protocol amendments gradually increasing the parties' commitment to freeze, reduce and phase out major ozone depleting substances. Furthermore, each step of this process seems to be correlated with the development of the scientific process; the gradually increased scientific understanding of the ozone problem. Three events in particular are illustrative in this respect:



The Würzburg Meeting in April 1987 concluded that the chemicals CFC113 and halons 1211 and 1301 could cause significant ozone depletion even if the other CFCs were controlled. In September the negotiators succeeded in including these controversial chemicals in the Protocol, although a freeze on halons was successfully delayed by the EC Commission: "After the Würzburg findings were thoroughly aired, there was no support at Montreal outside the European Community for narrow chemical coverage." (Benedick, 1991:79).

Furthermore, the OTP Report of March 1988 seems to have been the major catalyst to the altered positions of the United Kingdom in 1988. During the summer of 1988 the U.K. "...was transformed from a reluctant follower to a world leader in the drive to protect the ozone layer." (ibid:114). The role of DuPont in this transformation may also have been significant, a point to which we will return in section IV. This development also had significant impact on the speed of ratification of the Montreal Protocol. With the EC ratification in December 1988, a delay in the Protocol's entering into force was prevented at the last moment.

Finally, the Synthesis Report of the four expert panels (institutionalized in October 1988) presented in November 1989, and especially the "chlorine-loading" approach<sup>133</sup> adopted by the Science Panel, became the basis for negotiations on the Protocol Amendments finally agreed upon in London 1990.

This "symmetry" indicates that scientific input did have impact on the outcome of the decision-making process, at least in the sense that the findings presented were accepted as legitimate. In order to substantiate this proposition further, however, we shall look into the extent to which the scientific advice also was acted upon; i.e., the extent to which scientific advice is reflected in the agreements made.

Parson claims that: "It was not science, but bargaining, that determined the decisions adopted in Montreal. The 50% cut that was agreed had no particular scientific prominence." (1991:25). The scientific community was at that time divided between those favouring much weaker control measures (CFC freeze only) and those favouring much stronger control measures (such as 85% cuts). The 50% cut agreement therefore, was a bargained outcome between the EC's proposed freeze and the US proposition of 95% cuts (ibid.). Similarly, Litfin argues that the development of the US position was not the result of scientific evidence in itself, but rather the interpretation of science (Litfin, 1991).

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133 Already in 1985, Harvard scientists worked with a theory of "threshold levels" of chlorine concentrations: that a sudden collapse of ozone concentrations might occur once the amount of chlorine passed a certain level (Benedick, 1991). By the shift of focus, from measurements of the "ozone depleting potential" (ODP) of different gases, to chlorine loading, scientists were able to assess at which levels of chlorine concentrations damage to the ozone layer had occurred, and accordingly, at which levels the ozone layer would recover.

Furthermore, it seems as if the scientific community did not really formulate explicit advice beyond a general "controls-are-necessary" statement. On the other hand, this "advice" became more and more substantiated as knowledge accumulated, uncertainty was reduced, and the urgency of acting was emphasized by the scientific community. Moreover, at the pre-negotiating Workshops in Rome and Leesburg in 1986 (arranged by UNEP) EPA scientists formulated "indirect advice" by focusing on the *implications* of negotiating positions (primarily the EC position) in terms of predicted ozone depletion. Similar examples are found at the Würzburg Meeting, where the implications of not including CFC113 and halons were emphasized, and later in the Synthesis Report's adoption of the chlorine-loading approach. By focusing on chlorine-loading instead of ozone depletion, the scenario modelling field was opened up "...to anyone with a powerful desktop computer." (Parson, 1991:27).

Thus, it seems as if scientists, rather than formulating explicit advice, provided decision-makers with the *tools* to investigate, for themselves, the impact of their decisions. The approach also seems to have worked, given the pace and scope of the evolving agreement.

On this basis, it seems reasonable to conclude that the scientific input to the political process did serve as decision premisses, although the exact control measures adopted were largely determined by negotiations, not explicit scientific advice. To this extent, therefore, the scientific knowledge presented by the scientific community seems to have been accepted as legitimate and acted upon by decision-makers.

### **6.3. Institutional arrangements**

In chapter one we suggested that one key to a constructive science-politics relationship would be to *combine* scientific autonomy on the one hand, and involvement in the political process and responsiveness to questions and "needs" on the other. Each dimension is conceived of as a function of multiple variables. In this section we will study each dimension in terms of the indicators listed in chapter one.

#### **6.3.1. Degree of autonomy: The process of producing knowledge**

A general characteristic of the ozone process is that before 1988, when the four Assessment Panels were established, the science-politics relationship was formally institutionalized to only a very small extent. Illustrative in this respect is the fact that until the Ad Hoc Working Group started negotiations on preparations for an international convention on ozone in 1982, CCOL was the only formal international body concerned with ozone. The process of producing new knowledge and developing scientific consensus was largely initiated, conducted and controlled by scientific institutions themselves. The group of scientists involved in the process enjoyed

high autonomy, and their work was to a very small extent directed or controlled by any political body.

The group of scientists involved in the process before 1988 therefore, constituted a network rather than a formal body. This network, however, seems to have been quite firmly connected; throughout the process, the same scientific institutions (to some extent even the same scientists) were mainly responsible for initiating and coordinating research. Furthermore, one country, The United States, dominated the field in terms of intellectual capital, although important contributions also came from other continents.

The process was thus dominated by U.S. scientific agencies; most of the research was initiated, conducted and carried out by U.S. scientific institutions, with NASA serving as a driving force, and research funding was primarily provided by U.S. national institutions, governmental, industrial as well as scientific. Illustrative in this respect is the incident where the British Antarctic Survey, after having discovered the Antarctic ozone "hole" in 1985, was denied necessary additional funding from the British Government due to budgetary constraints. Ultimately the funding was provided by a U.S. industry association, "...because U.S. industry wanted an early resolution of the scientific uncertainties". (Benedick, 1991:30). Often, however, although carried out by U.S. institutions, research findings were published by intergovernmental organizations (separate from the regulatory body), such as WMO and UNEP, implying broad distribution of the results.

Research was primarily conducted by U.S. scientific agencies, but scientists from all over the world were invited to participate in scientific assessments. Thus, entry was restricted, but the criteria for participation were both scientific and political. With regard to this aspect, two tendencies seem to be significant: Separate research projects conducted by one, or a small number of scientific institutions were primarily carried out by the institutions' regular staff of scientists, or "specially invited" scientists from other institutions (both national and from abroad). For this category of research activities therefore, entry was primarily based on scientific criteria such as scholarly merits. In joint venture projects and Workshops, on the other hand, criteria for participation were both scientific and political. In the scientific assessment coordinated by Bob Watson (NASA) and published by WMO/UNEP in 1986, the political motivation behind broad participation, to mitigate nationalistic biases, is evident in the fact that Bob Watson would say: "Let's go ahead and have a person from a certain country, even if that person has nothing to contribute. We might be able to get something started there, or at the very least that person might be able to take the message home." (Cicerone in interview with Litfin, 1991:6). Furthermore, it seems as if the more politically nominated participants to some extent were being "used" as "intermediaries" between scientists and each individual representative's respective government in order to "take the message home". Thus, the political necessity of broad (and to some extent politically motivated) participation was accommodated by scientists by maintaining a firm control of the process and

so preventing these actors from gaining any significant influence on actual research and scientific conclusions.

The main function of the scientific institutions involved in the process was to do research; produce new knowledge, and coordinate and supervise research activities. There are several examples of institutions serving both these functions, for instance NASA. The main coordinating body, however, was the CCOL, whose main function was to "...bring together scientists from governments, industry, universities, and international agencies to assess the risks of ozone layer depletion." (Benedick, 1991:40). The CCOL assessments were published in periodic reports that served as important references for policy-makers. The character of CCOL is usually described as being primarily scientific. However, Parson notes that before the working group was established in 1982 CCOL reports had "...moved from science summaries toward cautious calls for international regulatory action." (1993:37). Thus, before the establishment of a political body at the international level, the CCOL also to some extent formulated policy advice. With the working group establishment, however, the CCOL "returned" to their primarily scientific functions.

The chemical industry participated actively in the provision of scientific assessments, represented by the Chemical Manufacturing Association (CMA) in CCOL. This involvement "...led to their substantial support of ozone research, and to their final support for the Montreal agreement..." (Isaksen, 1992:4). By involving governmental representatives in CCOL, a continuous update on the science to governments was also achieved. Thus, the CCOL seems to have played an important role, in addition to the coordinating function, by providing arenas for science-policy dialogues. Furthermore, it seems as if it was mainly at this level, more or less as an outcome of the dialogue, that scientific findings were transformed into decision premisses and policy recommendations formulated.

In October 1988, as an implication of Art. 6 in the Montreal Protocol but before the Protocol formally entered into force, the scientific process was institutionalized and formally subordinated the political body, with the establishment of four expert panels; a scientific, an environmental, a technological and an economic panel.<sup>134</sup> The main task of the panels was to provide the necessary knowledge basis for assessment and review of the control measures adopted in the agreement, to take place every four years beginning in 1990. The science panel "...was reviewing and analysing implications of the most recent measurements and predictive models of stratospheric ozone trends, as well as updating calculations of ozone depletion, chlorine loading, and global warming potentials for various chemicals — including those being developed as substitutes for CFCs." The environment panel "...was assessing the state of knowledge of health and environmental effects of altered ozone concentrations and increased

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134 Art.6 of the Montreal Protocol: Assessment and Review of Control Measures.

ultraviolet radiation." The technology panel "...was analysing technical options to determine and quantify the feasibility of reducing ozone-depleting substances", and the economics panel "...was examining the benefits of reduced use of CFCs and halons, the costs of technical solutions, and the implications for technology transfer to developing countries." (Benedick, 1991:128). The panels were meant to replace the CCOL although it still formally exists. No meetings in the CCOL have however been arranged, since the panels were established (personal communication with Per Bakken).

The most important implication of the institutional change in 1988 was that of broader participation. Before 1988 the scientific process was heavily dominated by industrial nations, the US in particular, more or less as a consequence of the existing structure of scientific networks. Other representatives participated only to the extent that they were invited by leading scientists and scientific institutions, and usually only in connection with isolated projects or conferences (see above). The institutional change, therefore, served to increase the geo-political representativeness of the process (personal communication with Per Bakken). Furthermore, before 1988 the process had mainly concerned the science of ozone depletion and to a lesser extent focused on other aspects related to the problem, such as environmental effects, technological options for substitution of ozone depleting substances and economic calculations on costs. Thus, the establishment of the four expert panels also served to broaden the area of research by including issue areas that had not been covered in a systematic manner earlier.

Although the scientific body now became directly subordinated the political body of the regime (the Meeting of the Parties to the Montreal Protocol), scientists still seem to have enjoyed high autonomy. In all panels recruitment was mainly based on scholarly merits, the participants were active in research and all reports were "peer-reviewed" according to the standard procedures of science at large.<sup>135</sup> In the terms of reference for the Panels, it is pointed out that the panels "...shall consist of selected experts who are qualified in the field ... and internationally recognized as such." It is, however, also pointed out that "the experts who are best qualified in the subject matter of the various chapters [of each report] shall be selected ensuring *the widest possible geographical balance*." (UNEP/OzL.Pro. 1/L. 1/Add. May. 1989, italics added).

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135 To the report of the science panel 136 scientists from 25 countries contributed (87 scientists from 15 countries prepared the report, while 78 scientists from 23 countries participated in the peer review process). To the report on environmental effects 48 scientists from 17 countries contributed (20 scientists from 8 countries prepared the report, while 28 scientists from 12 countries peer reviewed it). To the report of the technology panel 110 experts from 22 countries prepared the report, while "an even greater number, involving experts from additional countries" peer reviewed it. To the report of the economic panel 24 experts from 12 countries prepared the report, while it was peer reviewed by 25 experts from 18 countries. (2nd Draft Synthesis Report, September 1989, UNEP/ozL.Pro.WG.I (2)/L.1/Add.1).

The funding mechanism remained largely unchanged; that is, research was mainly funded by national governments and by the traditional mechanisms for research funding at national levels. The technology panel, however, was organized into sectoral sub-panels which mostly consisted of user industry experts who were financed by their companies (Parson, 1991).

The formal subordination of the scientific body did imply some restrictions on the panels' operational autonomy as their mandate was now given by the regulatory body of the regime. The meeting of the parties to the Protocol decided the chairmen of the panels as well as the chapters the reports should include. The subdivision of the issue area into chapters, however, seems to follow "natural" areas of specialization within the research field at large, and does therefore not seem to represent a substantial restriction in the panels' operational autonomy. The chairmen of the panels were responsible for selecting "chapter chairmen", and the panels were free to include whoever they found competent to contribute to the assessments in accordance with the terms of reference set up by the meeting of the parties. Substantially, therefore, the panels' operational autonomy seems to have been relatively high, despite the restrictions implicit of the subordination.

The panels seem to have been generally regarded as fora "...with the stamp of international objectivity and authoritativeness." (Parson, 1993:61). With regard to the report of the economic panel, however, critical remarks were raised regarding the objectivity of the assessment. In this panel only two representatives from developing countries participated, and at the second session of the first meeting of the parties to the protocol, several delegations found that some of the parts of the Synthesis report concerning this aspect "...did not fully reflect some of the points that these delegations regarded as fundamental to an objective economic analysis, so that they felt bound to express a reservation in regard to those conclusions..." (Final Report, UNEP/OzL.Pro.WG.I (2)/4), Sept. 1989). The criticism was met by adopting the report *Ad Referendum*, implying an opportunity to comment and modify the report at subsequent meetings, and by establishing a drafting group in order to integrate proposals for amendments of the report. It was, however, agreed that any amendment "had to stem from the Panel Reports", and that "additional technical information" not included in the panel reports could be included in the meeting report rather than in the Synthesis Report (Final Report, Sept. 1989).

### 6.3.2. Degree of involvement: Communication between scientific and regulatory bodies

Before 1988 the formulation of advice and the performance of research were to some extent separated. Scientists primarily did research, while the outcome of CCOL activities constituted the formulation of advice.<sup>136</sup> The CCOL, on the other hand, was designated to serve as a technical and advisory body to the Ad Hoc Working Group established in 1982, and thus served as the main link between the scientific and the political segment. Throughout the process several workshops were arranged with a more or less explicit "political" function (for instance the workshops held in Rome and Leesburg in 1986); they were political in the sense that decision-makers were invited, and that (tacit) negotiations took place, but the main (official) function of these workshops was the dialogue between scientists and decision-makers. Through these workshops a feedback mechanism was established. Atmospheric scientists participating in the provision of scientific assessments were therefore, indirectly involved in the political process, mainly through their participation in the advisory body, CCOL, which also served as the main communication channel between scientists and policy-makers.

With the establishment of the four expert panels in 1988, the scientific segment became a formal body subordinated to the political body of the regime, and thus more directly involved in the process. The indirect and implicit character of the formulation of advice seems, however, to have remained unchanged. The mandate of the panels was to assess the control measures adopted in the Montreal Protocol in terms of their effectiveness with regard to recovering the ozone layer and preventing possible damage from ozone depletion, and an implicit policy advice followed by implication of their conclusions. The Synthesis Report concluded that "even if the control measures of the Montreal Protocol were to be implemented by all nations, today's atmospheric abundance of chlorine (...) will at least double to triple during the next century" (Draft Report, UNEP/OzL.Pro.WG. I(2)/L.1/ Add.1, Sept. 1989:6). By the shift of focus in the science panel, from ozone depletion to stratospheric chlorine concentrations ("chlorine loading", see page 85) they were able to assess at what point, in terms of parts per billion by volume (ppbv), damage to the ozone layer had occurred, and accordingly at which levels of chlorine concentrations the ozone layer would recover. They concluded that "the Antarctic ozone hole will not disappear until the atmospheric abundance of chlorine is reduced to the levels of the early 1970s: 1.5 - 2 ppbv assuming present climate" (as compared to the 1989 levels of 3 ppbv) (ibid.:15). By studying four scenarios with different regulatory measures ranging from freeze to full phase-outs, they found that "...even with a global phase out, chlorine will continue increasing to about 4.5 ppb[v] around 2010,

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136 A too rigid dichotomization of this aspect, however, may prove artificial in this case: When scientific research discovers\* that man made emissions of pollutants can lead to ozone depletion, the conclusion, by implication, holds a recommendation to reduce emissions.

and will not return to 2 ppb[v] until 2060." (Parson, 1991:16). With these scenarios and the conclusion from the technology panel that phase-outs were technically feasible, the report certainly implied recommendations to phase out ozone depleting substances, although the recommendations were not stated explicitly.

The implications of the scientific findings of the panels were, however, "interpreted" and transformed into policy advice by, *inter alia*, the Executive Director of UNEP, Mustafa Tolba. In his opening statement he stated that a strengthening of the Protocol was required in order to "protect our planet's ozone layer." (Final Report, Sept. 1989). On the basis of the reports he made several "...specific recommendations for adjustments and amendments to the Montreal Protocol for consideration by the participants." (ibid.).

Throughout the process, therefore, the functions of research, policy advice and policy-making seem to have been quite clearly differentiated, at least in the sense that different segments of the process served different functions. Before 1988, the CCOL seems to have served an important function as a link between the scientific and the political segment, while Dr. Tolba seems to have served the same function after 1988, in his professional capacity as the Executive Director of UNEP under which the process was organized. The formulation of advice also seems to have taken place at this level, and not by the scientists themselves.

This does, however, add up to a quite complicated picture of the scientific process' involvement in the political process. Before 1988 the scientific and the political segments were separated, but both processes (the CCOL, with regard to the scientific process) were formally organized by UNEP. Between the CCOL and the political process, therefore, the organizational distance was relatively "short". For instance Dr. Tolba participated actively at both levels. The scientists in the process were, however, only indirectly involved in the political process, through the CCOL (although some of the front figures of the scientific process also constituted a "link" between the segments, and were, to some extent, more directly involved in the political process). The organizational distance between research, formulation of advice and policy-making was therefore present and clearly greater than after the subordination of the scientific process in 1988. After 1988 a separation was no longer provided for by institutional means as it was before 1988, when CCOL seems to have served as a "buffer" between the bodies. In practice, however, the functions were still separated, mainly because scientists left it to others to interpret and translate their findings into explicit policy implications and not as an effect of the institutional design.

Throughout the process, communication channels between the segments were provided. However, before 1988, although both processes (the CCOL and the negotiations) were organized under UNEP, no *regular* channels of communication existed. Communication took place indirectly; through the CCOL and the various workshops that were arranged. Parson argues that one implication of the institutionalization was that it "...provided a channel for



science to feed directly into the negotiation process..." and that this served to increase scientists' influence on the negotiations (Parson 1993:61).

## **6.4. The impact of institutional arrangements compared to the impact of other factors**

### ***6.4.1 The impact of institutional arrangements***

The preceding discussion of the ozone process in terms of the indicators we listed in chapter one, gives an impression that in the ozone case, a balance between autonomy and involvement has been achieved. Furthermore, it seems as if scientists' involvement in the political process gradually increased during the process without significantly reducing the process' autonomy. In this section we will discuss the impact of this institutional design on the outcome of the process, as compared to the impact of other factors: "How much" of the outcome may be explained by institutional design, and "how much" is ascribed to other factors? "The outcome" is here understood as, first, the extent to which scientific findings were accepted by the political body as legitimate knowledge on which to base decisions, and second, the extent to which scientific findings were acted upon by the decision-making body.

When the potential link between CFC gases and ozone depletion was discovered in 1974, "...the international scientific community was well organized and the foundation for large scale research on stratospheric ozone was in place" (Isaksen, 1992:1). This seems to have had significant implications for the manner in which the scientific process functioned and the science-policy dialogue was organized. The process of knowledge accumulation seems to have been primarily science driven in the sense that the process largely worked according to the existing scientific networks, and the norms and standards of the scientific community. Thus, the institutional design of the scientific process on ozone seems to a larger extent to be a function of the existing structure of the scientific community, than the other way around. If anything, this probably also constitutes the main feature of the institutional design in this case; the institutional design permitted to a large extent the science on stratospheric ozone to work according to its own traditions, rather than "forcing" a structure, "adapted" to the enlarged political context of the issue, upon the scientific community that was already there.

Interesting from our point of view, is that it is not quite clear whether this institutional framework was the result of a deliberate design. It seems rather to have been the result of the scientific community's ability to "capture" the process. It could be argued that before 1988 the relationship between the scientific and the political segment was characterized by informal *collaboration* rather than formal *institutionalization*. The scientific input to the political process was provided by an already existing scientific network with the will and ability to

communicate their findings to decision-makers. Thus, at least before 1988, the scientific "body" seems to a very small extent to represent an institution in accordance with our (implicit) definition of the term; as an institution with formalized procedures and rules established for a specific purpose.

A logical implication of this statement seems to be that the impact of institutional arrangements was necessarily low; the process worked according to its own rules, and the outcome would have been the same regardless of how it was organized. Such a conclusion, however, disregards the impact of the CCOL, which served an important function as a link between the political and the scientific segments. Although all actual research took place outside the CCOL, the CCOL served as a coordinating instrument, and not least as a channel for communication in "both directions". Through the CCOL scientists' involvement was increased, in the sense that their findings were fed into the political process and that they to some extent were responsive to the questions and "needs" of decision-makers.

The legitimacy of the scientific findings seems to stem from the fact that the scientific process, directly or indirectly linked to the political process, was perceived as representative for, and working according to, the norms of the scientific community at large. The most important aspect of the institutional design in this regard, seems to have been that of participation. Both before and after the institutional change in 1988 it was emphasized that recruitment should be based on scholarly merits; before 1988 as a more or less implicit function of the network structure of the process, after 1988 explicitly stated in the terms of reference for the expert panels. Moreover, it was largely left to scientists themselves to decide upon who were best qualified to contribute to, or review the reports, with due consideration, however, of ensuring the widest possible geographical balance. The provision of knowledge, therefore, seems to have been recognized as "objective and authoritative", despite the fact that scientists from industrialized countries (particularly the USA), also after 1988, dominated the scene (see also Parson, 1991 and 1993).

Thus, the institutional structure seems to have had significant impact on the first dimension of our dependent variable; the extent to which scientific findings were *accepted* by the political body as a legitimate basis for decisions. In politics, however, accepting that a problem exists does not automatically lead to actions directed towards its solution, and in this case it seems as if the extent to which scientific findings were *acted upon* is better explained by other factors than the institutional design of the science-politics dialogue.

We have chosen to focus on three other factors that may influence the outcome of the science-policy relationship in environmental and resource regime formation: the impact of already existing knowledge on the issue area, the impact of characteristics of the problem, and the political process, and the impact of the public saliency of the issue.

#### **6.4.2. The science on ozone depletion**

We have already pointed out that the existing scientific network on stratospheric ozone had a significant indirect impact on the outcome, through its implications for the manner in which the process was organized. Another related, but somewhat different factor that should also be given some attention in this case regards the scientific progress that took place during the process. Initially the ozone depletion theory was characterized by a significant amount of scientific uncertainty. During the process, however, this uncertainty was gradually reduced, and with the report of the Ozone Trends Panel, released in 1988, "Ozone layer depletion was no longer a theory; at last it had been substantiated by hard evidence...The panel concluded that the evidence "strongly indicates that man-made chlorine species are primarily responsible for the observed decrease in ozone"." (Benedick, 1991:110). Thus, policy-makers were (increasingly) confronted by scientific findings that were very hard to disregard or question, and an issue area in which a strong scientific consensus prevailed. Parson points out that "...particularly given the strong results they had to report, scientists' influence over the negotiations has advanced beyond the prior agenda setting role, to exercising substantial influence over certain aspects of the negotiated decisions." (1991:26). In addition to strong results and scientific consensus, the scientific findings became more and more alarming: In the synthesis report scientists concluded that "the scientific basis for the 1987 Montreal Protocol on Substances that Deplete the Ozone layer was the theoretical prediction that, should CFC and halon abundances continue to grow for the next few decades, there would eventually be substantial ozone layer depletion. The research of the last few years has demonstrated that actual ozone loss due to man-made chlorine (i.e., CFCs) and bromine has already occurred, i.e., the Antarctic ozone hole." (2nd draft, Synthesis Report, Sept. 1989). In London too, as negotiators were considering the proposed revisions of control measures, they were confronted by new and alarming data presented to them by the Norwegian scientist Ivar Isaksen and G.O.P.Obasi, the secretary-general of the WMO: "Although the new data were still provisional, ground-based measurements from northern Europe and Canada appeared to indicate a "very pronounced decline" in ozone concentrations of nearly 0.5 percent annually over a 20-year period, while satellite measurements showed a 2 to 3 percent drop during the last decade over equatorial regions. Such rates of decline far exceeded the model predictions." (Benedick, 1991:170, italics added). This tendency thus indicated that the scientific uncertainty that after all is associated with model predictions, in this case went in the direction of *under-estimations* of anticipated ozone depletion, rather than over-estimations.

#### **6.4.3. The political setting of ozone depletion**

The political setting of this process was to a large extent characterized by a two-dimensional conflict structure; The United States, Canada, the Nordic countries, Austria and Switzerland (who "joined forces" in the Toronto group in 1984) constituted one side, favouring strong

regulations, and the EC (primarily) constituted the other, opposing strong regulations.<sup>137</sup> At least before 1989, there was no major conflict between developed and developing countries. This implied that complicated and often ideological north-south issues were largely avoided. With this bilateral conflict structure, negotiations became a question of convincing the opposing parties to join the regime, and the major tool was that of science. After 1989 The United States took on a leadership role in the drive for CFC regulations. A major source to this position was her scientific capabilities. The science on ozone depletion was heavily influenced by US scientific institutions, also providing bargaining leverage. The institutionalized political setting with iterative decision-making provided the necessary framework for gradual persuasion. Thus, it seems correct to characterize the political problem structure as relatively benign, at least as compared to the related problem of climate change. Furthermore, the problem itself has features that serve to demonstrate this point.

First, production of CFCs was concentrated: In 1986 35% of global production was in the United States, 36% in Western Europe, 8% in the Soviet Union and Eastern Europe, 18% in Asia and the Pacific, and 3% in Latin America (Haas, 1992:197). At the time of the negotiations, moreover, CFCs were being produced by only 17 companies in 16 countries, and in this market DuPont was the world leader. DuPont held 50% of the US market, and over 25% of the global market, as well as being the only company that produced for the three major markets; North America, Europe and Japan (ibid.). As Haas points out: "Given the size of DuPont's market and the fact that the United States was the largest CFC-producing and CFC-consuming country, it is not surprising that the United States became the most powerful actor involved in ozone research and negotiations." (ibid.).

DuPont not only played an important role as the world's largest producer of CFCs; one of the first reactions to the OTP Report of March 1988 came from DuPont. DuPont announced that they would stop manufacturing all CFCs and halons by the end of the century, and accelerate research into substitutes. (Benedick, 1991). This development probably served as the catalyst to ICI's turn as well, the major producer in the U.K., which also "coincided" with the change in the position of the British government (see also Skjærseth, 1992). Thus, the impact of science should be seen in relation to this development in the chemical industry, and its implications for the structure of the CFC market. When the chemical industry accepted the risks to human health associated with CFCs and, even more important, anticipated international regulations and realized their potential "comparative advantage" in being in the forefront of the development of substitutes, substitutes were soon developed and the costs of a phase out for the industry significantly reduced, or turned into a potential profit. Maxwell

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137 The EC was split on this issue, with Denmark, the Netherlands and Belgium favouring regulations (FRG adopted this position in 1987). However, the Council had accepted a proposal from the Commission implying that all member states and the Commission ratify the Montreal Protocol *simultaneously* (Benedick, 1991).

and Weiner point out that "[l]ong-term interests were one of the primary reasons that DuPont sought, and ICI would ultimately accept, international regulation that helped create the market for substitution chemicals" (Maxwell and Weiner, 1993:28). As pointed out above, however, the development in the chemical industry in this respect was not independent of the institutional design of the process of knowledge accumulation. Industry experts took part in the process from the beginning, in CCOL and the technology panel; an involvement that implied a continuous update and part in the scientific progress on the issue area and probably an increased willingness to accept the findings and take the consequences of their implications.

#### **6.4.4. Public saliency**

Finally, the impact of public saliency should be taken into account when regarding this case. The US position on ozone was to a large extent formed by strong domestic pressures towards CFC regulation. From the beginning the public concern about the ozone depletion theory was much stronger in the United States than in Europe. As a response to the public reactions of the ozone depletion theories, Congress passed in 1977 a stratospheric ozone protection amendment to the U.S. Clean Air Act which authorized the administrator of the EPA to regulate "any substance... which in his judgement may be reasonably anticipated to affect the stratosphere, especially ozone in the stratosphere, if such effect may reasonably be anticipated to endanger public health or welfare." (cited in Benedick, 1991:23). The critical element in this legislation "... was the concept that was eventually to shape the U.S. position on international controls, the entire negotiation process, and finally the treaty itself - namely, that a governing authority is not obligated, before regulating a particular substance, to prove conclusively either that it modifies the stratosphere or that the consequences are dangerous to health and the environment. All that is required is a standard of reasonable expectation." (ibid.:24). In 1978, furthermore, the United States banned all nonessential use of CFCs as aerosol propellants. In Europe, on the other hand, national governments were under heavy pressure from domestic chemical and user industries, but without counter-pressures from the public or environmental groups, and adopted a more symbolic approach to the potential threat of ozone depletion. In 1980 the EC decided a 30% cutback in CFC aerosol use from 1976 levels, to take effect by end 1981. This was, however, "...a trivial target, since European sales of CFCs for aerosols had already declined by over 28 percent from their 1976 peak..." (ibid.:25). Thus, "to a significant extent, official disagreements between the United States and the European Community on ozone policy reflected important disparities in public perceptions of the danger. These differences influenced both the politics and the economics of the issue." (ibid.:27).

### **6.5. Conclusion**

The study of this process demonstrates that science did play a major role in the formation of the ozone regime, and that the institutional design of the science-politics dialogue seems to

have contributed to this effect. In this kind of case study, however, it is not possible to isolate effects; it is not possible to isolate and quantify the impact the institutional design as isolated from the impact of other factors. Institutional design will probably always serve as (only) one factor among many, and not even necessarily the most important, in the development of an explanatory framework. Still, in this case, it seems reasonable to argue that the institutional design served an important function in the development of the regime, especially with regard to the extent to which scientific findings were accepted by the political body as a valid and legitimate basis for decisions. The manner in which the science-policy dialogue was organized contributed to a general perception (in the political bodies) of the scientific process as being authoritative and their findings value-free.

On the other hand, we have also pointed to several other factors that seem important for explaining the outcome of the development of the ozone regime: the significant reduction in scientific uncertainty during the process and the character of the knowledge that was provided (the development of the "chlorine loading" methodology); the relatively benign political problem structure; and, finally, the public saliency of the issue, particularly in the United States. Moreover, while institutional arrangements seem to have been important for the extent to which the scientific findings were accepted as valid by decision-makers, these other factors seem to be *more* important for explaining the outcome in terms of the extent to which scientific findings were acted upon.

## VII The IPCC and the Global Climate Convention<sup>138</sup>

### 7.1. 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 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 major conference of this period, however, was the "WMO/UNEP/ICSU International Conference on the assessment of the role of carbon dioxide and other greenhouse gases in climate variations and associated impacts" arranged in Villach, Austria in 1985. This meeting was important in several ways (Lunde, 1991); it had a marked international profile; guaranteed through the WMO/UNEP/ICSU sponsorship, it was representative in terms of broad participation; scientists from most climate research communities representing 29 countries participated, and its objective was to evaluate and compare the major previous assessments made on climate change.

The Villach meeting represents a shift of emphasis; from a call for more research to a call for political response, and was thus instrumental in bringing the issue onto the political agenda. Due to an increasing scientific consensus on the climate issue, scientists started to call on politicians to act, and the Villach meeting was the first to propose a climate convention. The issue had also gained increased public saliency, due to "freak weather conditions" towards the end of the 1980s (Paterson, 1992). By 1988 the issue reached the political agenda: At the 1988 Toronto Conference more than 300 scientists and policy-makers from 48 countries, UN organizations, IGOs and NGOs participated, and agreed to an explicit policy recommendation.

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As a response to the Toronto meeting UNEP and WMO established the Intergovernmental Panel on Climate Change (IPCC).

The IPCC started its work in November 1988. The objective of the IPCC has been "...to provide the scientific, technical and analytical basis for informed and intelligent policy choices." (IPCC, Overview and Conclusions, draft 1990). 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 (IPCC Scientific Assessment, 1990). A working group was set up for each task. In 1990 the IPCC Report was presented to the Second World Climate Conference, where it was accepted as an adequate basis on which to start negotiations.

In December 1990 the Intergovernmental Negotiating Committee for a Framework Convention on Climate Change (INC/FCCC) was formally established in the UN General Assembly (UNGA) Resolution 45/212. Thus, the control of the process was moved from UNEP/WMO to the direct control of the UNGA. 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) to be held in Rio, June 1992. During this period, the IPCC continued its work, and presented a Supplementary Report in 1992, preceding the final negotiating session in INC.

The Climate Convention adopted in Rio is a framework convention, and does not hold binding commitments to specific greenhouse gas (GHG) emission control targets or time schedules for reductions of such emissions. The "ultimate objective" of the Convention is to achieve "...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..." (Article 2). The Convention does hold a binding commitment for developed country Parties to "Develop, periodically update, publish and make available to the Conference of the Parties, ..., *national inventories* of anthropogenic emissions by sources and removal by sinks of all greenhouse gases not controlled by the Montreal Protocol..." (Article 4a, italics added) and "Formulate, implement, publish and regularly update national and, where appropriate, regional *programmes* containing measures to mitigate climate change..." (Article 4b). However, it should be recognized that 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 could be 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.

## 7.2. Did scientific evidence count?

The Climate Convention's "failure" to specify substantial commitments to combat global warming could be regarded as indicating that the scientific evidence presented by the IPCC was not acted upon. In our view, however, such a conclusion would be premature due to the uncompleted nature of this process. As we have seen in the case of ozone, the Vienna Convention (in many respects quite similar to the Climate Convention) was only a first step, and the protocol and amendments agreed upon only a few years after the adoption of the Vienna Convention add up to a relatively strong regime. Therefore, the assessment of the extent to which scientific advice was acted upon in this case, will have to depend upon an evaluation of what constitutes a *reasonable expectation* as to what could be achieved at this stage: Has scientific advice been acted upon to the extent of the politically feasible?

What constitutes a "reasonable expectation", varies among the groups concerned with climate change. Ted Hanisch, as a participant of the negotiations, claims that "...we should not expect a convention negotiated in 15 months to be more than a modest step in the construction of an effective climate policy regime." (1992:64), while Jeremy Leggett and Paul Hohnen, representing the perspective of environmental NGOs (Greenpeace), state that "[i]t seems more important to underscore the vast scope of the opportunities lost in the 18-month process of negotiating the Convention - and conversely, to stress the importance of making up for lost time in the continuing sessions of the INC." (1992:76). The IPCC "calculated with confidence" that a stabilization of CO<sub>2</sub> concentrations at today's level, would require an immediate 60% reduction in emissions (Houghton et al., 1990). The Climate Convention as it stands today, is far from such a control target. However, the distribution of emission reductions (both between developed and developing countries, and especially *among* developed countries), and the level of "acceptable" global warming in the face of other urgent societal problems (such as poverty, internal strife, unemployment etc.) are complicated, political issues, touching the core of national interests. Underdal argues that "...much of the frustration expressed in the public debate about what has been accomplished through UNCED build upon a misconception of the political process." (1992:1). One such misconception is conceiving of the process as being essentially a decision-making process. Underdal points out that "...the initial stages of the UNCED process can be seen as much as an exercise in *search* and *learning* as an effort at negotiating a new regime.", and "...that a major challenge facing governments and other actors, particularly in the initial stages of problem-solving, is to develop a base of consensual knowledge that can serve as a (common) framework for decision-making. By implication, search and learning are integral parts of the problem-solving effort, and in logical sequence the two *precede* decision-making." (ibid:2). In this perspective, the IPCC has served a very

important function in the development of the Climate Convention, and their efforts "...could even be rated remarkably successful so far." (ibid:3).

Beyond doubt, the process leading up to the establishment of the IPCC, and the work of the IPCC itself, has served to bring the climate issue to the attention of policy-makers and thus to the international political agenda. Furthermore, the process has brought about a significant amount of learning, indicated only by the fact that policy-makers, as well as media and the general public know a lot more about the climate problem now than they did when the IPCC started its work in 1988. Moreover, the 1990 Assessment Report provided by the IPCC has become a point of reference to all groups concerned with the climate issue. This may indicate that the scientific findings presented by the IPCC to an increasing extent have been accepted as valid.

Finally, the outcome of both the IPCC and INC efforts (at this stage) should not be evaluated only on account of its (impact on) *collective* decisions. The process of bringing the issue to the international political agenda and to policy-makers' attention, has also brought about several self-imposed unilateral commitments. By the time formal negotiations started in 1991, all industrialized countries (OECD) except the United States had declared a target of CO<sub>2</sub> stabilization, either unilaterally or as part of a regional stabilization (for instance the EC and EFTA) by the year 2000 or 2005. By 1992, several OECD-countries had declared targets of a 20% reduction of CO<sub>2</sub> (see Paterson, 1992:182-3). Such emission reductions are mainly the result of implementing options that are beneficial in their own right - increased energy efficiency, installation of new technology etc. For some countries, implementations of options of this kind alone could serve to decrease CO<sub>2</sub> emissions significantly. It should be noted, however, that relatively few of the national targets adopted by 1992, are followed by implementation plans. Thus, the extent to which these commitments will in fact be implemented and their actual impact is still uncertain. Furthermore, they are often conditional; mainly on others taking like action, but also in other regards. Illustrative in this respect is that New Zealand's target of a 20% reduction in CO<sub>2</sub> emissions is "conditional on the target being cost-effective and providing a net benefit for New Zealand society." (Paterson, 1992:182-3). Finally, for some of the countries listed, the status of the "commitment" is that of a "policy goal". With all these reservations to the actual effect of such self-imposed commitments, the tendency is still important because it may signal anticipation by governments of future CO<sub>2</sub> (GHG) regulations, and that they to some extent are preparing for such regulations (practically, but also, and more important, politically in terms of negotiating positions).

Thus, recognizing the political, scientific and technical intricacies of this issue, and the immanent inertia of political processes of this magnitude we would conclude, that the IPCC process thus far has contributed to an increasing acceptance of the climate change problem, but that the IPCC findings have not yet resulted in concerted actions, in terms of regulatory commitments by the international community of states.

## 7.3. Institutional design

### 7.3.1. *The scientific-administrative/political distinction within the IPCC*

In contrast to what was the case in the ozone process, the political nature of the IPCC process has been evident since its beginning: The IPCC was established as an intergovernmental organization under UN auspices, by the Executive Council of WMO and the Governing Council of UNEP. The IPCC's mandate was also initially decided by these bodies. Being a UN body, the IPCC was submitted to the traditional UN procedures governing most UN bodies. Thus, the main characteristic feature of the IPCC is that it has a scientific mandate, while it is organized within a *political* institutional framework. This is reflected by a distinction *within* the IPCC itself, between the bodies largely serving "administrative/political" functions and the bodies constituting the "scientific/technical core".

Until 1992, the main bodies of the IPCC were the Plenary, the Bureau, three Working Groups (WGs) (some with ad hoc task force establishments on specific issues) and a Special Committee for Developing Countries (established early 1989). The scientific/administrative distinction seems to run through the WG level, between WG-plenary (as distinguished from IPCC Plenary) and task force establishments. The bodies constituting the scientific core were principally responsible for the provision of the knowledge base, while the administrative/political bodies (the IPCC Plenary in particular) were responsible for accepting and approving the outcome of the work taking place in the scientific core, as well as formulating policymaker summaries. The scientific-administrative/political distinction is "visualized" by the new procedures adopted at the IPCC's 9th plenary session (June 1993). At this session it was decided that the status of the reports published by the IPCC should be explicitly stated on its front page. Four categories were identified: supporting material (contributions), Reports accepted by WGs (assessment reports), Reports approved by WGs and accepted by the Panel (synthesis reports) and finally, Reports approved by the Panel (policymaker summaries). The categorization also signals the extent to which the reports have been submitted to negotiations, with "supporting material" at the "scientific" end of the pole (the "least" negotiated) and "reports approved by the Panel" at the "political" (the "most" negotiated). It should, however, be noted that there are significant variations also within the "scientific core" in terms of the extent to which the bodies in fact do perform science. These will be discussed in section 3.3. Still, the discussion in this chapter will be organized according to the scientific-administrative distinction, although an objective of pin-pointing the relationship between the levels implies some overlap.

### 7.3.2. *Administrative bodies*

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 representativeness of the groups of lead authors has been given more emphasis. Thus, the geographical balance of the groups has 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 Plenary and Working Group (plenary) sessions is in principle open; invitations are extended by the Chairman of the IPCC to Governments "and other bodies" (WMO, UNEP, scientific 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 were 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 representativeness 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." (1991:84). 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." (ibid.). Their work seems to have been relatively successful, as developing country participation was increased from 10 in early 1989, to 40 by August 1990 (ibid.).

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)." (IPCC/TF/3rd/Doc.2 (7.x.1992)). Thus, this issue is now integrated at all levels of IPCC activities, and the Special Committee on Developing Countries is dissolved. The 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 representativeness in all IPCC bodies and at all levels (implying the above mentioned involvement of the plenary in the election of lead authors).

IPCC activities are funded by voluntary donations from national governments, administered by the IPCC Trust Fund. The Trust Fund covers financial support to developing country participants, meeting expenditures (such as translation costs, printing, overhead etc.) and IPCC Secretariat functions. Research activities are mainly funded separately by national governments and the traditional funding mechanisms of scientific institutions.

### *7.3.3. The scientific core*

Until 1992 the IPCC served three main functions; scientific assessment, assessment of impacts, and formulation of response strategies, carried out by WGs 1, 2 and 3, respectively. The nature of these tasks implied that the results of the work carried out in WG 1 served as a condition for the work to be carried out in WGs 2 and 3. Thus, throughout the period from 1988 to 1990, WG 1 dominated the work of the IPCC as a whole. Furthermore, due to the complexities of the science on this issue, WG 1 was not able to provide the scientific basis needed in time for WGs 2 and, especially, WG 3 to fulfil their tasks in accordance with their mandate. A much criticized aspect of IPCC also refers to the coordination problems between the WGs: the three groups worked in parallel and simultaneously, while the nature of their functions indicates a necessity of a sequential approach. Sonja Boehmer-Christiansen has, for instance, found that, "[c]ommunications between the groups was poor to non-existent. Some scientists interviewed [in WG 1] were unaware of WG II and III" (Sonja Boehmer-Christiansen, 1993b:390, italics added).

The tasks of WGs 1, 2 and 3 had an increasingly political nature; in WG 1 the line between science and politics was more clear-cut than in WG 2 and, especially, in WG 3. This also had significant implications for the manner in which the WGs carried out their tasks, and the extent to which the political nature of the institutional framework of the Panel served to politicise their work accordingly. As WG 1 and 3 represent opposite extremes in this regard, we will concentrate our discussion to 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.

Research was generated through a series of workshops, and lead authors were appointed to write the respective assessments following each issue. The assessments were then reviewed by scientists who had not taken part in the preceding process. These two stages of the process seem to have been well shielded against political pressures (Lunde, 1991; Ivar Isaksen in personal communication). Due to the "scientific" nature of the task and the mechanisms applied to provide the assessments, there seems to have been little room for purely political considerations to gain any significant influence on the outcome: The level of scientific complexity in itself served to "exclude" non-experts from influencing the discussions (Isaksen in personal communication). The result of this process was then discussed in the WG as a whole (WG-plenary), where non-scientists and "governmentally appointed" experts also participated. At this stage there were some efforts at "filibuster tactics". However, texts were never changed *in substance* due to such efforts (Isaksen in personal communication). At the final stage, the Plenary discussions, the assessments as well as the summaries (WG report summaries and policymaker summaries) were to be accepted and approved (respectively) as the official IPCC view. At this stage "plain politics" dominated the discussions, and the "policymaker summaries" were submitted to paragraph-by-paragraph negotiations. Still, however, the *substance* of the assessments remained largely unchanged and was well reflected in the summaries (Lunde, 1991, and Isaksen in personal communication). There are examples of efforts by governmental representatives to change the substance of the policymaker summaries, where such efforts have been successfully met by the scientific representatives; the WG Chairman in particular (Lunde, 1991, New Scientist, Sept. 1991). Thus, the politicised *procedures* of the IPCC do not seem to have served to politicise the *substance* accordingly, where WG 1 is concerned.

The scientific nature of the task ascribed to WG 1 also had impact on recruitment and participation patterns in the WG. At task force and review levels, members were largely scientists recruited on the basis of scientific criteria and scholarly merits. In WG 1 individual scientists were invited to contribute in their own right either as contributors or as reviewers, thereby also counteracting the immanent potential for politicisation following from the recruitment principles adopted by IPCC Plenary. A very important aspect in this context, seems to have been the scientific networks developed in relation to the associated research on ozone depletion, which to some extent involved the same scientists (a point to which we will return in section 4). WG 1 could draw upon a pre-established scientific network in the development of the scientific assessments on climate change, and their work was "peer-reviewed" according to traditional scientific standards (Isaksen in personal communication).<sup>139</sup>

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139 Illustrative in this regard is that the science component (provided by WG 1) of the 1992 Supplementary Report involved 115 lead authors/contributors from 21 countries, and 341 reviewers from 60 countries and 19 non-governmental organizations (appendix B, Report of the Eighth IPCC Plenary Session).

In WG 3 the situation was somewhat different from that in WG 1. With the political nature of the *task* ascribed to this WG<sup>140</sup> 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 an increased *substantial* politicisation of their work as compared to the other WGs (Lunde, 1991, Boehmer-Christiansen, 1993a). The members were to a lesser extent scientists active in research, and they could to a lesser extent rely on existing networks and scientific review mechanisms. Boehmer-Christiansen claims that "...the third WG became a forum mainly for government people and lobbyists..." (p.19), and "...the main forum for pre-negotiations and conflicts..." (p.22). In this context it should also be noted that the WG leadership was held by The United States, one of the major opponents to a regulatory climate regime. 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 that resulted from their discussions. In some respects, the establishment of WG 3 may seem premature: In order to fulfil the task of formulating response strategies, a *pre-established* scientific knowledge base was required. At the time, however, the required knowledge base was about to be developed. Thus, the basis and condition for fulfilling the task was lacking. Furthermore, there did not exist a political forum for discussions concerning interpretations of the science in terms of response strategies at the time when the WGs were set up (negotiations on a climate convention started in 1991). Thus, there did not exist a forum to which the political aspects of the issues to be dealt with in WG 3 could be channelled. These two factors probably served to reinforce the tendency of politicisation in the WG.

The tasks ascribed to WG 3 are also included in the restructured IPCC, although in a somewhat modified formulation.<sup>141</sup> In its restructured form (the old) WGs 2 and 3 are merged. The task of the new WG 3 is mainly to provide "technical assessments of the socio-economics of impacts, adaptation and mitigation of climate change over both the short and the long term and the regional and global levels" (8th Plenary session report, item 6.10). Thus, the new WG 2 is now responsible for assessing information on impacts of climate change and response options. The wording of the task is therefore modified, from "*formulations* of response strategies" (the old WG 3) to "assess *information* of response options". It is furthermore explicitly pointed out, with regard to all (new) WGs that "a peer-review process should be incorporated in the preparation of the reports of the Working Group." Thus, it has now been attempted to place the issue more firmly within a scientific framework. Furthermore, the scientific knowledge base for fulfilling this task, based on *scientific* considerations, is to a larger extent present.

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140 "Formulation of response strategies" could be argued to be, more or less by definition, a *political* question.

141 Initially it was suggested to reduce the IPCC to two technical working groups, leaving the complicated political issues with ad hoc task forces, thus signalling a desire to return to a less politicised environment (Boehmer-Christiansen, 1993, session reports from IPCC Task Force on IPCC structure).

#### 7.3.4. How autonomous is the IPCC ?

As the preceding discussion illustrates, IPCC's "score" on the autonomy-dimension can not be evaluated solely by studying its relationship to the regulatory body (INC). Scientific autonomy, in this case, is also a function of the relationship between the bodies of the IPCC itself.

Both formal and operational autonomy of the IPCC in relation to the INC is high. The two bodies are organized under different UN bodies (see below), and the INC is in no position to place either restrictions or prescriptions on the manner in which IPCC chooses to organize its work. The INC may specify which questions they require the IPCC to address, but the IPCC is free to choose whether and when to respond to the inquiry.

When we turn to the relationship of the bodies within the IPCC, however, the autonomy of the "scientific core" seems to have varied a great deal, both between and across decision-making levels. As the discussion shows, the main distinction between the bodies runs between the more politicised WG and IPCC Plenaries and the "scientific core" constituted by task forces and subgroups. Although the "authority" of the IPCC lies in the IPCC Plenary, where the intergovernmental status of the institution has significant impact on the composition of participants, this does not seem to have severely restricted the operational autonomy of the "scientific core". They could still decide on aspects such as the division of labour within the group, lead authors on the respective areas, contributors and scientific reviewers. The extent to which this potential operational autonomy was realized seems, however, to have been dependent upon, *inter alia*, the scientific qualifications of the WG chairman. As we have seen, (until 1992) the chairman of WG 3 also served as head delegate to the US delegation in INC. This combination of functions, may have served to restrict the operational autonomy of the "scientific core" of this group. The most important explanation of the observed variation in autonomy *across* decision-making levels seems, however, to be found in the nature of the *task* ascribed to the WG. The task ascribed to WG 3 lies very close to the political domain and it is not encompassed in any clearly identifiable scientific discipline or group of experts with pre-established scientific networks. Due to these circumstances the production of knowledge itself was moved from the bodies constituting the "scientific core" to the more politicised decision-making levels of the IPCC, and the group's autonomy was reduced accordingly.

#### 7.3.5. Involvement

One of the main purposes of the IPCC establishment was to assist in the provision of the scientific basis for political deliberations on regulatory measures to mitigate human-induced climate change. Until 1992, one of the WGs of the Panel, WG 3, was given the mandate to "formulate response strategies", also indicating the advisory function ascribed to the Panel. Thus, one of the main purposes of the IPCC is to support a political body with scientific



information as well as advice, indicating already at the outset some level of involvement in the political process.

Both the production of knowledge and the formulation of advice took place within the framework of the IPCC, but the functions were ascribed to different WGs. Thus, the tasks were separated and performed by different actors, but the organizational distance between the bodies was relatively "short". However, due to the circumstances discussed above, the *status* of the outcome of WG 3 proceedings is somewhat problematic. With the scientific uncertainties characterizing the issue area, the political potency of the problem, and lacking pre-existing research networks, it proved difficult to maintain the control of WG 3 in the hands of its environmental experts. WG 3 therefore, may be regarded as gradually turning into a political forum for pre-negotiations, and the texts drafted by this group held general, "diplomatic" and non-controversial formulations bearing clear signs of being negotiated. Thus, the "advice" was as much based on purely political evaluations as scientific, and does not, in our view, qualify as "explicit *scientific* advice". Moreover, the most widely cited "advice" of the 1990 IPCC Report was not formulated by WG 3, but by WG 1.<sup>142</sup> This may indicate that, to the extent that advice actually was formulated (and this is a question of perceptions), it was rather formulated by *implications* of scientific knowledge (see also the ozone case).

Furthermore, it has been pointed out that the scientists taking part in this process have learned "...how to talk science to politicians..." while at the same time "...not to play at being politicians." (Comment, New Scientist, 8 September, 1990). This may indicate that the formulations of the Scientific Assessment Report were chosen with a conscious notion of how to catch policy-makers' attention, and could be regarded as some sort of implicit formulation of advice. This was clearly the case in the 1992 Supplementary Report, where WG 1 devoted considerable time to discussing the question of how their findings on the negative warming potential of SO<sub>2</sub> should be presented due to its possible policy implications (Isaksen in personal communication). The actual functional differentiation between research and advice is therefore difficult to assess; the functions were separated organizationally, but in reality they seem to overlap.

The IPCC reported to UNEP and WMO, while negotiations, by implication of UN Resolution 45/212, were under the control of the UN General Assembly. In the resolution it is taken note of "...the important work accomplished by the [IPCC]...", and the ad hoc secretariat of the INC is requested "...to co-operate closely with the [IPCC] to ensure that the Panel can respond to the needs and requests for objective scientific and technical advice made during the

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142. We here refer to WG 1's statement - "calculated with confidence" - that a stabilization of CO<sub>2</sub> concentrations at today's levels would require an immediate 60% reduction in emissions. It should be noted, however, that the statement does not include a scientific evaluation of the *necessity* of stabilizing *concentrations* (not emissions), and should, therefore, probably rather be regarded as an illustration, not a recommendation.

negotiation process...". The "dialogue" between the two bodies seems primarily to have taken place by "representation": The Chairman of the IPCC was invited to speak at INC sessions, and the Chairman of the INC was invited, in an *ex-officio* capacity, to attend the IPCC (Plenary), the Bureau and the Working Group sessions. Overlapping participation seems, furthermore, to have played a very important role in strengthening the relationship between the bodies; several (if not most) of the participants of IPCC Plenary sessions were also delegates at INC sessions. Finally, several participants serving as delegates in one body also served some formal function in the other body: Before 1992, the head of the US delegation to INC also served as Chairman of WG 3; and after 1992 the Chairman of the INC also serves as the head of the Argentinean delegation to IPCC.

Thus, formal links between the scientific and regulatory bodies were established, as well as both formal and informal channels for dialogue and communication. On the whole therefore, these variables indicate high involvement in the political process at all decision-making levels of the IPCC.

#### **7.4. Impact of institutional arrangements and other explanatory factors**

In this case, one important determinant to the impact of scientific evidence as decision premisses, seems to have been the extent to which the WGs experienced *substantial* politicisation. The first part of this section, therefore, will be devoted to a discussion of how much of the observed variations in politicisation within the scientific core of the IPCC can be explained by institutional arrangements. We will then go on to discuss other explanatory factors to this variation, and finally discuss the general impact of institutional arrangements, as well as other factors, on the extent to which scientific evidence was acted upon by decision-makers in this case.

##### **7.4.1. Explaining variations in politicisation**

In the preceding discussion we have argued that the political institutional framework of the IPCC, the intergovernmental status of the body in particular, implied politicisation in at least two respects: First, the institutional framework implied a procedural politicisation at higher decision-making levels; i.e. rules of procedure, recruitment principles etc. resemble those of political bodies. Second, the institutional framework served to reinforce, or rather failed to prevent, *substantial* politicisation of the science on certain issue areas. What *could* be our main conclusion therefore, seems to fit quite well with a statement made by one scientist engaged in the process, who maintained that the outcome of the IPCC effort (with regard to WG 1 only) was rather achieved *in spite of*, than *because of* the institutional, design (Ivar S. A. Isaksen in personal communication). This impression is supported in that variations across decision-making levels (within the scientific core) in terms of autonomy (also reflecting degree

of politicisation) largely seems to be explained by factors other than institutional design. We will, however, examine the extent to which institutional aspects can explain why a seemingly optimal balance between autonomy and involvement was achieved in WG 1, when it was not achieved in WG 3.

Some may have been puzzled by why we have placed the "production unit" of WG 3 so firmly within the "scientific" core of the IPCC, only, in the next sentence, to argue that it was not science but politics that took place in this body. The reason for this is that the *formal* mandate of the IPCC *as a whole* is of a scientific character, and that it was this feature that served as our point of departure. This point being made, we will in this section argue that some of the explanation of the "success" of WG 1 (in terms of providing a scientifically legitimate assessment of climate change) may be found in the political nature and function of WG 3. The founders of the IPCC were probably well aware that formulations of response strategies, at least in a politically potent area such as this, were more or less "doomed" to turn into political deliberations and negotiations. This institutional device may, however, have served to provide a political forum, within the IPCC context, to which political aspects of *all* issues under the IPCC mandate could be channelled. In this manner, the WG 3 establishment may have served to "protect" WG 1 (and 2) against undue political control and influence in that decision-makers' interests in "controlling" the outcome were channelled towards WG 3, and away from the other WGs. This function was particularly important in the first phase of the IPCC process, before negotiations in INC had started. At the time, no political forum existed at the global level, where "greenhouse politics" could be discussed. With the political institutional setting of the IPCC, the WG 3 establishment may have been a *condition* for preventing politics from penetrating and probably obstructing the whole process. There was a need for a political forum, but this could not be formally established, because the knowledge base was lacking. Thus, the political discussions were "pooled" in WG 3.

This aspect, therefore, changes our impression both of the impact of institutional design and the functions of WG 3. First, the institutional design may, in this respect, have served as an important determinant of the outcome of the first phase of the IPCC effort. Second, the "failure" of WG 3 contributed to the "success" of WG 1. Institutional design does therefore provide some explanation for the degree of politicisation among the WGs.

An important additional factor in explaining why WG 1 did not experience significant substantial politicisation, however, seems to have been the performance of leadership functions. First, the scientific leadership of WG 1, in close cooperation with the leadership of the IPCC as a whole (the Chairman in particular), managed to maintain control of important "scientific" decisions: The Plenary could for instance only suggest candidates for lead authors, contributors and reviewers, while the actual decision was made by the "scientific leadership" of the WG. In the first phase of the IPCC process, they made these decisions quite freely. With the restructuring, the Plenary placed more emphasis on the geo-political balance in the

group of lead authors implying some restrictions on scientific autonomy in this regard, but the decision remained in the hands of the scientific leadership (personal communication with Bob Watson). By keeping these decisions, and their like, outside the control of the Plenary one has first most probably prevented endless discussions on who should hold the various positions in the WG. More importantly, however, one has also prevented political nominations to positions *in the scientific core* of the WG, thereby also increasing the operational autonomy of the WG and the scientific legitimacy of the outcome. Second, the scientific leadership of WG 1 used the scientific linkage between ozone and "greenhouse" science instrumentally to "exclude" politically appointed actors from important scientific fora. The science on climate change and ozone depletion is intimately connected, and members of the scientific core of WG 1 were also participating actively in the scientific bodies of the ozone process (the science panel in particular). The scientific linkage implied that conclusions drawn in the ozone-context also had scientific implications for the "range" of possible conclusions in the climate change-context. The scientists participating in both processes became increasingly aware of this linkage, and were conscious of how ozone science conditioned greenhouse science. Thus, the development of the scientific knowledge base on climate change also took place outside the IPCC framework, in a body where "political" actors did not have access.<sup>143</sup>

#### 7.4.2. *Explaining scientific impact*

To what extent were institutional arrangements decisive for the extent to which scientific evidence was acted upon by decision-makers? As emphasized in the other case studies in this volume, this question is a complicated one. An additional complication is added in this case, moreover, in that it is not yet evident whether scientific knowledge is acted upon at all (see section 2 in this chapter).

At a general level it seems in this case as if an important condition for scientific impact was not only to achieve an optimal balance between autonomy and involvement, but to achieve an even more paradoxical optimal balance between (political) control (involvement in the "opposite direction") and (scientific) autonomy. This "need" also seems to be the origin to the distinctive characteristics of this manner of organizing a scientific process. Throughout this chapter we have emphasized the "tension" between IPCC's scientific mandate and its political institutional framework. The institutional framework implied a more explicit *integration* between science and politics than has been evident in earlier cases. It is not, as the studies in this volume show, unusual to establish scientific (advisory) panels in relation to international

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143 There is a marked overlap in the "scientific leadership" of the two processes. As argued in the preceding chapter, scientists managed to "capture" the scientific process on ozone depletion, implying that the process, at least for a relatively long period of time, was largely "science driven". Thus, several of the "scientific leaders" in the IPCC came into the process with some experience in handling processes of this kind. It seems reasonable to assume that the scientific leaders of the IPCC also had developed the ability and skills to retain some "control" over the process.

environmental and resource cooperation. It is, however, unusual to integrate the whole process of developing knowledge in a relatively new field of research, in the political process of developing solutions to a common problem.

The climate change issue, as a global commons problem, touches upon vital national interests of all countries in the international society. As Ted Hanisch points out, the name of the game could have been "negotiating a Convention for New Energy Systems for the World" (1992:63). Furthermore, the issue is scientifically complex and associated with profound scientific uncertainty. The combination of these features seems to have implied a desire by decision-makers to ensure scientific objectivity, and this was, paradoxical as it may seem, achieved by submitting the scientific process to political "control" through rules and procedures. The intergovernmental status of the IPCC seems to have been the key factor in this regard, because it implied a requirement of a geo-political balance, preferably at all levels, but at least in the main decision-making bodies of the institution. This design could have been devastating for the science on global warming, because it left the process particularly vulnerable to undue political influence. When this (as yet) has not been the case (with regard to WG 1), the WG 3 establishment seems to provide an important explanatory factor. Thus, to the extent that scientific evidence has been acted upon in this case, therefore, it seems as if the institutional setting of the scientific body does provide some explanation.

Other important explanatory factors are, of course, also the political setting and the public saliency of the issue. As emphasized above, the political setting of the issue is extremely complicated and conflictual, not least because the "traditional" 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 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<sub>2</sub> emission reductions may offer, are not at all shared equally among the industrialized countries. As Boehmer-Christiansen points out, "any political elite committed to economic growth with the help of cheaper energy, (USA, UK, developing countries) had strong reasons to doubt the need for rapid action in reducing emissions at high cost. The "activist" countries, on the other hand, had energy policy interests which converged immediately with emission reduction. They either wanted to strengthen their nuclear interests or research ambitions (Germany, EC) or replace domestic coal in the national fuel mix (...UK and now also USA), or maintain their export earnings from gas or nuclear electricity (Norway,

France)" (Boehmer-Christiansen, 1993b:373). Major actors, therefore, have differing incentives to use both scientific uncertainties and scientific findings as tools for energy policy.

The public saliency of the global warming issue was probably quite high when negotiations in INC started. During the last part of the 80s extreme weather conditions in various parts of the world aroused public awareness and speculations on whether we already were experiencing a rapid increase in global mean temperature. Furthermore, the issue of climate change was probably linked, in people's minds, with the threat of ozone depletion, which did arouse public concern. One has not, however, despite extreme weather conditions, experienced any "crisis" in the global warming issue like for instance the discovery of the Antarctic ozone hole. Moreover, due to the complexities of the science on this issue, it has been easy for politicians to "pay lip-service" to the problem instead of implementing effective policies towards solving it, and thereby "calm down" the public. The CO<sub>2</sub> reduction and stabilisation plans of OECD countries mentioned in section 2 of this chapter have probably served this function. In terms of developing agreement on the Climate Convention, which does not hold binding commitments, public pressure may therefore have had some impact.

## VIII Some Tentative Conclusions

### 8.1. Science as a source of inputs

To repeat, our dependent variable is the extent to which conclusions from scientific research are adopted (undistorted) as (consensual) premisses for policy decisions. We conceive of the "adoption" in terms of a cumulative scale with three levels. At the first and lowest level decision-makers recognize the relevance of knowledge produced through scientific research and look to the scientific community for information. At the second level they also accept as valid the substantive conclusions reached by general consensus within the scientific community. At the third level, decision-makers also respond "positively" to more explicit advice from scientists; not only are descriptive conclusions accepted as valid, their normative "implications" are also "acted upon".

The general impression, based on the limited set of cases that we have studied, can be summarized as follows:

1. Science is generally recognized as the principal supplier of "advanced" knowledge. A first observation is that in all of our cases decision-makers turn to science for problem identification and diagnosis, in some cases also for explicit policy advice. In all of our cases it appears that scientific knowledge has been generally accepted as an important, in many cases even a necessary, basis for making informed or rational decisions. One indication is the fact that some kind of scientific body or bodies have been established as more or less integral parts of the decision-making system in all these regimes.

Moreover, the "demand" for inputs from the scientific community seems to have increased somewhat over time. At the very least, we find a the tendency towards increasing formalization of links between decision-making bodies and the scientific community. However, it also seems that the demand for inputs from science depends on, inter alia, the kind of regulatory decisions to be made, and on the "exclusiveness" and conclusiveness of the knowledge produced by scientific research. The general pattern seems to be that the more specific the regulatory decision to be made (compare, e.g., catch limits to general framework agreements), and the more advanced the methods of measurement required to determine the nature and magnitude of the problem (compare ozone depletion to oil spills), the more decision-makers will turn to science for problem identification and diagnosis, perhaps also for explicit policy advice.

2. Governments rarely dispute what the scientific community considers "consensual knowledge". Moving to level 2 of our scale, we may first of all observe that universal and

perfect agreement on all aspects of the description and diagnosis of environmental problems is rarely, if ever, achieved within the scientific community (nor is it pursued). However, in most of the cases studied here there seems today to be broad agreement within the scientific community on the essential description of the problem and also on the causal mechanisms that are or may be operating. Admittedly, there is often considerable uncertainty, implying that large error margins may pertain to any estimate of ecological impact or "damage costs". Moreover, in most cases, the road towards a fair amount of consensus has been long and winding. In the early stages of international negotiations decision-makers have, in most cases, had considerable leeway to "pick and choose" among diverging conclusions and estimates. The evidence that we have examined here nonetheless seems to suggest that if and when broad consensus on the description and diagnosis of a problem emerges within the scientific community itself, most governments are reluctant to dispute openly what a clear majority of competent scientists consider "text-book wisdom". However, in most cases that we have studied there was at least one government that "held up" the process by pointing to the "need" for further study and more conclusive evidence. Scientific uncertainty seems to interact with political conflict; the more intense the conflict, the less ambiguity will be "tolerated" in the science base. At the same time, political conflict tends to "contaminate" research, so that the more intense the conflict the less likely that a "consensual" knowledge base will be established.

3. Faced with "general consensus" on the description and diagnosis of a (severe) environmental problem, governments do in fact most often respond, at least by institutional reform. It is, of course, quite possible to accept the description and diagnosis of a problem as valid and still conclude that the costs of problem-solving efforts exceed expected benefits, or that what ideally "should" be done is not politically feasible (for the time being). Moreover, governments will often "act upon" the inputs they receive from the scientific community without taking what the scientists concerned would consider "adequate" measures, fully recognizing the "policy implications" of their findings. Yet, in all of our cases, except the very recent climate change negotiations, some substantive action has been taken in response to broad consensus among competent scientists that a real environmental problem exists or is developing and is caused, at least in part, by human activities. Of the cases examined here, the stratospheric ozone regime stands out as the case where policy responses have been most vigorous. More "typical" seem to be the regional regimes on acid rain and the North Sea regime. In the whaling regime it seems that whenever the conclusions presented by the committee of scientists has been generally accepted, some regulatory action has usually been taken, although in many cases "insufficient" to protect stocks from serious depletion. The less conclusive the scientific evidence available, the more likely it seems that governments will respond by institutional reform rather than substantive policy measures. UNCED is a case in point; the conference produced significant decisions on institutions and procedures for future deliberations, but little in terms of commitments to specific substantive actions.



Consensual knowledge about the nature and ramifications of a problem is clearly not a sufficient condition for policy measures to be taken. It does not seem to be a necessary condition, either. We do have instances where policy moves "beyond" what the scientific community would recommend. In our sample, the undifferentiated ban on commercial whaling is the most obvious case in point. There can be no doubt, however, that consensual knowledge contributes significantly towards facilitating international negotiations, and also tends to enhance the "effectiveness" of the policy response.

## 8.2. Institutions matter, but politics decides

Our basic hypothesis was that the extent to which inputs from research are adopted as premisses for policy decisions depends on, inter alia, a set of institutional factors determining the autonomy and involvement of the scientific community. We suggested that high autonomy would be important to establish and preserve confidence in scientists as impartial experts, but also that a moderate-to-high level of involvement in the negotiations themselves would be important to get their findings and conclusions across to decision-makers, and to enhance the sensitivity of the scientists involved to the questions and concerns of the latter. We did not claim, however, that institutional arrangements would be the most critical determinants. Rather, our reasons for focusing on this particular set of variables were (1) that these are instruments that can, at least in principle, be deliberately manipulated, and (2) that we want to know more about how institutional arrangements affect the use of findings from science as inputs in international environmental regimes.

Our analysis seems to confirm that autonomy and involvement are indeed important dimensions, but also that other factors, including our control variables - notably problem malignancy and the conclusiveness of the present state of knowledge - generally account for considerably more of the variance actually observed in the adoption of inputs from science as decision premisses. More specifically, the explanatory power of our independent variables seems to be greater at levels 1 and 2 than at level 3. In other words, institutional arrangements seem to affect the extent to which decision-makers turn to the scientific community for information and advice, and also the extent to which findings from scientific research are accepted as valid or tenable by decision-makers. However, when it comes to deciding whether to act in response to these findings, and what to do, institutional arrangements and procedures for the science-politics dialogue seem to make little difference.

Autonomy and involvement are clearly not sufficient conditions for the "adoption" of inputs from science, at any level. Nor do they seem to be necessary conditions. It seems that confidence in the science base may be drawn from two different sources. One is a combination of competence, independence, and scholarly integrity (enhanced by "autonomy"). The other may be labelled representation and intersubjectivity, the underlying assumption being that the

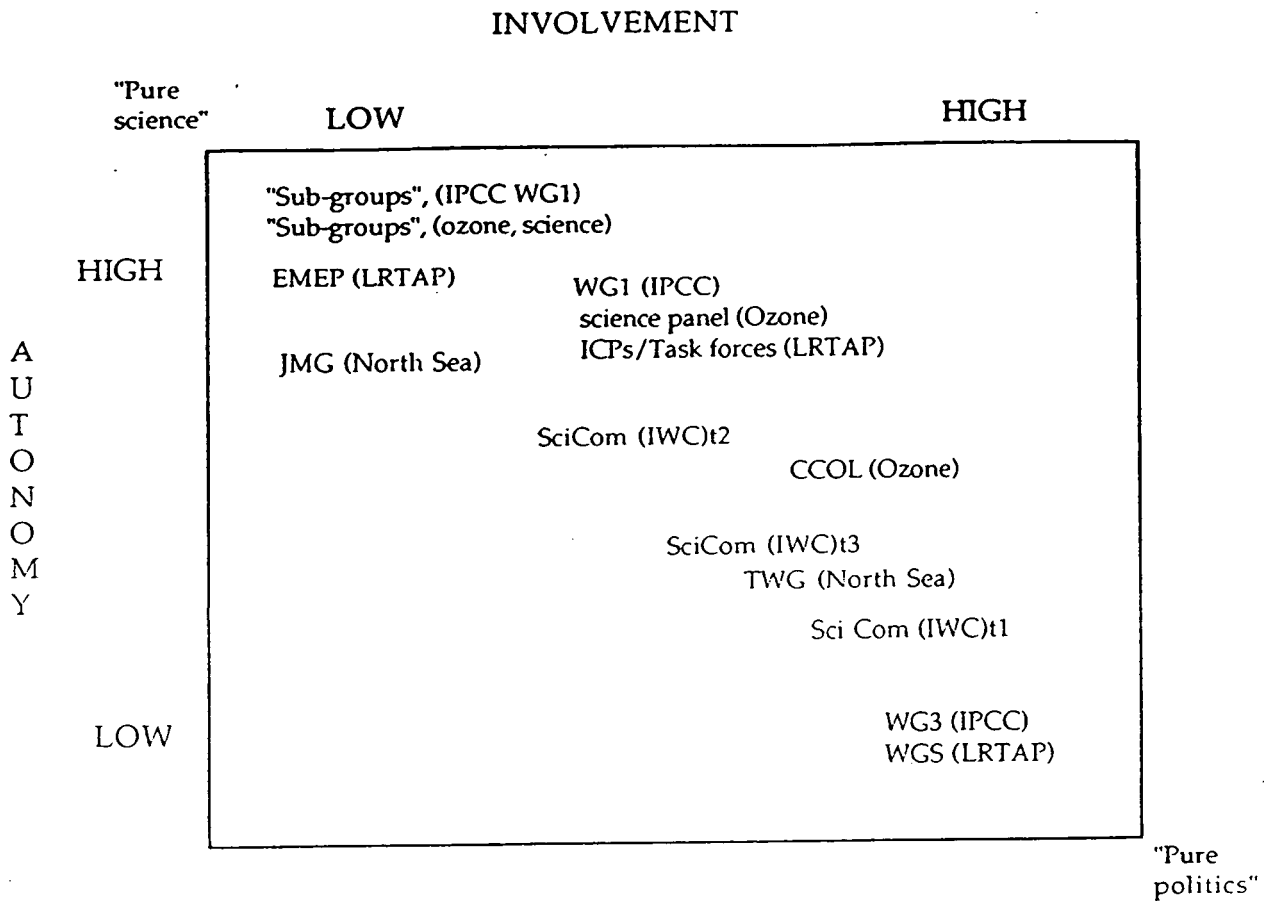
best test of reliability is to subject propositions to the adversarial scrutiny of competent people representing conflicting interests. Independence and integrity may be more important for ensuring quality of research than for enhancing trust and legitimacy. Moreover, looking at our cases, it seems that top scores on the autonomy dimension are very rare, at least if scores are based on the formal indicators that we listed in part I. This may serve to remind us that the notion of science as an independent enterprise is very much a liberal Western ideal which is by no means universally shared.

The analysis also indicates that the relationship between our independent and dependent variables is interactive rather than uni-directional. "Demand" for inputs from science provides incentives for coordinating scientific activities and establishes some mechanism for translating the multitude of hypotheses and findings into some kind of authoritative judgement or "advice". Demand for "policy relevant" outputs thus affects the organization of the scientific community, and tends to lead to a redistribution as well as to an overall increase in the amount of resources available. Conversely, the "supply" (and also clever "marketing") of more or less authoritative conclusions seems to stimulate "demand". The more and better answers scientists can provide, the more their inputs will be sought.

One crucial question that we raised in the introduction is whether, or to what extent, autonomy and involvement are compatible; to what extent shall we have to sacrifice one in order to achieve the other? The analysis of these few cases clearly indicates that the two are hard to combine. In figure 1 (next page) we have provided a very tentative summary of scores for our cases, differentiating phases and bodies, where appropriate. We should like to emphasize that the scores are based on informed judgement rather than precise measurement, and that the scales are used for purposes of ordinal level rankings only. Yet, it is interesting to note that the distribution seems to indicate that autonomy and involvement tend to be inversely related: the greater the autonomy of a scientific body, the less involved it tends to be in the science-politics dialogue.

The analysis also suggests, however, that there may be a way out of the dilemma. The "solution" is functional differentiation. In several of our cases roles are differentiated so that those scientists who are most involved in the dialogue with decision-makers serve as coordinators of research and as mediating agents between those who do the actual research and the decision-makers. In most of the regimes studied here there seems to be a "core" of scientific activity that is fairly well secluded from the political process, although top scores on some of the formal indicators of autonomy (such as independent funding and recruitment on scholarly merits only) seem to be very rare.

Figure 1: A summary of tentative scores.



Examples of such bodies are WG 1 within the IPCC and the JMG in the North Sea Regime. The closer we get to political issues and the decision-making process, however, the lower, by and large seem the scores on our autonomy indicators. The contrast between WG1 and WG3 of the IPCC is, perhaps, the most striking example, but a similar pattern is quite evident also within the North Sea regime (compare the JMG to the TWG). The picture is not entirely clear, though; the scientific committee of the IWC also seems, at least in some periods, to have enjoyed a fairly high level of operational autonomy despite its involvement as supplier of explicit policy advice to the decision-making body.

This pattern of differentiation seems to support Miles' "buffer hypothesis". To recall, Miles suggested that "...indirect rather than direct links to management decisions will facilitate the emergence of scientific consensus" (1989:49). Furthermore, he maintained that "...from the scientific perspective, it is preferable that the research being conducted be of sufficient concern to warrant continued government support, but that decision processes be deliberately designed so as to provide a buffer between research results and their utilization for regulation and - especially - for the distribution of benefits and/or apportionment of costs" (Miles, 1989:50). Our sample of cases points to some institutional devices that can be used to provide such a "buffer". WG 3 in the IPCC case is the most obvious example of a body serving as a "lightning rod", protecting the scientific "core" from (strong) politicization.

The analysis also help us interpret what might at first glance appear to be contradictory findings. In the case of the ozon regime, broad participation in the work of the scientific bodies seems to have been an important vehicle for establishing a base of consensual knowledge from which negotiations could proceed. In the case of whaling, however, the influx of new participants seems to have strained the process substantially. One important clue to explaining why the two cases came out so differently seems to be *the amount of control exercised by "core"*. Open access without "hegemony" by the "core" may put scientific bodies under severe stress, but broad participation combined with strong "hegemony" by the "core" can be a most effective means of "driving home the message". This in turn suggests that coherent and effective leadership within the scientific community itself may be as important as institutional arrangements.

## ABBREVIATIONS

ASMO:	Environmental Assessment and Monitoring Committee
CCOL:	Coordinating Committee on the Ozone Layer
CFCs:	Chlorofluorocarbons
CMA:	Chemical Manufacturing Association
CoP:	Conference of the Parties (to the Climate Convention)
CO <sub>2</sub> :	Carbon dioxide
EB:	Executive Body
EC:	European Community
ECE:	UN Economic Commission for Europe
EMEP:	Cooperative Program for Monitoring and Evaluation of Long-range Transmissions of Air Pollutants in Europe
EPA:	Environmental Protection Agency
GEAP:	Group of Economic Experts on Air Pollution
GHG:	Greenhouse gas
HCFCs:	Hydrochlorofluorocarbons
ICES:	International Council for the Exploration of the Sea
ICPs:	International Cooperative Programs
ICRW:	International Convention for the Regulation of Whaling
ICSU:	International Council of Scientific Unions
IGO:	Intergovernmental Organization
INC(FCCC):	Intergovernmental Negotiating Committee for a Framework Convention on Climate Change
IPCC:	Intergovernmental Panel on Climate Change
JMG:	Joint Monitoring Group
LRTAP:	The Convention on Long-range Transboundary Air Pollution
NAS:	National Academy of Science
NASA:	National Aeronautics and Space Administration
NGO:	Nongovernmental Organization
NOAA:	National Oceanic and Atmospheric Administration
N <sub>ox</sub> :	Nitrogen oxide
N <sub>SC</sub> s:	North Sea Ministerial Conferences
NSTF:	North Sea Task Force
ODP:	Ozone Depleting Potential
OECD:	Organization for Economic Cooperation and Development
OSCON:	The Oslo Convention on Marine Dumping
OTP:	Ozone Trends Panel
PARCON:	The Paris Convention for the Prevention of Marine Pollution from Landbased Sources
PRAM:	Programmes and Measures Committee
QSRs:	North Sea Quality Status Reports
SACSA:	Standing Advisory Committee for Scientific Advice (OSCON)
SO <sub>2</sub> :	Sulphur dioxide
TAC:	Total allowable catch
TFs:	Task Forces
TWG:	Technical Working Group (PARCON)
UNCED:	United Nations Conference on Environment and Development
UNEP:	United Nations Environment Programme

UNGA: United Nations General Assembly  
VOCs: Volatile organic compounds  
WCP: World Climate Programme  
WG: Working Group  
WGAS: Working Group on Abatement Strategies  
WGE: Working Group on Effects  
WGT: Working Group on Technology  
WGV: Working Group on VOCs  
WMO: World Meteorological Organization  
WPAP: Working Party on Air Pollution Problems

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