Mending the Ozone Layer: The Role of Transnational Policy Networks

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Abstract

Successful international cooperation is a puzzling problem for social scientists. The ozone layer has been subject to both international treaties and domestic legislation. It is one of the foremost success stories in international relations, yet insufficiently understood. In this paper I argue that existing approaches – including the sophisticated and highly acclaimed epistemic community approach – do not take the underlying theoretical problems seriously enough. Departing from the epistemic community approach, I propose a framework for a network analysis combining interests, knowledge and power into a coherent model, which is derived from this case but can apply to similar cases sharing similar characteristics. It is argued that one of two rivaling policy networks gained hegemony over the other, mainly by winning over allies from the competing network. Ultimately this contributed to the competing network's breakdown.

Zusammenfassung

Erfolgreiche internationale Kooperation ist für Sozialwissenschaftler oft ein Rätsel. Eines der erfolgreichsten Beispiele internationaler Kooperation sind die Maßnahmen zum Schutz der Ozonschicht, die auf nationaler und internationaler Ebene ergriffen wurden. Dieser Erfolg wird bislang unzureichend erklärt: vorliegende Ansätze stellen sich den theoretischen Schwierigkeiten nicht in adäquater Weise. Dies gilt auch für den Epistemic-community-Ansatz, der in letzter Zeit viel Aufmerksamkeit auf sich gezogen hat. In kritischer Auseinandersetzung mit diesem wird ein Netzwerkansatz entwickelt, in dem Macht, Interessen und Ideen als Erklärungsmomente zusammengeführt werden. Der zentrale Mechanismus, der zu internationaler Kooperation führt, besteht darin, daß es einem von zwei konkurrierenden Politiknetzwerken gelingt, Akteure aus dem gegnerischen Netzwerk herauszubrechen, wodurch dieses zerfällt. Dieser Ansatz sollte auf andere Fälle anwendbar sein, die ähnliche Eigenschaften wie der vorliegende aufweisen.

Contents

1 A Threefold Puzzle

The main purpose of this paper is to answer the question of why international regulations to protect the ozone layer could come about.[1] International regulations for the protection of the ozone layer seem to be quite successful.[2] They are even deemed to be a blueprint for other global environmental problems like climate change. One of the architects of the Montreal Protocol, former UNEP director Mostafa Tolba, is one of the most prominent persons who espouse such "policy learning" when he says: "The mechanisms we design for the protocol will – very likely – become the blueprint for the institutional apparatus designed to control greenhouse gases and adaptation to climate change" (quoted in Benedick 1991: 7).

Three characteristics of the case seem to make its success unlikely. First, the successful representation of diffuse interests vis-à-vis an industry which has to face costs (Wilson 1980). Second, in contradistinction to regulations within the nation-state where the "shadow of hierarchy" sets more favorable conditions for cooperation, regulations in the international arena are bound to take place in the "shadow of anarchy" (Oye 1986). As neo-institutionalist approaches have pointed out, under certain conditions cooperation is also likely in the international arena. The main mechanism is iterated prisoners' dilemmas (Axelrod 1984; Keohane 1984) which lead to or are institutionalized in international regimes.[3] Both possibilities have to be excluded as an explanation for the case under consideration. As

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1 In this paper I present results from a research project which is based mainly on my own interview material. I also use standard sources such as Benedick (1991), Cagin/Dray (1993) and Roan (1989). I would like to thank all of my interview partners, especially F. Sherwood Rowland for giving me access to his archive. Previous versions of this article have profited from comments by audiences at the Max Planck Institute for the Study of Societies, Cologne, and at the University of Bielefeld, Faculty of Sociology. I would like to thank especially Thomas Gehring, Philipp Genschel, Adrienne Héritier, Wolf Krohn, Giandomenico Majone, Renate Mayntz, Uwe Schimank and Volker Schneider. I owe special thanks to Philip Manow and Thomas Plümper, who forced me to make my argument much stronger.

2 I do not discuss the question whether their implementation can be considered successful, too. There are indications that this is indeed the case, see Montzka et al. (1996) and Parson/Greene (1995). According to the United Nations Environmental Program (UNEP), world production of chlorofluorocarbons (CFCs) was virtually cut in half during the period from 1986 to 1992. (UNEP 1995: 32). In order to keep technical matters as simple as possible, I shall refer throughout the paper to ozone-depleting substances as "CFCs," although these are not the only ones.

3 See Krasner's definition of an international regime as "implicit or explicit principles, norms, rules and decision-making procedures around which actors' expectations converge in a given area of international relations" (Krasner 1983a: 2).

I shall argue, this case did not follow the logic of a prisoners' dilemma. At the beginning of the international negotiations, main parties to a prospective treaty found themselves in a deadlock.[4] This situation was transformed when the pro-regulation camp no longer contented itself with the status quo (which did not provide regulations). However, the camp which opposed regulations continued to do so even as others were going to take action (i.e. cooperate). Nor can the existence of an international regime be invoked to explain the solution of this collective action problem, since the regime was the result of a prior successful international cooperation.

A third puzzle pertains to the character of the good which is at stake. The ozone layer is a common pool resource, not a public good.[5] This distinction may be subtle, yet it has important consequences. A public good can (in principle) be provided by a single actor without the free-riding of others adversely affecting it. In contrast, unilateral action cannot produce or protect a common pool resource, but can harm or destroy it. As a consequence, all potential polluters of the atmosphere must be part of an international agreement to protect the ozone layer (i.e. the *k*-group is large).[6]

All three reasons seem to pose considerable difficulties for international cooperation. Therefore, its success should not be underestimated. But neither should it be overestimated, as some interpretations of the Montreal Protocol seem to do (for example Benedick 1991). In this view, the agreements were based on the precautionary principle (i.e. on scientific models about future ozone depletion). There is academic support for this argument, suggesting that these agreements were based on scientific consensus (Haas 1992b, 1993; see below). These claims seem either to clash with social science models (according to which governments do not act on the basis of scientific consensus) or rely on additional assumptions.

Certainly, several approaches have been applied to this case.[7] However, I think that they do not grasp fully the essential mechanism which explains the actual

4 Compared to prisoners' dilemmas, deadlocks are much more resistant to solution since each side prefers to defect instead of cooperating. The logic of deadlocks has rarely been investigated in the literature on international relations. See Downs et al. (1986); Axelrod / Keohane (1986).

5 As Keohane and Ostrom put it: "Before CFCs had been invented, the stratospheric ozone layer was a public good; and since it was provided by nature, there was no problem of underprovision. Now it is a common-pool resource, subject to human depletion" (Keohane / Ostrom 1994: 417).

6 This would reduce the chances for successful cooperation even where the logic of a prisoners' dilemma applies (Hardin 1982: 153, 193).

7 Most notably, Haas (1992b, 1993); Maxwell / Weiner (1993); Oye / Maxwell (1994); Rowlands (1995); Sebenius (1992); Sprinz / Vaahtoranta (1994).

cooperation that took place. Maybe this is due to applying theoretical schemes without paying enough attention to the case at hand (see my remarks above). I am aware that in taking the inductive road one runs the opposite danger of overdrawing conclusions which are based on a single case. Specifying general characteristics of the case is therefore crucial in order to avoid false generalizations.[8] However, I do take as a starting point the assumption that international institutions make a decisive difference to the outcome, however one likes to define it.[9]

Briefly stated, the main characteristics of this case are as follows:

1. *Decisions had to be taken under uncertainty.* This points to scientific expertise as a major source of contention. However, cognitive uncertainty leads to rivaling claims about the seriousness of the problem. Science is thus Janus-faced: on the one hand, it could potentially provide a guide for action, while on the other it contributes to fueling a political controversy.

2. *The protection of the ozone layer involved a collective action problem.* As conventional social-science wisdom tells us, collective action problems can be overcome by selective incentives or hierarchical force, i.e. either by the market or the state (Olson 1965). However, global environmental problems emerge in the first place because of market failure and the absence of a world state.

3. *The collective action problem was further complicated by the fact that diffuse interests had to be represented against a well-organized industry.* In other words, long-term, diffuse benefits oppose short-term, concentrated costs. Whereas the benefits are uncertain and accrue to the world population in general, the costs are certain and hit industry which is usually well organized.

I advance a model in which two alliances with conflicting policy goals fight each other by invoking different norms. One alliance is in favor of quick regulations, even if scientific uncertainties remain. The other alliance demands scientific evidence to legitimize regulations. For matters of convenience, I call the first alliance "advocacy coalition," the second counter-alliance.[10] In a dynamic process the first

8 This paper does not analyze the institutional design of the Montreal Protocol, such as decision rules, membership, or compliance enforcement. See Benedick (1991), Bernauer (1995) and Brack (1996).

9 As indicated in Footnote 2, there is evidence that CFC emissions have decreased. This implies that the international regulations are effective in that they have led sovereign states to curb the emissions of their domestic CFC producers.

10 For the sake of simplicity, I ignore here the fact that industry may be divided. The notion of advocacy coalition is based on Sabatier (1993). Drawing on the work of Luce / Raiffa (1957) and Riker (1964), Sebenius uses the terms "winning coalition" and "blocking coalition". Throughout this paper, I use the terms "coalition", "network" and "camp" interchangeably.

manages to put the latter onto the defensive and to gain hegemony by enhancing its credibility dramatically. Important actors of both coalitions come from science, politics, industry, environmental organizations and the mass media. As I will argue, the key to understand the victory of one camp over the other dates back to the 1970s. Decisive resources in this process of gaining hegemony are symbolic resources, closely related to the scientific dispute. They can be defined as "problem definitions" and "solutions." More precisely, they are predictions, principles and solutions.[11] All three exist in two versions, which aligns them with the two camps. There are warning (reassuring) statements and *predictions* either in the form of scenarios or reports about (alleged) catastrophic events. There are competing *principles*, the precautionary vs. the wait-and-see principle. Finally, there are suggestions for *solutions* which oppose each other. On the one side are the advocates of controls of contested substances, on the other those of no controls (or less stringent controls).

After presenting the case in Section 2, I will examine the epistemic community approach in greater detail in Section 3, since it is seen by many authors as a promising candidate for explaining international cooperation in the field of environmental regulation. In Sections 4 and 5, I develop a model which tries to account for the power of ideas in a different way, which then will be applied to the case in Section 6.[12]

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11 They are in part cognitive, in part normative. Principles influence problem definitions and determine their solutions.

12 The results presented in this paper draw on interviews the author conducted in 1994 and 1995 with experts (N = 52), many of whom were atmospheric scientists (N = 27). I gathered and encoded statements by these scientists pertaining *inter alia* to their environmental beliefs and activities, causal relations about CFCs and ozone, and policy options. Although the sample can be considered representative (there are about 300 active atmospheric scientists today who are participating in the international assessment reports published by the World Meteorological Organization, WMO, and UNEP), visible scientists are overrepresented. About half of those interviewed were (at least once) lead authors for chapters of these reports. All authors of major scientific publications in the relevant period are in the sample, as are the three 1995 Nobel Prize winners for chemistry.

2 The Case

In June 1974[13], two chemists of the University of California at Irvine published an article in *Nature* in which they put forward the hypothesis that chlorofluorocarbons (CFCs) could damage the ozone layer (Molina/Rowland 1974). This so-called Molina-Rowland Hypothesis (MRH) called for a revision of the long-cherished belief in the harmlessness of CFCs, which were very popular both with producers and consumers of numerous domestic and industrial appliances since they seemed to be chemically inert, non-toxic and non-corrosive. According to the MRH, CFCs could deplete stratospheric ozone and hence lead to an increase in UV-B radiation, which in turn would have severe effects on biological systems (causing skin cancer in humans, crop damage, algae diminution) and on the global climate.

From the very beginning, there was open controversy between two camps. The camp against CFC controls demanded more time for scientific research before addressing the question of controls. The pro-regulation camp believed that although little was known, this knowledge sufficed to warrant controls. This had a self-binding effect on both parties involved in the controversy. Only by this token could scientific evidence play such an important role in the whole process. Both tried to adduce reasons for the rationality of their logic and support their arguments with empirical data. Perennial questions were: What shall we do in the face of doubt? How long can we wait? Which of the two sides has the burden of proof?

On June 30, 1975, the Du Pont company ran a full-page ad in the *New York Times* and declared: "Should reputable evidence show that some fluorocarbons cause a health hazard through depletion of the ozone layer, we are prepared to stop production of the offending compounds." In the following years, Du Pont took great pains to make this point, namely that *reputable evidence* was not available. In the same ad the company spelled out the program for the coming decade: "Claim meets counterclaim. Assumptions are challenged on both sides. And nothing is settled."

Two years after publication of the MRH, citing a report of the US National Academy of Sciences, a government task force recommended banning CFC 11 and 12 in all non-essential uses (such as propellants in spray cans). As early as December 1978, this recommendation had become reality (Bastian 1982). This swift action by the United States was soon followed by other countries (like Canada and Sweden), but not by other main producer countries (like the EC, the Soviet Union or

13 For an overview of the historical development of the case see Table 1.

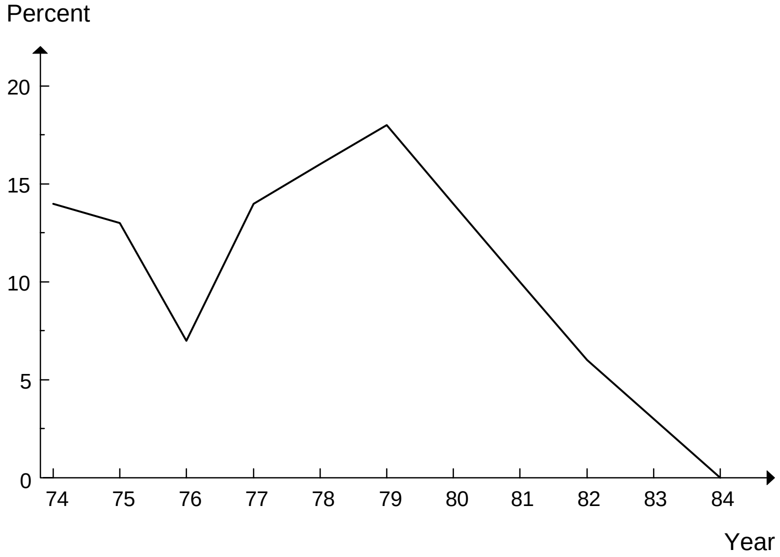
Table 1 Short History of Key Actors, Resources, and Regulations, 1974–1992

1974	Molina-Rowland Hypothesis
1975	US government task force (IMOS) sees “considerable cause for concern”
1976	First report of the US National Academy of Sciences (NAS) estimates 6–7.5% ozone depletion
1977	US Clean Air Act: Aerosol ban. First intergovernmental meeting in Washington
1978	Canada, Sweden, Norway impose an aerosol ban
1979	Second NAS report: estimate of 16.5% ozone depletion
1980	EC: 30% reduction in aerosols, production capacity cap
1982	Third NAS report: estimate of 5–9% ozone depletion
1984	Fourth NAS report: estimate of 2–4% ozone depletion
1985	Vienna Convention for the Protection of the Ozone Layer British Antarctic Survey: Ozone dramatically low over Antarctica
1986	“Ozone hole” accepted as real: • Activation of most atmospheric scientists • Germany in favor of international regulations • Du Pont in favor of international regulations
1987	Montreal Protocol on Substances that Deplete the Ozone Layer: US, Japan, Soviet Union and EC agree on CFC controls, immediate freeze and 50% cutback by 1999 of 5 ozone-depleting substances
1988	Scientists working in WMO-UNEP framework: • Ozone hole caused by CFCs • Global ozone depletion detected Du Pont declares CFC phase-out
1990	London Amendments: • Great Britain and EC in favor of tighter regulations • Developing countries not opposed, depending on monetary aid
1991	• German CFC halon ban • US Clean Air Act amended
1992	Copenhagen Amendments: Multilateral Fund established

Japan). These countries followed nine years later when they agreed to sign the Montreal Protocol.[14] In the meantime, the issue seemed to fall off the political agenda. Following the early regulations at the end of the 1970s, there was evi-

14 This is something of a simplification since the regulations of the Montreal Protocol did not restrict specific applications. Rather, they called for a production cap and a 50% reduction in certain major ozone-depleting substances (such as CFCs 11 and 12) by the year 2000. However, in order to fulfil the reduction requirements, these countries did actually end up eliminating CFCs as aerosol propellants.

Figure 1 Variations in Predictions of Long-term, Global Ozone Depletion



Source: based on Benedick (1991: 13) and Brodeur (1986)

dence that CFC emissions had decreased. Hence, atmospheric models predicted only little long-term ozone depletion (see Figure 1).

This perception changed quickly after 1985 when a British team of scientists published measurements on dramatically low ozone concentrations over springtime Antarctica (Farman et al. 1985). However, it was not until NASA confirmed their data that they were believed. Only then was the phenomenon accepted as real and, what is more, loaded with symbolic meaning ("ozone hole"), thus enhancing its dramatic impact. This phenomenon was a decisive symbolic resource which galvanized major actors, particularly a large number of the atmospheric researchers in the scientific community. This had a lasting effect on others, who gradually either became actively involved in the pro-regulation camp (the governments of the US, Canada, the Scandinavian countries, Germany) or ceased to put up any resistance against regulations (Du Pont,[15] Japan and the Soviet Union, some countries of the European Community and the developing countries). Every time an actor spoke up against regulations in these twenty years, she insisted that there was not enough scientific evidence. In mid-1988, a scientific consensus emerged with respect to two crucial questions: the explanation of the ozone hole and negative global ozone trends.

15 In autumn 1986, Du Pont declared "that it now would be prudent to limit worldwide emissions of CFCs" (*New York Times*, 11 October 1986).

3 International Cooperation and Epistemic Communities

Why was cooperation possible in 1987 even though the scientific consensus did not come about until one year later?[16] It seems as if Haas (1992b, 1993) goes astray because he makes two questionable assumptions: that policy makers turn to specialists to ameliorate uncertainties, and that they act on the basis of the consensus view held by these specialists. In so doing, Haas overlooks two other possibilities which might be more effective in the process of regulating under conditions of uncertainty. First, epistemic communities might emerge even before decision makers realize that there are problems which involve uncertainties. Because of their political motivation, members of epistemic communities might want to try to influence policy options from the outset. Second, the need for shared knowledge might be more limited than Haas perceives. What we frequently observe is that the specialists are divided. This suggests that the decision makers' problem is aggravated instead of ameliorated. At the same time this allows mechanisms other than cognitive ones to play a role in the struggle over the right policy answers. Whereas the first point suggests that Haas underestimates the potential initiative role of epistemic communities, the second tries to give a more modest ("realistic") account of the effectiveness of scientific ideas on the policy process.

Where rivaling expert claims exist, the cognitive dimension loses its privileged status: there is no longer one authoritative source to which policy makers can turn. Under such conditions, processes of power and manipulation gain importance. If one is committed to the precautionary principle, it is sufficient to have a vague idea about the size of the damage and the likelihood of its occurring. The interesting question is, then, how the precautionary principle becomes dominant. Likewise, to react to alarm signals, one does not need to have a complete analysis of the causes. Alarm signals were in evidence throughout the preparatory phase of the international negotiations both in the form of scenarios and of actual signals. In other words, the causes of global ozone destruction (in middle and Northern latitudes) were not well understood then (and are still not today). Various theories exist, but there is no consensus. And even the consensus of 1988 was limited. The Ozone Trends Panel (OTP), which was formed after the discovery of the ozone hole, established global ozone losses in 1988 as a scientific fact, without being able to give the reasons. At that time only the hole over the Antarctic could

16 The high advocacy of atmospheric scientists around 1986 (which is shown in detail later in Table 4) was surely perceived by policy makers as "consensus." However, as I try to argue below, one has to distinguish between a "normative" and a "cognitive" consensus.

be explained fairly well – one year after the signing of the Montreal Protocol.[17] While Haas recognizes the problem of the international cooperation of the Montreal Protocol being concluded one year before this "consensus," he does not adequately account for it.[18]

It seems as if Haas recognizes that scientific consensus cannot play the role of an independent variable. But the image remains strong. Sebenius also holds that potential agreement is more likely "as interests and beliefs about causal mechanisms converge" (Sebenius 1992: 354). However, both make one crucial assumption in order to explain international cooperation: Actors without interests, who are only committed to their cognitive frameworks or "nonmaterial interests," as Sebenius calls them, "can function in analytically identical ways to more conventional tangible interests" (Sebenius 1992: 355; cf. also Haas 1992a: 17). The empirical validity of this assumption was challenged (Young 1991), but not the underlying rationale. For most authors in the field of international relations, it seems to be generally accepted that actors without interests make cooperation less problematic. As I try to argue, this is wrong if one puts norms and ideas in the place of interests since we then have actors with a high ideological commitment.[19] And high ideological commitment makes bargaining more difficult, not less so (Hirschman 1977; Pizzorno 1986).

Since contested expert judgments always claim to be based on scientific data, part of the controversy revolves around the question of what is "good" science.[20] If a scientist from a highly reputable lab publishes a measurement which contradicts another measurement made by a researcher of a lesser known university, the conditions for reception are distributed unequally within the scientific community. When a scientist enters the public arena, this criterion matters much less. It has been shown that it is rather (public) celebrity than (scientific) reputation which makes a scientist become the object of media attention.

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17 If one takes the overlap between model predictions and measurements as an indicator, it is said to be somewhere between 60% and 90%, whereas in the case of mid-latitudes the fit is only 50% (various interviews).

18 Likewise, Ian Rowlands (of the London School of Economics, not to be confused with F.S. Rowland, the chemist) observes that the politicians "signed the agreement in September 1987, while scientists did not publish their report until March 1988" (Rowlands 1995: 89). This invalidates his hypothesis that a "consensus regarding the problem's causal relations is necessary for international co-operation" (Rowlands 1995: 30).

19 "I argue that an epistemic community can be understood as a special kind of de facto natural coalition of 'believers' whose main interest lies not in the material sphere but instead in fostering the adoption of the community's policy project," as Sebenius (1992: 325) put it.

20 There is an abundant literature, see Gieryn (1995), Jasanoff (1990), Nelkin (1979), Porter (1995), Wynne (1992).

> Today's scientists become visible primarily not for discoveries, for popularization, or for leading the scientific community, but for activities in the tumultuous world of politics and controversy. ... To succeed, they must be knowledgeable, articulate, dramatic, persistent, and sophisticated about press operations. Those who do succeed become known to the public not for their science but for their public involvement. (Goodell 1997: 4)

Therefore, the role of visible scientists can be decisive in public controversies which involve a scientific argument.[21]

Haas seems undecided between a rationalistic and a normative interpretation of the meaning of epistemic community. In the rational version the influence of the epistemic community on the policy process seems like the victory of reason over narrow economic interests. In the normative version the strong ideological component casts doubt on the purely "objective" value of scientific advice. This ambiguity results from Haas's definition of epistemic communities: "It is the combination of having a shared set of causal and principled (analytic and normative) beliefs, a consensual knowledge base, and a common policy enterprise (common interests) that distinguishes epistemic communities from various other groups" (Haas 1992a: 18). Haas distinguishes members of these communities from interest groups on the one side and from policy entrepreneurs on the other. Members of epistemic communities will, unlike interest groups, withdraw from the political debate when confronted with anomalies which cast doubt on their causal assumptions (Haas 1992a: 18). However, in contrast to policy entrepreneurs, they subordinate their causal assumptions to their normative goals (Haas 1992a: 20). Taken together, epistemic communities are different from interest groups because of their ideas (causal or normative). They are different from policy entrepreneurs since they pursue normative goals. Here a contradiction arises since a dominance of norms and principles would make a challenge by anomalies not very likely (cf. Kuhn 1962). Such a challenge would only be likely, if the beliefs of the members of the epistemic communities were limited to causal beliefs. Haas has to make up his mind: either epistemic communities are committed to their causal assumptions and withdraw from debate if they are seriously challenged or they are primarily committed to normative principles to which they stick even in cases where there is good evidence to doubt the causal models.[22] This latter case is much

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21 Again, this makes for a more "realistic" picture of the role of scientists than the one offered by Haas. As he tells us, the "epistemic community members' professional training, prestige, and reputation for expertise in an area highly valued by society or elite decision makers accord them access to the political system and legitimize ... their activities" (Haas 1992a: 17).

22 Cf. the following definition of an epistemic community: It is a "knowledge-based network of specialists who share beliefs in cause-and-effect relations, validity tests, and underlying principled values and pursue common policy goals. Their orienta-

closer to the historical facts, but it has one fatal flaw. It cannot adequately explain international cooperation because it only focuses on "neutral" technical or scientific expert knowledge. Epistemic communities are thus drawn into the struggle of the contending camps for competing policy goals. Their fixed ideological orientations guarantee that the two camps remain stable over longer periods of time. In this plausible version, Haas's model comes close to Sabatier's (1993) *advocacy coalition framework*. This gets Haas into trouble: Interest mediation is unlikely if the ideological commitment of the actors is too high.

4 Combining Interests, Ideas and Power

Broadly speaking, one can distinguish between reductionist and integrative approaches towards explaining cooperation in the ozone case. Among the reductionists, there seems to be a growing awareness that monocausal explanations fly in the face of empirical evidence, which makes them add more variables (Haas 1993; Oye / Maxwel 1994) or introduce ad hoc arguments.[23] The integrative approaches rightly see that only a combination of interests, ideas and power is able to illuminate the problem at hand (Keohane / Ostrom 1994; Sebenius 1992; Young 1991). But here the problem arises of how to integrate these different elements into a coherent whole.

The present paper is not an exercise in rational choice or game theory. While I do not deny that possible explanations could emerge from such approaches, I do not try to provide them.[24] Rational-choice theories see two possible solutions to global ecological problems (cf. Ostrom 1990): hierarchical force (Ophuls 1973; Hardin 1978) or the establishment of property rights (especially in cases where the problem of the commons is present) (Demsetz 1967). My argument is that

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tion is perhaps best expressed in the words of one member, who voiced his willingness to accept the 'plausibility of a causal link without certainty'" (Haas 1992b: 187–188). In another place Haas writes: "Because of their environmental values, the members of the group advocated anticipatory action despite the range of uncertainties" (Haas 1993: 176).

23 Sprinz / Vaahtoranta (1994: 94) encounter the oddity that central actors (the United States and Germany) do not conform to their model predictions. Their reaction, however, is not to revise their model, but to introduce the argument that the development of new technology accounts for this divergence.

24 Any such attempt would have to solve the problem of preference change during interaction, which is an extremely difficult task: "Uncertainty and strategic interaction, taken separately, create problems for rational belief formation. When both are present, they wreak havoc" (Elster 1990: 29).

neither market nor hierarchy can be expected to solve global ecological problems. Instead, I argue that policy networks play a decisive role in international cooperation and regulations. They are able to mobilize actors and resources. In cases where uncertainties loom large, symbolic and cognitive resources are most important. Among them, problem definitions and solutions can function as structuring cores in a social and political controversy. Under conditions of high uncertainty they have the power to prestructure a conflict and to accomplish a virtual grouping of potential allies (which can be realized in the course of the policy process). The recent debate has ranked these under the heading of ideas (see, for example, Goldstein / Keohane 1993). As Risse-Kappen rightly points out, "ideas do not float freely" but are mobilized by networks (Risse-Kappen 1994). It is often observed that under conditions of high uncertainty relevant actors in a policy field claim scientific evidence for their preferred options. Only by this token can science achieve such a prominent place in political controversies. The consequence is that the actors involved make a precommitment: They bind their future options to the outcome of scientific research, sometimes without considering that this outcome could be unpleasant for them.

In claiming that the institutional form of the network explains the whole process, I have to show at least two things: first, why interests, power or knowledge *per se* were not structural variables and, secondly, how networks led to the emergence of surprising cooperation. As regards the first point, Young rightly observed that each of these "captures some important features of reality but none offers a satisfactory account of the full range of observable phenomena" (Young 1989: 213). Where scholars have set themselves the task of combining these factors, (Haas 1993; Maxwell / Weiner 1993; Rowlands 1995) they end up with an additive approach, thereby leaving the theoretical status of the explanation in limbo. In contrast, I try to devise a dynamic model in Section 5 in which power, interests and ideas can be brought together in a more coherent way. This model will be applied to the present case in Section 6, showing how cooperation could in fact come about.

4.1 Interests

Contrary to other authors, I do not try to offer an explanation in which economic preferences of industry play a key role in the emergence of international cooperation.[25] Instead, I start with the role of interest organization. If one looks at the

25 Maxwell / Weiner (1993); Oye / Maxwell (1994); Rowlands (1995). No doubt, the preference change of Du Pont was one of the decisive points in the chain of events. However, this was rather brought about by the mobilizing force of the pro-regulation

Table 2 Typology of Interest Organization

		Costs Diffuse	*Costs* Concentrated
Benefits	Diffuse	Majority policy	Policy entrepreneurs
Benefits	Concentrated	Clientelism	Interest groups

Cf. Wilson (1980)

distribution of costs and benefits, one can distinguish along two dichotomous dimensions, namely concentration and dispersion. A cross-tabulation yields four ideal-typical cases, which denote four types of regulatory policy (Wilson 1980; cf. Table 2).

(1) If costs and benefits are widely dispersed, a *majority policy* is most likely. This policy need not, however, lead to regulations. An important precondition is that the issue gets onto the political agenda and that the proposed regulations are not contested. (2) If benefits are spread widely, but the costs concentrated within a small group, the power of the latter usually prevails unless someone takes the issue up and fights for it. This can be single actors, like policy entrepreneurs, public interest groups or policy networks. I call them *speakers* or advocates.[26] (3) If benefits are concentrated and costs are shared by many, we get a policy of *clientelism*, since a small number of well-organized groups profit from the regulation whereas the public at large has little incentive to mobilize. (4) *Interest groups* emerge around special interests in cases where both benefits and costs are shared by only a few. In this case one group in society gets subsidy payments at the expense of another group.

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network, not by an endogenous process (see below). The truism that there are economic reasons for every preference change of industry is no comfort: It is merely bad functionalism.

26 Michael Taylor defines the role of policy entrepreneurs as follows: "In what sense do political entrepreneurs or leaders 'solve' or remove collective action problems? In general, to solve or remove a collective action problem he or she must of course change individual preferences (or more generally attitudes), or change beliefs (including expectations) or inject resources (very probably knowledge, or new technology, like guns) into the group so as to make its members' efforts more productive" (Taylor 1987: 24). See below for the provision of "guns." It should be noted that this definition of policy entrepreneurs differs from Kingdon's (1984), which is why I chose the term "speaker" as a more generic term.

Above I argued that the ozone layer is a common pool resource which can only be protected by speakers who represent diffuse interests. In their absence, we have to assume as a null hypothesis the prevalence of well-organized interest groups.

4.2 Ideas

The analysis of the interest dimension does not guarantee that policy entrepreneurs will be around for any specific issue, much less that they will win out against industry. The reason they do emerge and can be successful at times has to do with a second characteristic of the policy field: the role of ideas in decisions under uncertainty. Policy entrepreneurs are able to inject symbolic resources into the policy process, thus manipulating the behavior of decision makers. Lau et al. see in the use of interpretations the most frequent mode of manipulation in the policy process. "The aim of each interpretation is to emphasize a dimension of judgment that will lead people to prefer one policy proposal over competing, alternative proposals."[27]

Under conditions of uncertainty, ideas facilitate actors' selection from among problem definitions and solutions. Ideas can play a passive or active role. They play a more passive role (a) when used as post-hoc legitimations of decisions, as the garbage can model exemplifies,[28] or (b) when they figure conspicuously ("focal points" [Schelling 1960]) and channel the process in a certain direction. They play an active role when they serve as resources for the contending camps.

4.3 Power

As one of the proponents of the "realist school" has rightly pointed out, "knowledge alone is never enough to explain either the creation or the functioning of a regime. Interests and power cannot be banished. But knowledge and understanding can affect regimes. If regimes matter, then cognitive understanding can matter as well" (Krasner 1983b: 368). The acknowledgment of the role of ideas from someone who usually focuses on power-based explanations of international cooperation is valuable (if ironic), as is the fact that one of the foremost propo-

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27 Lau et al. (1991: 645). Negotiation theory, too, stresses this point, see Sebenius (1992). These approaches are based on Schattschneider's notion of mobilization of bias and related work by Bachrach / Baratz (1970) and Lukes (1974).

28 Cohen / March / Olsen (1972). For an application of this model to the case of climate change, see Hart / Victor (1993).

nents of a knowledge-based explanation invokes elements of both power- and interest-based approaches (Haas 1993). But how can one incorporate the power dimension into the proposed framework? To anticipate my answer to this question (which will be developed at greater length in the next section): out of two competing policy networks one gained hegemony over the other. I will argue that transnational policy networks are the necessary condition for international cooperation in policy fields in which diffuse interests and high uncertainty have to be dealt with. Unlike a cognitive explanation, the most promising mechanism to explain hegemony is the winning of resources or actors from the competing network. Once hegemony is achieved, the high ideological commitment is no longer an obstacle to cooperation but an enabling device. It accounts for the fact that concessions can be made which would not come forward if purely economic reasons were to underlie the range of preferences.

5 Self-reinforcing Networks

One of the requirements of model construction is certainly parsimony. Confronted with a 20-year process, the model should be able to incorporate the historical dynamic of the process, yet remain sufficiently simple. The self-reinforcing dynamic of the one network and the self-defeating dynamic of the other explain the international cooperation in a sufficiently parsimonious way without sacrificing the historical data in a rush for monocausal explanations. My inspiration for such a model comes from Maruyama's negative and positive feedback loops. The former are deviation-minimizing feedback processes, the latter deviation-amplifying processes.[29] In positive feedback loops initial conditions and internal feedback are the two determinants for the future development of the system. Such processes are to be found in nature and society alike. An example for a natural positive feedback loop is the weathering of a rock, where a "small crack in a rock collects some water. The water freezes and makes the crack larger. A larger crack collects more water, which makes the crack still larger" (Maruyama 1963: 166). Examples from the social sciences are also well known, where they have been developed in economics and sociology (Arthur 1990; David 1985; Merton 1973; Myrdal 1937; Tinbergen / Polak 1950). Often these processes are la-

29 Maruyama (1963). Maruyama's distinction departed from the common view, which concentrated on negative feedback loops (the thermostat was the global model of this view). According to him, this "first cybernetics" (based on negative feedback or "deviation-counteracting mutual causal processes") excluded the equally interesting mechanism of deviation-amplifying processes (thus labeled "second cybernetics").

beled path-dependent; like mutual positive feedback loops, they are morphogenetic processes which are not determined by outside factors. In contradistinction to an influential philosophical tradition, Maruyama develops the theorem that similar initial conditions can lead to dissimilar products, since different endogenous processes create an internal order of their own, and can even construct an environment of their own.[30]

Social feedback loops are similar to natural feedback processes, but need some modification (Mayntz 1989). They are characterized by circular stimulation of actors and their motivations. Such processes produce results which are used for their own perpetuation and, what is more, they tend to involve other actors from their environment. Such actors may be drawn willy-nilly into the dynamics of networks which operate on the basis of self-reinforcing feedback loops (cf. Mayntz / Nedelmann 1987).

5.1 Network Dynamics

The circular motivational stimulation of social actors is one of the outstanding features of controversies. A network may mobilize resources and thus attract new allies or vice versa.[31] One can distinguish four mechanisms of network growth:

1. A network gains a new ally which leads to the growth of the network.
2. A network mobilizes a new resource which leads to the growth of the network.
3. A network mobilizes a new ally which carries a new resource which leads to the growth of the network.
4. A network mobilizes a new resource which leads to the enrollment of new ally which in turn leads to the growth of the network.

At the heart of these mechanisms lies the credibility of a network's policy goals: a network is able to attract allies if they can be convinced that the case in point is sound. This belief can depend both on the number and reputation of the actors already enrolled and on the nature of the resources. Therefore, it is of vital importance for the network to mobilize resources since this will increase the likelihood of attracting more allies. Since the symbolic value of resources is critical, material

30 "A sacred law of causality in the classical philosophy stated that similar conditions produce similar effects. Consequently, dissimilar results were attributed to dissimilar conditions. Many scientific researchers were dictated by this philosophy" (Maruyama 1963: 166).

31 In what follows I focus on positive feedback loops. All I say applies, ceteris paribus, to negative feedback loops as well.

resources are typically used to produce symbolic resources. Money does not enhance the credibility of a claim (if anything, it is counterproductive), but the assertion of a likely hazard does, if plausible reasons can be given.[32] This indicates that material resources serve as input into a process (scientific research) which puts out non-material goods. It also indicates that countries without the material infrastructure to conduct scientific research in a competitive way are unlikely to contribute symbolic resources to the controversy.

The growth processes listed above can be linear or non-linear. Cases of linear network growth are cases in which a constant number of allies (or of new resources) are mobilized within a given period of time. Cases of non-linear growth are cases in which differing numbers of new actors (or of new resources) are attracted (Granovetter 1978).

This describes the general mechanisms of the network dynamics. In cases like the one under consideration, where symbolic resources are decisive, science and the public can be influential. Speakers or advocates may mobilize actors and resources or turn contingent events into symbolic resources. As soon as the representation of diffuse interests is made by such advocates, organized interests have to be represented by advocates, too. By this token, the structural advantage of organized interests is relativized. As Majone pointed out:

> Because policy is made out of language, arguments are used at every stage of the process. Every politician understands that ideas and arguments are needed not only to clarify his position with respect to an issue, but to bring other people around to his position. Even when a policy is best explained by the actions of groups seeking selfish goals, those who seek to justify the policy must appeal to the intellectual merits of the case. (Majone 1996)

In the public debate, arguments are compelled to present themselves in such a way that they are universally acceptable. The quest for scientific certainty (the "wait-and-see" approach) and the precautionary principle are most prominent among them.

32 One might suspect a bias in media coverage in favor of catastrophic events since they are more attractive than comforting statements. This increases the probability of moving the issue up the political agenda.

5.2 Antagonistic Structure

Another possibility of non-linear growth is provided by the dualistic structure of policy fields in which long-lasting controversies persist. The mechanism, put briefly, is the growth of one network at the expense of the other. If every actor follows a tacit algorithm like "I change my preference if X," then critical thresholds will be passed *if* X. X may be an event, the availability of a new resource, or the change in the preference of another actor.

In this case, the "deviation" of a resource leads to the enrollment of an actor by the rivaling pro-regulation network, although this actor was initially tending towards the anti-regulation network. Such mechanisms can decisively change the balance of power between networks. In such cases a large number of actors are mobilized who had been undecided between the two. Imagine many actors who all follow the same algorithm. The algorithm will be the same if the definition of the situation is consistently shared by the relevant actors. If one side succeeds in winning over resources or allies from the adversary alliance, the feedback can turn into a chain reaction. The growth of one side at the expense of the other will lead to lasting changes the balance of power.

6 Application

6.1 The Mobilization of Diffuse Interests by Advocates

Advocate scientists who were in favor of regulations showed remarkable skills in dealing with the public. Operating in the United States, they succeeded in presenting the case in a convincing way in public discussions, with school classes, during parliamentary hearings, in interviews and in media statements. They also developed a political strategy for the protection of the ozone layer. In the early days it was the proposal to replace CFC in spray cans which led to a supportive reaction in the United States, on the part of both consumers and lawmakers. In the 1980s the advocate scientists tried to determine the scientific basis for a comprehensive solution, speaking out publicly after 1985 in favor of a worldwide ban of CFCs. Their motivation and world views are captured by the following two statements:

> Our calculations [in 1974] made the situation very bad. The rate of growth of CFCs was enormous. There was a rate of 10–15% per year, so we had to assume a doubling time between 5 and 10 years. Then we had the natural delay time, the

> reservoir in the lower atmosphere had not been diminished for some decades, so it looked as if the ozone layer would be harmed. I was very concerned from the beginning and spoke out. (Interview 5)

> In my view it was not appropriate to release large amounts of any thing into the environment without knowing what happens to it... I was in favor of regulations, although I clearly had to let a little bit of time go by to see how the scientific community would receive this first. Very soon thereafter it became clear to me that we had to be advocates to this issue. We had to carry the voice for a regulation to happen. (Interview 13)

6.2 The Role of Symbolic Resources

Symbolic resources were important in many ways. I shall give three examples: the definition of the problem, principled solutions, and alarm signals. All shaped the course of events from the beginning. As we shall see, the growth of the pro-regulation network was non-linear both in the period 1974–1976 and after 1985. The difference was that it was only in the second period that one network won allies from the opposing camp.

Definition of the Problem: The Molina-Rowland Hypothesis

The MRH claimed that a causal chain was operating in which the following five steps were crucial:

- CFCs ascend to the stratosphere where they are broken down by UV-radiation.
- Free chlorine radicals destroy ozone in a catalytic chain reaction which leads to
- depletion of stratospheric ozone, which in turn leads to
- increased UV radiation, which is
- harmful to humans and other living beings.

Both parties to the controversy tried to attack the problem at the first step, i.e. to find out the role of industrially produced substances that are believed to cause damage. At the end of the causal chain were effects on biosystems (e.g. skin cancer, primary growth), which were never nearly as fully researched. Neither was the "middle link," increased UV radiation. However, the power of focusing on anthropogenic causes (with unknown but potentially disastrous effects) was so great that all relevant actors were committed to them, including some of the chemical firms. It was only after all the regulations in international and domestic law were put into place that a countermovement *visibly* started to cast doubt on the less well-established facts of the causal chain.[33] The precommitment of relevant actors to science and the agreement that the beginning of the causal chain was decisive led to the creation of a peculiar battle field: the atmospheric sciences and their predictions of future ozone losses. If we look at the influence of scientific data on the policy process, it is striking that the first decade of the controversy (1974–1984) focused on estimates of future ozone depletion. These estimates oscillated considerably, ranging from 0 to nearly 20% (cf. Figure 1). As the situation seemed to relax in the early eighties, the issue did not attract any political interest, although the likely effects of ozone depletion on human health were considered to be much more serious than in previous years. Likewise, even when ozone depletion estimates were low, there was good reason to suspect climatic changes as a result of a redistribution of ozone from high altitudes (where it is destroyed) to low altitudes (where relatively more is created; Brodeur 1986).

Principled Solutions: The Precautionary versus the Wait-and-See Approach

From the beginning of the controversy two exclusive principles were competing: the precautionary principle and the wait-and-see principle. Both tried to adduce reasons for their credibility and support their case with empirical data. Perennial questions were: What shall we do in the face of doubt? How long can we wait? Which of the two sides has the burden of proof? A powerful tool in this process was the shifting of the burden of proof. In the United States in 1976, the chair of the Council for Environmental Quality (QEC), Russell Peterson, coined the phrase: "Chemicals are not innocent until proven guilty".[34] This definition of the situation amounted to a reversal of the burden of proof. It confronted in an ingenious fashion the argument of the chemical industry, which had also used a legal analogy when a Du Pont spokesman said that he wanted a fair trial, not a lynching party. Peterson's proclamation carried a special weight since he had been with Du Pont for more than 25 years before he became Governor of Delaware and eventually chair of the QEC.

The pro-regulation network succeeded in institutionalizing the precautionary principle in the Clean Air Act of 1977. It authorized the administrator of the Environmental Protection Agency, EPA, to regulate "any substance … which in his

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33 Proponents of such skepticism with scientific backgrounds have existed from the early days of the debate, cf. Elsaesser (1978), Singer (1989) and the account given in Dotto / Schiff (1978: 283). One of their standard arguments is that a 1 or 2% UV-B increase is not harmful since it is equivalent to moving 25 kilometers South (Elsaesser 1994: 44).

34 *New York Times*, 18 September 1976.

judgment may reasonably be anticipated to affect the stratosphere, especially ozone in the stratosphere, if such effect may reasonably be anticipated to endanger public health or welfare."[35] The key here was that no conclusive proof was necessary for action to be taken – just a reasonable expectation.[36] Nearly a decade later, this concept shaped the US position on international controls as well. Like the definition of the problem, principled solutions were also taking root early in the history of the controversy.

Sounding the Alarm: The Ozone Hole

The most powerful resource in support of the pro-regulation network in the 1980s was the ozone hole and its symbolic use. Without this alarm signal it would have been very unlikely that the deadlock between countries in favor of regulation and those against could ever have been broken. In May 1985, a team of scientists of the British Antarctic Survey (BAS) published an article in *Nature* claiming that they had found ozone concentrations over Halley Bay that were 40% below normal. These measurements were one-point measurements, which meant that they were not considered representative. As one scientist put it, "When I heard about the discovery of the Antarctic ozone hole, I thought it must have been very bad measurements" (Interview 11).

The article in *Nature* had a tough time getting through the review process, too. One referee said, "This is quite impossible, but if it is true it is actually quite important, better publish it." Another challenged the correlation between CFC and the observed phenomenon.[37] A NASA satellite was recording global ozone concentrations, but its data did not confirm the British measurements. As it turned out, NASA had deliberately excluded very low levels of ozone as errors in order to reduce the amount of data. It was mainly due to the activity of one of the early advocates that the British data were taken seriously. This was no easy task since this could involve a potential embarrassment for NASA.[38] Once the phenomenon was "real," and once the highly suggestive symbolic term "ozone hole" had been

35 Clean Air Act, 42 U.S.C. §7457(b), quoted in Benedick (1991: 23).

36 EPA 1987: Protection of stratospheric ozone, 52 *Federal Register* 47491.

37 Interview 44. As one scientist told me, "You may make an equally justified plot between the Dow Jones industrial index and the ozone hole. If you have something going up and something going down, then you can always slide the scales and it will look like a correlation, but there is nothing scientific about it" (Interview 30).

38 Personal trust was decisive for the advocate's belief in the British team: "I had met G___, who was Farman's colleague, and realized that this was a superb professional and that you would not be going to dismiss their data. They knew what they were doing" (Interview 16).

coined,[39] it had an enormous impact on the international negotiations. The term "ozone hole" is a metaphor, since the depletion of the ozone column is never complete ("only" 50% of the column is destroyed, which occurs between the altitudes of 12 and 22 km; above and below there are no drastic changes). On November 6, 1985, one of the early advocates gave a seminar at the University of Maryland, describing the phenomenon as an "ozone hole." He also phoned Walter Sullivan of the *New York Times* who ran an article the next day, using the term "hole."[40] Once the term was current, it substituted the earlier metaphor of a "thinning" of the ozone layer. This change in the root metaphor of the controversy led to a changed perception of the problem. Whereas a "thinning" connotes images of a tissue which loses density, the term "hole" evokes the image of irreparable damage, like a hole in a ball which might explode with a big bang.[41] Still, opponents of CFC regulations claimed that too little was known about the causes of the ozone hole to warrant regulations.

In 1986 and 1987 there were two NASA-led expeditions into Antarctica. In the second expedition, the results of one experiment attracted the special attention of scientists and policy makers alike. In the words of an influential scientist who serves as a policy entrepreneur at the interface between science and politics:

> Once we had the aircraft campaign from Punta Arenas, there was in 99.9% of the science-community view no doubt whatsoever. Once we got such a large perturbation and such wonderful results from aircraft and satellite observations of the extent of the ozone hole, that started to galvanize the community fairly quickly. The voices against the anthropogenic mechanism became very, very low.
> (Interview 39)

The result of this aircraft-based measurement (which was obtained by risky flights into the Antarctic vortex, thus enhancing its appropriateness for headline news)[42] showed a clear anti-correlation between ozone and chlorine monoxide. Soon it became known as the "smoking gun." This term, which according to Webster's denotes a "piece of incontrovertible evidence," first surfaced in the

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39 Once again, one of the early scientific advocates coined the term "ozone hole." "It was such a simple description, it's a code word that means 'that phenomenon down there'" (Interview 45). Initially it was rejected by most scientists and, of course, industry.

40 "We've used up our margin of safety and we've used it up frivolously," Rowland told the campus newspaper in Maryland. He demanded an immediate ban of CFCs in the US and worldwide.

41 It is worth noting that the early metaphors of the "thinning" layer were accompanied by the onomatopoetic "Not with a bang, but with a pfffft," which referred to the sound of spraycans (*New York Times*, 21 December 1975). *Der Spiegel* had the headline "Leakage in the spaceship earth."

42 See Roan (1989) for an account of the spectacular and heroic dimensions.

American public during the Watergate scandal. It is a metaphor that catches the proof-like quality of an event, witnessed by anyone who can see. In a way, this can be considered the weapon-like resource Taylor was referring to when he mentioned the resources policy entrepreneurs have at their disposal (cf. Footnote 26). However, this had no bearing on the process of regulation before 1988. During the negotiations which led up to Montreal, two important pieces of information were available to the negotiators: that there were high amounts of chlorine over Antarctica (and that CFCs were thus a likely cause of the ozone hole), and that decreases in global ozone concentrations were likely. The latter information came from a research group which was put together in 1986 to re-evaluate existing long-term ozone data, the Ozone Trends Panel. Before its results were published officially, the message came through unofficially. Ten days before the start of the negotiations in Montreal, the *New Scientist* quoted the NASA Program Manager, Bob Watson, stating that there was a 3% global ozone loss, which presumably had to be attributed to CFCs.[43] Apart from this information, there was the visual representation of the ozone hole in a movie. All must be considered decisive symbolic resources for the pro-regulation network.

> This had a tremendous influence on arriving at an agreement on the Montreal Protocol. Before Montreal and in Montreal, there was no agreement among the big countries on a position. Everyone said: 'Maybe regulations are too costly, and are we really sure about the risks?' But when it was documented that chlorine monoxide really existed in enormous amounts in the vortex, then things started to fit together. (Interview 2)

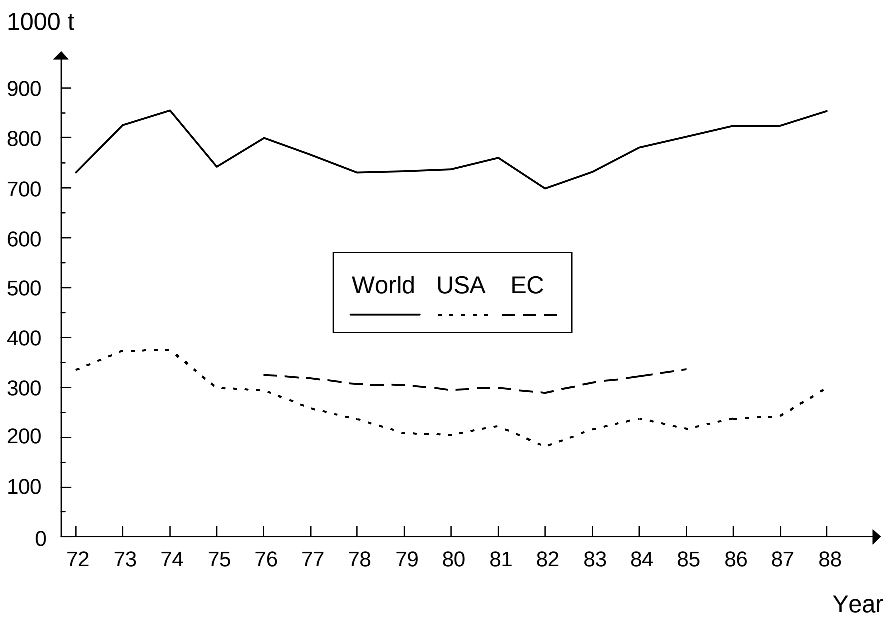
6.3 Actors

Having listed some major symbolic resources which determined the strength of the pro-regulation camp, I now turn to the actor dimension.

In the early period, advocates from both sides used the estimates of future ozone depletion to legitimize their goals, and *interpreted* them in a way that suited their purposes. The advocates of regulation carried the victory. They succeeded in alarming the public and winning important allies early on. The National Resources Defense Council (NRDC), an environmental group, was probably the first to join in November 1974. Other support came from Senator Bumpers, who chaired the House Subcommittee on the Upper Atmosphere, which held hearings during 1975 and 1976. "By the fall of 1975, it was no secret that Bumpers was impressed with the Rowland-Molina theory and the testimony of Rowland, Cicerone and other supporters" (Roan 1989: 46–47). In 1975 a government task force

43 *New Scientist,* 3 September 1987: 24.

Figure 2 Production of CFC 11 and 12 from 1972 to 1988



Source: Dudek et al. (1990)

saw “considerable cause for serious concern” (Council on Environmental Quality/ Federal Council for Science and Technology 1975: 1). In 1976 the National Academy of Sciences released a report which recommended some sort of regulation of CFCs, albeit cautiously. Even before that, industry was faced with consumer boycotts and state laws banning CFCs from spray cans.[44] Walter Sullivan from the *New York Times* brought the issue up in the national press and continuously wrote favorably on the MRH and the network which supported its claims. The public attention caused by him and the subsequent coverage by other papers served as important resources for the CFC critics. However, as CFC production leveled off in the second half of the 1970s (see Figure 2) and new model calculations indicated that the problem was not serious, former proponents of further regulations were put on the defensive: The EPA postponed its projected “phase two” regulations, for instance. The pro-regulation network stagnated but never gave up its policy goal of enacting further CFC controls. Although the new head of the EPA appointed by Reagan regarded the ozone case as spurious, agency officials arduously worked towards an international solution. Although the CFC issue was politically dead around 1983, the anti-regulation network never succeeded in revising existing controls.

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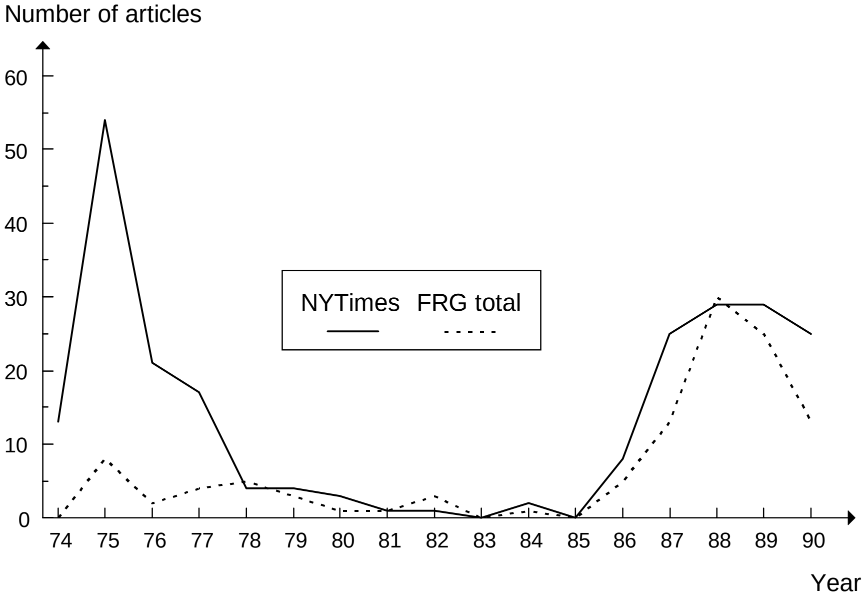
44 The sharp decline in CFC production from 1974 to 1975 was due mainly to an economic recession.

Figure 3 Media Attention on CFCs in US and German Newspapers



Source: German sources were Frankfurter Allgemeine Zeitung, Frankfurter Rundschau, Süddeutsche Zeitung, Der Spiegel, Die Welt, Die Zeit (data base according to the Zeitschriftenindex). For the US, only the New York Times was chosen, because it is the only paper with an index for the whole period. I am grateful to Jürgen Aretz for his assistance on collecting the data.

At the beginning of the 1980s, NASA took over an important role in organizing the international scientific research for the World Meteorological Association (WMO) and UNEP. It put together the results of the various research groups from all over the world. Admittedly, the main findings still came from US laboratories, but the people in charge at NASA made sure that a minimal consensus could eventually emerge between research groups in different countries.

> By 1980, probably the most important thing that I did was to realize that in about two years there were at least six or seven assessments of the state of knowledge. The EC wrote one in the late seventies, so did NASA, NAS, UNEP, WMO, and the British government. At that stage industry and other people were looking rather at the differences than at the commonalties of the different studies. So I tried to work with the international science community toward a single international assessment, so that there was not a German document, a British document, and an American document. We had some success in 1980 in writing a document, by 1985 we had a document that really was representative of the world view. (Interview 39)[45]

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45 This orchestrating of consensus is viewed with skepticism by some scientists. Consider the following example: "We are told that there were 159 scientists from 65

In the second half of the 1980s, the network in favor of regulations was able to muster important allies, like UNEP and WMO, which as international organizations fulfilled the task of coordinating meetings and providing information.[46] They also acted as speakers to the world public, stressing the need to take measures. The executive director of UNEP took sides in the process of negotiations and supported the most stringent control schemes, which is unusual for a high-ranking representative of a UN body. The media started giving much more attention to the issue than in previous years. It became a major theme for the *New York Times*, one of the United States' leading newspapers (Mazur / Lee 1993). The same thing happened in Germany, where the issue had barely attracted any public attention in the 1970s and early 1980s (see Figure 3).

The public reaction was largely due to the mobilization of the majority of the active atmospheric scientists and the spectacular expeditions to the South Pole. Many of these scientists now felt that something had to be done quickly. On the basis of expert interviews conducted by the author (cf. footnote 12), the scientists can be divided into three groups, whose relative size changed over time. I have compared their professed attitudes around 1975 and around 1986. One can distinguish between advocate scientists (who can be visible or quiet), skeptical scientists (who can also be visible or quiet), and undecided scientists:

- Visible advocates are those who were in favor of stringent CFC regulations and spoke out in public. Quiet advocates were in favor of stringent CFC regulations and partly played a role in advising public policy, but they usually did not speak out publicly.
- Skeptics were doubtful as regards the proof for the damaging effects of CFCs on the ozone layer. They were against quick or drastic measures. In the 1970s, two of them were highly visible; in the 1980s there was one quiet skeptic. Around 1986, none of the skeptics publicly opposed regulations.
- The undecided were not visible in public, they waited for data and proof.

As Tables 3 and 4 show, there is a conspicuous difference as regards the relative weight of these groups. Around 1975, there was only a very small group of visible advocates on both sides, while the larger part remained undecided. One of the visible skeptics was European, but none of the visible advocates were. Around 1986 the visible skeptics disappeared and the large group of undecided joined the advocates.

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countries who found out such and such. But in science there is no democracy, there is the possibility that one is against the majority and is right. I don't wish to think we are playing a democratic game" (Interview 28).

46 The involvement of UNEP was spurred by the NRDC way back in the 1970s (cf. Cagin / Dray 1993: 220).

This led to a reinforcement of the network in favor of regulations which, in turn, led to the change in orientation of large corporate actors like Du Pont and the German government. These events, which happened all around 1986, were the most important in a catalytic process which was triggered by the occurrence of the Antarctic ozone hole.

6.4 Agreement in Montreal

Until spring of 1987 the definition of the situation had not yet been agreed upon by the prospective parties to an international protocol. There were two contradicting views, the precautionary and the wait-and-see approach. It was the achievement of the pro-regulation network to make its view binding on all parties to a prospective protocol.[47] This precondition for the negotiation process was not a simple task of spreading scientific truth, but rather one requiring considerable skills of manipulation. Once a common definition of the situation was created, the negotiations started on two questions. How much of which substance will be reduced in what time period? How can the burden be distributed equally? These are interesting questions, but subordinate to the fundamental question of how a common definition of the situation emerged.

Since the strength of the pro-regulation network in the US was decisive, I shall briefly look at how it came into being. The US position for the international negotiations was confirmed in spring 1987. Before that, the anti-regulation network, rallying within the Domestic Policy Council, had sparked off a new controversy within government. The Secretary of the Interior, Hodel, apparently favored a "personal protection plan" instead of international regulations. The NRDC exploited this rumor by making it public, which backfired on Hodel and his allies (Benedick 1991: 60–62; Cagin/Dray 1993: 332–334). In an article with the catchy headline "Advice on Ozone May Be: Wear Hats and Stand in the Shade," the *Wall Street Journal* quoted Hodel as saying: "People who don't stand out in the sun – it doesn't affect them."[48] The attempt to redefine the situation as one of personal risk ended in ridicule once it was made public. EPA administrator Lee Thomas confirmed the US option of a 95% reduction, which was supported by the Secre-

47 Surprised by the speed of the process, Maxwell and Weiner implausibly suggest that there were no real differences in the first place: "The speed with which an agreement was actually reached following discovery of the Antarctic ozone hole provides evidence of a greater convergence of viewpoints than is commonly believed" (Maxwell/Weiner 1993: 37). See Benedick (1991) and Gehring (1994) for details of the controversies.

48 *Wall Street Journal,* 29 May 1987.

Table 3 Profiles of Scientists and Their Distribution, 1975, N = 27

Advocacy	High		Low	
Pro	Visible advocates	3	Quiet advocates	6
Contra	Visible skeptics	2	Quiet skeptics	0
Neither	–		Undecided	16

Table 4 Profiles of Scientists and Their Distribution, 1986, N = 27

Advocacy	High		Low	
Pro	Visible advocates	7	Quiet advocates	17
Contra	Visible skeptics	0	Quiet skeptics	3
Neither	–		Undecided	0

tary of State, Shultz.[49] Shultz appointed Richard Benedick as State Department's Deputy Assistant Secretary for Environment, Health, and Natural Resource Issues and chief negotiator for the international talks.

The official US negotiation position confirmed the provision of the Clean Air Act, namely that the precautionary principle ranked higher than any economic interests.[50] This bargaining position led to a peculiar structure during the international negotiations: there were two opposing camps, one in favor of a comprehensive solution to protect the ozone layer, the other primarily concerned with economic aspects. The first group called for a 95% reduction, the latter wanted no binding reductions. A contract zone emerged when most countries agreed to a production freeze and a 20% cutback. One of the remaining critical issues was the proposal from the pro-regulation network that this be followed by further reduction of 30% within two to four years (Benedick 1991: 64; Gehring 1994: 249–251). Initially, there was reluctance, mainly on part of the United Kingdom, to accept such an automatic cutback. It was overcome by making further reductions contingent upon the evolution of science. This minimal consensus among the contending networks allowed more stringent controls in the end. Again, it was symbolic evidence (credibility) rather than scientific certainty which led to the reversal of their

49 President Reagan eventually came over to this line: The US will go for a 95% reduction but settle for a 50% compromise. Cf. Benedick (1991: 57) and Doniger (1988: 90).

50 While the emergence of the US position was facilitated by the fact that Du Pont did not oppose an international protocol, the company was evidently not directly involved in defining the position.

position. This holds true even for the United Kingdom, which prides itself on having reacted only after the evidence was conclusive (i.e. in 1988). True enough, the UK started to play a much more active role in 1988. However, the UK agreed to an automatic 50% cutback in Montreal, which can mainly be explained by its fear of isolation induced by the enormous credibility of the pro-regulation camp.

6.5 Why Not a Structural Explanation ?

The shift of key actors could be explained differently. It could be argued that their pivotal change in orientation was due to other factors: Du Pont was driven by the search for oligopoly profits, the German government by the aim to monopolize the issue in its domestic policy game and the atmospheric scientists by consensus. Depending on which was the most important, one could find an economic, a political, or a cognitive explanation.

The empirical evidence suggests that the initial kick for the preference change of main actors was Du Pont's change in attitude. Du Pont was the main protagonist of the blocking coalition. It was the most outspoken and visible representative, not only of the CFC producers, but of all those who believed that there was no need to rush into regulatory action. When Du Pont changed its position in the fall of 1986, this led to a split in industry ranks and to confusion among those who believed that CFCs were not posing a major problem. The speaker role went over to Great Britain. In a way, Du Pont's switch was more effective than scientific proof, because all participants and observers of the controversy realized that if the main speaker of the anti-regulation coalition no longer saw any reason to fight international controls, then others might have even less reason to do so. But how can one explain Du Pont's shift in orientation? My argument is that it can be attributed to the anticipated domestic regulations which would have been enacted in the US even if the international negotiations failed. After nearly 15 years of bitter struggle, Du Pont reacted defensively to the strength of the advocacy coalition, which could be expected to grow even stronger after the discovery of the ozone hole. Even before the ozone hole, the NRDC had brought a lawsuit against the EPA which was resolved in an out-of-court settlement[51] in which the EPA agreed to make a decision by November 1987 (Brodeur 1986: 85). Senator Chafee introduced a bill which "called for unilateral CFC cuts by the US, accompanied by trade restrictions against countries who do not reciprocate" (Parsons 1993: 43). It would have required the US to take unilateral action – a move which introduced a self-binding mechanism into the international negotiations Schelling 1960: 28). Du Pont may also have feared lawsuits from skin cancer victims. Fi-

51 It was based on a clause of the Clean Air Act of 1977, cf. Footnote 35.

nally, an insistence on CFCs might have impaired the corporate image of the firm, leading eventually to consumer boycotts. In the light of all this, the question of whether the (uncertain) option for oligopoly profits was at all decisive – as economists would have us believe – is a relatively minor issue.[52]

7 Conclusion

The Montreal Protocol is an instance of successful international cooperation which can be explained by a model in which competing policy networks pre-committed themselves to the results of scientific research, fought for their policy goals based on principles and tried to enhance the public credibility of their case. In a process which took more than a decade, the pro-regulation network eventually gained a position of hegemony. The causes of its success have to be sought in its historical roots in the 1970s, when it succeeded in framing the situation in a way that made precautionary measures look very reasonable and in institutionalizing a set of legal norms which could be used by the pro-regulatory forces in the mid-eighties. The core of this pro-regulation network was made up of advocate scientists, bureaucrats and environmental activists. Their case was supported by powerful allies, such as the National Academy of Sciences, the *New York Times*, CEQ, consumers and a small number of countries who enacted partial CFC bans.

The core of the anti-regulation network was made up of CFC producers and skeptical scientists in the 1970s, part of the Reagan administration, the EC, Japan and the former Soviet Union in the 1980s. In the mid-1980s, NASA, WMO and UNEP tried to put together the scientific evidence in order to decrease uncertainty and legitimate controls. The EPA and the US State Department acted in concert to define a leadership role in the international negotiations. Their most important symbolic resources were the discovery of abnormally low ozone concentrations over springtime Antarctica, the coinage of the metaphor "ozone hole" for this unforeseen phenomenon and the subsequent dramatization of the situation. This led to a catalytic process in which key players changed their orientation completely – a process which was started by the main speaker against regulations (Du Pont) changing sides. Embarrassing public statements from hard-liners in the anti-regulation camp led to a consolidation of the pro-regulation network within the Reagan administration. The switch of the German government was influenced both by its internal policy game and by the activity of the transnational policy

52 Oye / Maxwell (1994). The argument often cited – that Du Pont was ahead of its competitors in finding alternative substances – must be regarded as a myth (cf. Grundmann 1996).

network fighting for regulations. This shift in the balance of power persuaded draggers and undecided countries to sign the 50% compromise of the Montreal Protocol. A second bandwagon process started after 1988 when the British government changed its position, which led to even more stringent measures in the years that followed.[53]

Two questions remain. How can this model be generalized? Does it generate testable hypotheses?

As regards the first question, it seems to me that the model works well for the explanation of national variance in policy outcomes in the 1970s, where the relative strength of a pro-regulation network is correlated to stringent CFC controls, while its absence correlates with lax or no CFC controls. It can be applied to similar cases in which the representation of diffuse interests requires speakers (public interest groups or policy entrepreneurs)and policy decisions have to be taken under uncertainty. Examples abound: Think of cases like acid rain, BSE, or global warming. Advocate scientists are well suited to integrate these two processes. They can represent diffuse interests and reduce uncertainty. However, decisions under uncertainty probably generate two types of experts, one being in favor of regulations, the other against.

This leads to the second question. Policy makers cannot judge which expert is right or wrong – yet they have to decide. The paradox can be resolved in two ways. The first solution can be that each policy network tries to mobilize as many resources and allies as possible and to undermine the credibility of the opposing network. A very effective means of so doing is to recruit resources and allies from the opposing network, which leads to non-linear effects. The other solution emerges when scientific evidence helps to shift the balance between the two sides: This can only happen if both are committed to the results of scientific research and if both agree on what constitutes an unbearable risk or a plausible threat. This second solution is most likely when industry is involved in the scientific research, too. If there is no common definition of the situation and if both camps are equally strong, there will be no agreement on solutions. (Cases where one camp imposes its definition of the situation from the outset are trivial.) The actors' power thus depends heavily on their ability to convince others of their problem definition. A major resource is public credibility, which in turn depends on the availability of symbolic resources.

53 The UK acted as speaker for the anti-regulatory forces within the EC. These were three countries involved in CFC production: Spain, Italy and France. Once the UK switched over, the others followed.

It is not clear whether the model can be applied to cases in which only one of the above conditions exists, i.e. uncertainty combined with well-organized interests or diffuse interests in the absence of uncertainty. This requires further research. I submit the general hypothesis that whenever a speaker of one of the contending camps changes sides, this will enhance the prospects of the camp to which he turns.

Abbreviations

CFCs	Chlorofluorocarbons
CEQ	Council on Environmental Quality (USA)
EC	European Community
EPA	Environmental Protection Agency (USA)
IMOS	Ad hoc Federal Interagency Task Force on the Inadvertent Modification of the Stratosphere (USA)
MRH	Molina-Rowland Hypothesis
NAS	National Academy of Sciences (USA)
NASA	National Aeronautics and Space Administration (USA)
NRDC	National Resources Defense Council (USA)
OTP	Ozone Trends Panel
UNEP	United Nations Environmental Program
WMO	World Meteorological Organization

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