

## **The Precautionary Principle, Knowledge Uncertainty, and Environmental Assessment**

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### ***Introduction***

It was only recently, about fifteen years ago, that the “precautionary principle” rose to international prominence in environmental policy making. Loosely formulated, the principle states that if there is an indication that a certain activity may be harmful to humans or the environment, that activity should be abandoned. The principle provides politicians with the possibility to install measures even when uncertainty still exists about a problem. In this paper I will take a closer look into the way the precautionary principle is supposed to deal with knowledge uncertainty. The background argument of this paper is that the current pace of innovation requires institutional shifts towards reflective governance under scientific uncertainty. There is especially a need for independent “knowledge-and-uncertainty brokers” that can assist societies in applying the precautionary principle in a balanced manner. To lay the groundwork for my argument, I first address some general issues in science for policy. Subsequently, I discuss some examples of science for policy from the area of environmental assessment. The paper continues with a concise discussion on knowledge uncertainty. Finally, the precautionary principle is analyzed in the context of the issues introduced earlier.

### ***Science for Policy***

The relation between science and policy is not as simple as some technocrats would like it to be. The technocratic ideal of science speaking “value-free” truth to political power gained institutional currency in the nineteenth century. It was based on the expectation that the impartiality and objectivity of scientists could help to overcome political conflict (Proctor 1991). In the twentieth century, however, it became evident both that science cannot be value-free and that politics deals more and more with issues that are clouded with uncertainty. Thus, the presence of conflict among scientists, both epistemic and social, makes it hard to provide politicians with neutral advice. Even if scientists cooperate to produce “consensus” advice, this advice must not be considered as the objective truth. This is especially the case if the policy problem is unstructured, that is, when there is both a lack of certainty concerning the kind of knowledge that is relevant to the problem and a lack of consensus on relevant norms and values (Hisschemöller and Hoppe, 1996).

To observers favoring technocratic politics this may seem a hindrance for policy making. But to those who favor participatory politics and problem structuring, the political condition in which we find ourselves when science cannot speak truth to power can be seen as

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an opportunity for the democratization of both the scientific advisory process and politics more in general. This democratization can start from a renewed awareness that it is inevitable to make political decisions under uncertainty:

To be political is to *have* to choose—and, what is worse, to have to choose under the worst possible circumstances, when the grounds of choice are not given a priori or by fiat or by pure knowledge (*episteme*). To be political is thus to be free with a vengeance—to be free in the unwelcome sense of being without guiding standards or determining norms yet under an ineluctable pressure to act, and to act with deliberation and responsibility as well. (Barber 1984, 121)

Scientists—or philosophers, for that matter—cannot deduce the norms that should govern our behavior from empirical research or pure thought. The key concepts of democratic politics are “deliberation” and “responsibility.” Scientists and philosophers can, of course, take part in these deliberations, but they have a responsibility to consider the perspectives of other citizens.

In practice, policy makers do seek scientific advice when dealing with their policy problems. Scientific advice, for example in the form of “regulatory science,” often even plays a crucial role in policy making. But, as was argued by Jasanoff in the context of risk regulation, we can not hope for more than scientific advisers delivering a “serviceable truth”:

a state of knowledge that satisfies tests of scientific acceptability and supports reasoned decisionmaking, but also assures those exposed to risk that their interests have not been sacrificed on the altar of an impossible scientific certainty. (Jasanoff 1990, 250)

Scientific certainty is unattainable for the most important policy problems of the present day. Therefore, the uncertainties in the science used for policy making (“science for policy” for short) are inescapable and have to be dealt with.

The value of scientific advice, including serviceable truths, must be relativized, however. Sometimes science seems to have only a negligible influence on policy making. This led Collingridge and Reeve (1986) to draw a pessimistic conclusion about the practice of scientific advising. They considered two modes for the relation between science and policy: either a political consensus already exists, and supportive scientific evidence is selectively used to legitimate policy, or political opponents keep arguing about technical details, continually deconstructing each other’s scientific claims, not allowing that one of the competing claims gets “black-boxed” (cf. Latour 1987).

Recognizing the often unproductive interactions between science and policy making in the area of risk assessment, especially when the decision stakes and systems uncertainty are very high, Funtowicz and Ravetz proposed to distinguish a new type of risk assessment called “total-environmental assessment” (Funtowicz and Ravetz 1985, 228). This is a form of risk assessment in which the total environment, or context, of a risk issue is taken into account, and which is appropriate to apply to cases with high decision stakes and systems uncertainty (these two variables are correlated, since, the recognition of systems uncertainty is typically

enhanced if the decision stakes are high; cf. Jasanoff and Wynne 1998, 12). A total-environmental assessment

is permeated by qualitative judgments and value commitments. Its result is a contribution to an essentially political debate on larger issues, though no less rational in its own way for that. The inquiry, even into technical questions, takes the form largely of a dialogue, which may be in an advocacy or even in an adversary mode. (Funtowicz and Ravetz 1985, 228–229)

In very polarized settings, the least one can hope for is a “consensus over salient areas of debate” (Funtowicz and Ravetz 1985, 229).

In her study on the role of several American advisory committees, Jasanoff drew a less pessimistic conclusion than Collingridge and Reeve and offered advice on how to improve scientific advising. Her advice was less radical than Funtowicz and Ravetz’s. She had observed that some interactions between science and policy can be judged to have been relatively successful and others not. From her case material, she extracted the following condition for success:

scientific advisory proceedings—no less than administrative proceedings of a nontechnical kind—are most effective in building consensus and guiding policy when they foster negotiation and compromise. (Jasanoff 1990, 230)

Thus, if one wants scientific advice to enter the policy process in an effective manner, a non-advocacy, non-adversary mode of operation seems to be the logical choice. The problem then is, however, how to guarantee that the deliberations of scientific advisers really take the different legitimate viewpoints and interests on the issues into account.

Jasanoff here seeks a middle ground between two naive models of science for policy held by “technocrats” and “democrats.” According to Jasanoff, both the “technocratic” model and its typical “democratic” critique are flawed. The “technocrats” argue for deference to the scientific community when technical matters have to be decided upon (or alternatively, they claim that technical material used in policy making must undergo rigorous peer review by the scientific community). The main problem with their conception of scientific advice is that technical aspects cannot be neatly separated from political aspects. The “democrats,” on the other hand, argue for a better incorporation of different societal values in science-based decision making. They demand that membership of scientific advisory committees should be opened up to include different—not necessarily technical—viewpoints. Jasanoff concludes from her study of scientific advisory committees connected to U.S. administrative agencies that “it is crucial for claims certified by agency advisers to be persuasively labeled “science.”” (Jasanoff 1990, 244). As a corollary, interest groups should not be directly represented in scientific advisory committees.

Instead, according to Jasanoff, the ideal scientific adviser is an esteemed scientist, who “not only transcends disciplinary boundaries and synthesizes knowledge from several fields but also understands the limits of regulatory science and the policy issues confronting the

agency” (Jasanoff 1990, 243). From a democratic point of view, the precondition of the legitimacy of science for policy then is that the advisers, who necessarily exercise political judgment when they give scientific advice, must act as citizens, who are free to make their own choices (cf. Barber 1984, 127). The advisers must thus be responsive to the fact that different societal and political actors may have different perspectives on the underlying uncertainties, and it is important that they integrate these perspectives within the advisory process.

Sometimes, however, it may be impossible to negotiate and reach a compromise. Funtowicz and Ravetz observed that when the stakes and uncertainties are high (and hence the uncertainties are amenable to politicization), that “there are at present no mechanisms towards a consensus on such politicized uncertainties” (Funtowicz and Ravetz 1990, 15). With respect to the possible solution that could follow from an improved management of uncertainty within science for policy, they furthermore modestly conclude:

Any genuine attempt to improve the quality of scientific information as it is used in the policy process must be undertaken with such political realities in mind. (Funtowicz and Ravetz 1990, 16)

Thus, according to Funtowicz and Ravetz, structural changes in the direction of enhanced participation are needed in order to democratize scientific advisory proceedings. For this reason they have generalized their original normative view on risk assessment into a sweeping normative statement on the future of science:

Now global environmental issues present new tasks for science; instead of discovery and application of facts, the new fundamental achievements for science must be in meeting these challenges. . . . In this essay, we make the first articulation of a new scientific method, which does not pretend to be either value-free or ethically neutral. The product of such a method, applied to this new enterprise, is what we call “post-normal science.” (Funtowicz and Ravetz 1991, 138)

In fulfilling this new role,

[s]cience cannot always provide well-founded theories based on experiments for explanation and prediction, but can frequently only achieve at best mathematical models and computer simulations, which are essentially untestable. On the basis of such uncertain inputs, decisions must be made, under somewhat urgent conditions. Therefore, science cannot proceed on the basis of factual predictions, but only on policy forecasts. (Funtowicz and Ravetz 1991, 139)

Where Funtowicz and Ravetz first wrote about “three types of risk assessment,” they subsequently generalized their analysis to “three types of problem-solving strategies,” of which “post-normal science” (or “second order science”) corresponds to “total-environmental” type of risk assessment (Funtowicz and Ravetz 1991, 137, 144–145).

Instead of appointing people who represent interest groups directly to advisory panels, another way to enhance participation, argued for by Funtowicz and Ravetz, is to extend the system of peer review to include more diverse perspectives. The idea is that under conditions of post-normal science

experts whose roots and affiliations lie outside that of those involved in creating or officially regulating the issue must be brought in. These new participants, enriching the traditional peer communities and creating “extended peer communities” are necessary for the transmission of skills and for quality assurance of results. (Funtowicz and Ravetz 1991, 149)

Funtowicz and Ravetz emphasize that not only legitimacy is at stake, but it is really the *quality* of scientific advice which stands to gain from extended peer review, especially if the systems uncertainty is high:

When problems do not have neat solutions, when the phenomena themselves are ambiguous, when all mathematical techniques are open to methodological criticism, then the debates on quality are not enhanced by the exclusion of all but the academic or official experts. (Funtowicz and Ravetz 1991, 149)

As we will see below, the characterization of the methodological acceptability of scientific results constitutes an important part of “dealing with uncertainties.”

### ***Examples of Environmental Assessment***

The different authors cited above all agree that knowledge uncertainty is a dominant feature in present-day science for policy. An area in which this becomes especially obvious is environmental assessment. Environmental assessment is a discipline in which policy-relevant environmental-scientific information is assessed and communicated to policy makers and society at large. In line with the argument in the previous section, many social scientists who have studied the relationship between environmental science and decision making concluded that these two activities cannot be neatly separated. One way to phrase this conclusion is the following: “natural knowledge and political order are co-produced through a common social project that shores up the legitimacy of each” (Jasanoff and Wynne 1998, 16). In this section I will discuss two examples of environmental assessment to illustrate this point. One example comes from climate science and the other comes from ecotoxicology (both cases will also figure in the final section on the precautionary principle).

A much-discussed example of environmental assessment is the ongoing assessment process conducted by the Intergovernmental Panel on Climate Change (IPCC). It was due to widely-publicized warnings from scientists in the 1980s that the public in Western democracies became interested in the risks involved with an enhanced greenhouse effect induced by anthropogenic emissions of CO<sub>2</sub>, leading to a human-induced global warming—and its associated effects, such as sea-level rise. Since the societal changes implied by the different solutions proposed for solving the global-warming problem are quite drastic, one of

the first steps politicians took to address the problem was to ask scientists to regularly assess the state of climate science, as well as the possibilities for adaptation to climate change and mitigation of the problem by reducing anthropogenic greenhouse-gas (mostly CO<sub>2</sub>) emissions. This led to the establishment of the IPCC in 1988.<sup>2</sup> The advisory process involving the IPCC is regarded by many social scientists as being a “co-production” of, on the one hand, our knowledge about the climate system and, on the other hand, the international political order:

The IPCC’s efforts to provide usable knowledge resonated with the belief of sponsoring policy organizations that climate change is a manageable problem within the framework of existing institutions and cultures. (Jasanoff and Wynne 1998, 37)

The alignment of scientific and political views is a common feature of environmental assessment. The phenomenon was studied in detail by Haas in his analysis of the development of the Mediterranean Action Plan (Med Plan), an example of regional environmental cooperation that had arisen for dealing with the issue of marine pollution in the Mediterranean in the 1970s. The Med Plan was agreed upon in 1975 and was implemented in the succeeding decades. During the preparation and implementation of the Med Plan environmental assessments were conducted, but the advisers were much less visible than the IPCC panelists. Haas showed that an “ecological epistemic community” of professionals had been influential within the governments involved in the plan. An “epistemic community” consists of professionals who believe in the same cause-and-effect relationships and share common values (Haas 1990, 55). According to Haas, epistemic communities are particularly effective in influencing policy making in issue areas where uncertainty is high:

As these types of issues with a high degree of uncertainty gain in salience for leaders—as is indeed the case—there is a greater range of influence for epistemic communities that possess authoritative claims to understanding the problems; analyzing them is also more useful. (Haas 1990, 246)

The uncertainty in this case was related to uncertainties in ecotoxicology. The members of the ecological epistemic community active within the Med Plan “shared an abiding belief in ecological principles and were committed to preserving the physical environment, which they thought was threatened by pollution” (Haas 1990, 74–75). The ecological principles defining this ecological epistemological community resided at a conceptual-theoretical level. These principles were partly derived from theoretical-ecological computer simulations that were used to study the behavior of complex ecological systems.

System-dynamical metaphors about ecology had also gained currency in the political domain (cf. Kwa 1987). The shared beliefs in ecological principles were thus spread outside science. This made it possible that during the early negotiations in the beginning of the 1970s

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<sup>2</sup> The IPCC consists of three working groups. The current distribution of subjects addressed by each working group is as follows: Working Group I deals with the natural-scientific basis of climate change; Working Group II addresses issues of impacts, adaptation, and vulnerability; and Working Group III assesses mitigation options.

membership of this epistemic community included, besides the leadership of the United Nations Environment Programme (UNEP):

members of the Greek government, French modelers and systems scientists, UNESCO [United Nations Educational, Scientific and Cultural Organization] bureaucrats, FAO [Food and Agriculture Organization] lawyers, and individuals in the Israeli, Spanish, and Egyptian governments. (Haas 1990, 75)

The authority of the experts was not questioned by actors outside the epistemic community. According to Haas, there were no industry challenges to the decisions made by the experts, because “the saliency of pollution issues was sufficiently low” at that time: issues of marine quality were not highly politicized anymore, which for the countries of the European Economic Community (EEC) could be explained by the presence of an earlier EEC directive. Also, the public was not much interested in marine issues (Haas 1990, 163).

In the climate-change case, obviously, the stakes are high. Despite the massive uncertainties about the subject, the advisers of the IPCC have been able to act as a kind of “climate-change epistemic community” with significant influence on policy making (as I will briefly discuss below and as I will argue more extensively in a book that I am currently working on, tentative titled *Simulating Nature: A Philosophical Study of Computer-Model Uncertainty in Climate Science and Policy*). One evident signs of the presence of this epistemic community is the fact that the top advisers of the IPCC are usually present at meetings of the climate convention, where they typically act as stout defenders of the instalment of climate policies. Furthermore, the government negotiators accept, without reservations, the claims and projections of IPCC Working Group I as the scientific basis underlying the political negotiations.

The policy relevance of the IPCC is formally ensured by its ties to the climate convention and by the fact that it is an intergovernmental body. The reason for establishing the IPCC in 1988 was not only the presence of uncertainties in climate science, but also the perceived need for an international agreement on the issue of climate change. The IPCC became an *intergovernmental* (not just international) body, because in that way the IPCC’s assessment of the available knowledge could not be discredited during the negotiation of policies and actions. In fact, the IPCC probably was so successful because the problem addressed was already considered relevant and consensual legitimation for climate policies was what was sought for.

The ties of the IPCC with political processes aimed at climate action have remained strong ever since, although the link gradually became less direct. With the first comprehensive assessment, released in 1990, the IPCC provided direct input to the policy process. For example, the 1990 report explicitly discussed possible ingredients for a climate convention. After the convention had gained momentum in 1995, the Subsidiary Body for Scientific and Technological Advice (SBSTA) took over the discussion of matters closely related to the convention. This convention body now is the intermediary between the IPCC and the convention and has good working relations with the IPCC.

Currently, after the completion of the Third Assessment Report in 2001, the main role of the SBSTA vis-à-vis the IPCC is to discuss the political impact of the IPCC conclusions in the context of the climate convention, e.g., the definition of “dangerous anthropogenic interference” and, related to that, necessary future commitments (post-Kyoto). The IPCC has made it very clear that the answers to such political questions, although they must be scientifically informed, basically involve value judgements. Another role of the SBSTA is to guide the IPCC in taking up policy-relevant questions, for instance by requesting Special Reports or Technical Papers, or by having governments submit policy-relevant scientific questions to be addressed by the IPCC. The *Climate Change 2001 Synthesis Report*—the final part of the Third Assessment Report—was structured around nine questions that were approved by the IPCC on the basis of submissions solicited by the SBSTA.

The dominance of one specific way to frame the climate-change problem, by using global climate models to project future climate change for different emissions scenarios and different model assumptions, as done by the IPCC, has been criticized in the social-scientific literature. Especially in the early years of the IPCC, the often publicly voiced criticism of climate models by “greenhouse skeptics” was not adequately dealt with, nor clearly reflected in the summaries for policy makers. My research into the production of the Working Group I contribution to the Third Assessment Report of the IPCC (2001) indicates, however, that the criticism of the “greenhouse skeptics” is not warranted for the recent years of the IPCC (a detailed analysis will be presented in my book). Still, perspectives of local peoples on climate change—especially relevant when dealing with impacts, adaptation, and mitigation, but also pertaining to local knowledge on the climate itself—are to a considerable extent left out of the picture provided by the IPCC. This led Jasanoff and Wynne to offer the following advice about future climate-change assessments:

Where will the universal, hegemonic, yet institutionally weakly grounded science of climate change turn for authentic, globally effective legitimation and public authority? The view from the social sciences suggests that the solution lies partly in the patient construction of communities of belief that provide legitimacy through inclusion rather than exclusion, through participation rather than mystification, and through transparency rather than black boxing. (Jasanoff and Wynne 1998, 77)

As indicated, with respect to “greenhouse skeptics,” this advice was taken to heart by the IPCC when they made procedural changes before the production of the Third Assessment Report.

The procedural changes had mainly to do with the review process. One of the main characteristics of the IPCC is its emphasis on the quality of the review process. Since the aim of the IPCC is to produce reports that are not only credible among scientists and governments but also within society at large, representatives of non-governmental organizations are admitted to IPCC sessions as observers and experts from all organizations that represent interested or affected parties are invited to participate in the review process. The reason that the IPCC review process has been so successful is that no single country (or group of countries) has been able to dominate the review process. This can be explained by, on the one

hand, the divergence of interests among countries and the presence of a U.N. procedure of decision making by consensus, and, on the other hand, the intellectual independence of some of the most influential scientists involved.

Although specific stakeholders, notably those representing the interests of fossil-fuel industries and oil-exporting countries (but also several independent critical scientists), still claim that their views are not adequately reflected in the IPCC summaries for policy makers, there was no lack of participation and influence of these actors. The procedural changes that the IPCC has made over the years were mostly intended to make the review process more robust and transparent. The innovation in the production of the Third Assessment Report was the appointment of review editors, whose task it was to ensure that all valid—possibly minority—viewpoints were adequately represented in the reports. Furthermore, the comments submitted by experts and governments and the responses by the lead authors were all made available from the IPCC upon request.

Due to the emphasis on the scientific quality of the texts (bearing in mind other goals, such as policy relevance and clarity), the scientists involved within the IPCC have remained in control of the texts, also of the much-discussed summaries for policy makers. Although governments are allowed to make changes to the summaries—their active use of this right serves to enhance their sense of “ownership” of the texts—the authors have the power to reject proposed changes if they consider these to be inconsistent with the findings of the underlying chapters or if they could result in unbalanced summaries. Contrary to what public critics have sometimes claimed, special-interest organizations do not co-decide on the text; in general, observers are not even allowed to speak at the plenary sessions. This signals the limited nature of the participation of other stakeholders than scientists and governments. Contrary to what one would expect from the social-scientific analyses, these limitations have not led to a public questioning of the legitimacy of the IPCC reports.

From the discussion of the IPCC, we can conclude that the review procedures provide for a limited kind of extended peer review. The review process is extended, since indeed every expert who wants to participate in the review process can participate. However, the peer review is limited, since, ultimately, the lead authors and governments have the power to decide on the texts. Following Jasanoff’s argument against direct representation of interest groups in advisory committees, one could argue that the effectiveness of the IPCC in the policy making arena has been enhanced by its upholding of the authority of the lead authors.

### ***Knowledge Uncertainty***

How should the uncertainties we have to deal with in science for policy be categorized? Answering this question makes it possible to address the possible political meaning of these uncertainties in the context of the precautionary principle. In this section, I will use climate science as an example for introducing a typology of uncertainty. It must be stressed that there exists no typology of uncertainty which includes all of its features in a way that is clear, simple, and adequate for each potential use of such a typology. The typology introduced here reflects the major distinctions contained in several existing typologies (e.g., Funtowicz and Ravetz 1990; van der Sluijs 1995; van Asselt 2000), and is tailored to facilitate the analysis of science for policy based on computer simulation (the subject of my book).

First, since we are dealing with uncertainty that is communicated to policy makers, I distinguish three *ways to express* uncertainty in scientific claims (the list runs parallel to the three “sorts” of uncertainty distinguished by Funtowicz and Ravetz 1990):

1. by *presenting a range* in research results
2. by *characterizing the methodological acceptability* of research results
3. by *acknowledging ignorance* about the system studied

In the case of climate change, we can, for example, express uncertainty by *presenting a range* in expected global surface temperatures in 2100, as was done by the IPCC, which projected that temperature will increase by 1.4 to 5.8°C over the period 1990 to 2100 (IPCC 2001, 13). By presenting a range in results, we can express the uncertainty about the research results as “inexactness.” Both random and systematic errors can be cause results to be inexact (cf. Funtowicz and Ravetz 1990, who associated inexactness primarily with random error).

In addition to stating a range, we can qualify the results by *characterizing their methodological acceptability*. For instance, the IPCC wrote: “Confidence in the ability of models to project future climate has increased” (IPCC 2001, 9). Thus, the claims of the IPCC about future climate were characterized as being more reliable in 2001 as compared to five years earlier (when the *Climate Change 1995* report was published). The concept of “unreliability” can be used when assessing methodological acceptability (cf. Funtowicz and Ravetz 1990). A claim can be considered “reliable” if it has stood up against intense scrutiny of both the claim itself and the methods that were used to arrive at the results, and it can be considered “unreliable” if it can be severely criticized on factual or methodological grounds.

I will here briefly discuss how the IPCC dealt with the reliability of models. In the Third Assessment Report, the IPCC has used expert judgment to qualify the reliability of models, for example, the models which are used to attribute the observed increases in temperature over the last 50 years to different factors, such as anthropogenic greenhouse-gas emissions. In the summary for policymakers the main statement about the attribution to human factors was formulated as follows:

In the light of new evidence and taking into account the remaining uncertainties, most of the observed warming over the last 50 years is likely<sup>7</sup> to have been due to the increase in greenhouse gas concentrations. (IPCC 2001, 10)

Footnote seven is repeatedly used in the summary and reads as follows:

<sup>7</sup> In this Summary for Policymakers and in the Technical Summary, the following words have been used where appropriate to indicate judgmental estimates of confidence: *virtually certain* (greater than 99% chance that a result is true); *very likely* (90–99% chance); *likely* (66–90% chance); *medium likelihood* (33–66% chance); *unlikely* (10–33% chance); *very unlikely* (1–10% chance); *exceptionally unlikely* (less than 1% chance). The reader is referred to individual chapters for more details. (IPCC 2001, 2)

Both statistical considerations and judgments of model quality play a role in selecting a confidence category. For the specific claim on the attribution of climate change to human influences, the lead authors initially derived from their model exercises that the attribution claim was “very likely” to be true (based on a statistical analysis of the model results). Subsequently, in recognition of the unreliability of climate models, the lead authors decided to lower their estimate of confidence to the “likely” category. However, these considerations were left implicit in the text, and the judgment on model unreliability was not clearly communicated by the IPCC. By sticking to a one-dimensional scale of “judgmental estimates of confidence,” on which the dimensions of chance and methodological acceptability were collapsed, a systematic assessment of the unreliability of models was made difficult (only when discussing the different external factors forcing climate change the IPCC explicitly addressed the “level of scientific understanding” about each factor; IPCC 2001, 8).

The third way of expressing uncertainty is by *acknowledging ignorance* (related to the “border with ignorance” discussed by Funtowicz and Ravetz 1990). The category of “acknowledged ignorance” signifies the fact that experts making a claim may acknowledge that they are ignorant about specific sources of uncertainty. For instance, in the case of a complex system such as the climate system, it may be acknowledged that the system may show surprises of which one only has qualitative knowledge and which are not reflected in the models used. A measure of acknowledged ignorance is the “openness” of a claim, the presence of an explicit reflection that the experts are ignorant about certain sources of uncertainty. Closed claims, lacking such a reflection, convey an image of certainty about the results given in the claim. The openness of a claim can either be expressed in a general manner, such as in “surprises are not excluded” (while stating the rationale for this acknowledgment, stemming, for instance, from systems theory) or can be incorporated in the reference to particular sources of uncertainty about which one knows one is ignorant.

Having introduced the three ways in which we can express uncertainty, it is also useful to distinguish between different *sources* of uncertainty. Fundamentally, there is one basic source of uncertainty: *lack of knowledge* of how a system behaves. This lack of knowledge, in its turn, can stem from two different sources:

1. the way the system behaves (*ontic uncertainty*)
2. scientific limitations (*epistemic uncertainty*)

This is the basic distinction underlying many binary uncertainty typologies, such as “aleatory uncertainty” (due to chance) versus “epistemic uncertainty” (National Research Council 1996, 107), or “variability” versus “lack of knowledge” (e.g., van Asselt 2000, 85). Although the distinction is analytically sound, in scientific practice often mixtures of ontic and epistemic uncertainty are encountered (cf. Kirschenmann 2001, 4). For example, on the one hand, the fact that the weather is unpredictable stems from a fundamental property of chaotic systems (their “sensitive dependence on initial conditions”). On the other hand, this unpredictability stems from our limited knowledge of the initial state, the unreliability of numerical weather prediction models, and other scientific limitations. The reason why the distinction between

ontic and epistemic uncertainty is useful is that from both categories different conclusion follow with respect to the reducibility of uncertainty. Many ontic uncertainties are irreducible, while many epistemic uncertainties are reducible, although the match between the two distinctions (ontic–epistemic and irreducible–reducible) is not perfect.

<b>source</b>	<b>way of expression</b>	range in results	methodological acceptability	acknowledged ignorance
	<b>measure</b>	inexactness (exact–inexact)	unreliability (reliable–unreliable)	openness (closed–open)
input data	<ul style="list-style-type: none"> <li>system variability</li> <li>scientific limitations</li> </ul>			
model	<ul style="list-style-type: none"> <li>modeled system variability</li> <li>scientific limitations</li> </ul>			
software implementation				
treatment of output data				

Table 1: Typology of uncertainty in scientific claims based on computer simulation

Depending on the type of science which is considered, different categorizations of the sources of uncertainty can be more or less fruitful. Table 1 gives an example of a possible distinction among sources of uncertainty for sciences that are based on computer modeling (see van der Sluijs 1995 for a comparable two-dimensional typology). I will not discuss all the elements in detail here (therefore I again refer the reader to my book which is currently in preparation).

I conclude this section with some remarks on the question of how scientists should deal with uncertainty, especially when they act as advisers in science for policy. Since uncertainty is inherent in scientific practice, the answer to this question resides in the issue of quality assurance and quality control in the work of scientists. The basic idea here is that “high quality does not require the elimination of uncertainty, but rather its effective management” (Funtowicz and Ravetz 1990, 1). The precautionary principle thus not only requires us to be prudent in our potentially dangerous actions but we must also be prudent in

our scientific work, following guidelines on the management of uncertainty.<sup>3</sup> The management of uncertainty also involves the communication of uncertainty to policy makers and to the wider public. Here it must be recognized, however, that it is difficult to communicate uncertainty:

If [statements of different sorts of uncertainty] are expressed in mathematical form the statement is quite incomprehensible to the lay user; if in prose, it is tedious and unclear; and if they are omitted altogether, the advisor can be accused of conveying a certainty that is not warranted by the facts. (Funtowicz and Ravetz 1990, 12)

Facing this dilemma of uncertainty communication, we must try—especially when the uncertainties and stakes are high—to find uncertainty expressions which both match scientific practice and can be understood by lay people.

### ***The Precautionary Principle***

The precautionary principle constitutes an important element of the emerging international political order involving the environment. Before the principle rose to international prominence about fifteen years ago, it was largely left implicit in environmental policy making (only Germany had used a *Vorsorgeprinzip* since the 1970s). Still, precautionary measures had been taken for decades—also internationally. For instance, in the area of international pollution control, decisions to eliminate the release of certain substances into the environment were taken under conditions of large uncertainty. Also in policies on food safety, we can find early examples of precautionary action. One example from the U.S. is the “Delaney clause,” which made part of a 1958 amendment to the Food, Drug, and Cosmetic Act (1957–1996). The Delaney clause did not allow any food additive to be cleared for use if it were found, in studies on animals or humans, to induce cancer. U.S. Congress had decided that for a study to be admitted as evidence, it had to be deemed “appropriate.” The agency responsible for administering the clearing had some, but limited, discretion to determine whether a certain piece of scientific evidence should be considered “appropriate” (Jasanoff 1990, 42). The level of proof required for showing that an additive posed a danger to human health can be considered low in this case.

In general, determining whether or not precautionary action may be triggered will not be a straightforward technical exercise, given the multi-faceted nature of uncertainty and the political stakes. Typically, in regulatory science the level of uncertainty is high:

In spite of increased research, circumstances when risks to health or the environment can be established beyond doubt remain extraordinarily rare, and regulatory action still proceeds for the most part on the basis of indirect and uncertain evidence. Accordingly, knowledge . . . almost always has to be supplemented by discretion. (Jasanoff 1990, 42)

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<sup>3</sup> For this reason the Office for Environmental Assessment of RIVM is currently implementing “Guidelines for Uncertainty Management” (in Dutch: “Leidraad voor Omgaan met Onzekerheden”).

In the case of the Med Plan, for instance, government experts had to decide on the list of substances that should be banned, while they were confronted with considerable uncertainties in the field of ecotoxicology:

Scientists did not fully understand the way in which . . . pollutants were transmitted through the ecosystem, the related issues or economic sectors in which effects might be felt, or the long-term and possibly irreversible effects of pollution. The identity of individual pollutants is often not known or unclear. The behavior of specific substances in the environment is often only poorly understood. Projections are shaky at best. Tolerable environmental thresholds for pollutants are seldom known, and the dynamics by which pollutants are absorbed in the environment and are transferred to other media are often unclear. (Haas 1990, 30–31)

Reliable prediction of marine pollution (using, for instance, computer simulation)—and its ecological impacts—was not considered possible at the time and is still rare nowadays.

Still, when the Mediterranean governments were negotiating a protocol on pollution from land-based sources in the 1970s, the uncertainties in ecotoxicological knowledge did not hinder the experts involved in “black-listing” substances from making their decisions—which were partly based on what *was* known on toxicity, persistence, and potential for bioaccumulation (Haas 1990, 113). The precautionary approach which had earlier been followed by the EEC was adopted by all eighteen Mediterranean countries involved in the Med Plan and some controls and ambient standards even exceeded those contained in the earlier EEC directive (Haas 1990, 116). However, no explicit political discussion of the uncertainties had taken place and the essentially political decisions on which substances should be black-listed had just been deferred to the experts. Therefore, the plan did not yet amount to the explicit adoption of the precautionary principle in environmental matters, which only happened internationally in the 1980s.

Since it had arrived on the international scene, the precautionary principle has received dozens of formulations. According to some analysts, even hundreds of formulations are possible. Still, all formulations share some basic elements. First, the precautionary principle is supposed to be about the following:

for a given *activity* that may have a given *effect* on [humans or] the environment, the precautionary principle is supposed to indicate a *remedy*. (Manson 2002, 265)

Furthermore, all formulations of the precautionary principle found in legal texts share a common three-part structure: in any formulation, a *damage condition*, a *knowledge condition*, and a *remedy* can be identified (Manson 2002, 265–266). The damage condition and the knowledge condition together provide the “triggers” for the application of the principle. There is considerable disagreement in the literature on whether the precautionary principle also implies the proportionality principle (i.e., that measures should be cost effective), and thus whether the precautionary principle must be seen as separate from cost-benefit analysis or as itself a special kind of cost-benefit analysis in which broader consideration is given to pros

and cons. Usually both elements—precaution towards humans and the environment, on the one hand, and cost-effectiveness, on the other hand—are included in the legal texts, without clearly defining what part of the texts actually constitutes the “precautionary principle.” In practice, precautionary measures cannot be expected to be taken without considering issues of proportionality.

Often the formulation of the knowledge condition (the level of proof needed to trigger the application of the principle) is kept vague. For example, the text of Article 3(3) the United Nations Framework Convention on Climate Change (1992) reads as follows:

The Parties should take precautionary measures to anticipate, prevent or minimize the causes of climate change and mitigate its adverse effects. Where there are threats of serious or irreversible damage, lack of full scientific certainty should not be used as a reason for postponing such measures, taking into account that policies and measures to deal with climate change should be cost-effective so as to ensure global benefits at the lowest possible cost. (United Nations 1992)

The phrase “lack of full scientific certainty” leaves open what level of certainty is needed to trigger obligatory action.

The question on the level of proof is important, since its answer has significant implications. In more recent formulations of the precautionary principle, we often find more specific knowledge conditions. For instance, according to the Communication on the Precautionary Principle published by the European Commission published in 2000, what is needed for triggering application of the precautionary principle are

indications through preliminary objective scientific evaluation that there are reasonable grounds for concern that the potentially dangerous effects on the environment, human, animal or plant health may be inconsistent with the chosen level of protection. (Commission of the European Communities 2000)

Now, how do we determine whether a scientific evaluation is “objective”? And what counts as “reasonable” here? To be able to make these determinations, we will need guidelines which enable us to characterize the uncertainties involved in scientific claims. We also need objective institutions—that is, institutions operating relatively independently from both the executive branch of government and special-interests actors—which can be charged with the task of applying these guidelines.

A recent report from the European Environment Agency (EEA), titled *Late Lessons from Early Warnings: The Precautionary Principle 1896–2000*, concludes on the issue of the level of proof:

Choosing which level [of proof] to use in particular situations involves a decision that can radically shift the size, nature and distribution of the costs of being wrong. This is a key political decision with profound ethical implications. The level of proof that is appropriate for particular issues depends upon the size and nature of the potential harm,

the claimed benefits, the available alternatives, and the potential costs of being wrong in both directions, i.e. of acting or not acting in the context of uncertainty, ignorance and high stakes. (Harremoës et al. 2001, 193).

The search for one single formulation of the precautionary principle is thus futile, because the level of proof that is demanded by the knowledge condition may vary depending on the specific type of problem under consideration. If we would accept a level of proof which is too low for a certain type of problem, we run the risk of unacceptably large negative consequences when—with the benefit of hindsight—it turns out that there had not been a real problem.

Scientific advice must therefore be given responsibly. Now, whose responsibility is it when scientific advice that is used to trigger the precautionary principle turns out to have been wrong? For scientists there has always existed a certain “right to be wrong,” since uncertainty is the driving force of science. Could we extend this right from scientific research to scientific advice (as was proposed by Fuller 2000, 151–155)? Again, the issue here boils down to one of quality. As long as “negotiated standards of adequate scientific performance” which are valid across the disciplines are adhered to, it seems that scientific advisers should not be penalized for being wrong (cf. Fuller 2000, 153). Fuller offers a bold proposal for universities to remedy the negative consequences of bad advice given by academic advisers:

the university [could] engage in some creative cost-accounting of consultancies done under its auspices, specifically by demanding overheads that are then ploughed back into a fund to compensate the victims of poor policy advice. In short, the university would function as an ‘epistemic insurance agency’. The resulting higher consulting charges might influence the nature of the advice given and taken. (Fuller 2000, 153)

One can indeed wonder which kind of advice academic advisers would still be willing to give under such a strict liability regime. The specific proposal made by Fuller might be unrealistic, but even if we use it as a thought experiment, it must become clear how important the issue of quality assurance and quality control is when advice is being given in high-stake situations.

We now come to the final question of this paper: what do the various types of uncertainty imply for the application of the precautionary principle? The answer depends on how the required level of proof is formulated and which types of remedies are being considered. Typically, a characterization of the methodological acceptability of a claim will be used to determine whether the claim is reliable enough to trigger action. Another trigger—resulting in different types of action—can be the acknowledgment of ignorance. It is useful here to follow the EEA report and distinguish between the categories of risk, uncertainty, and ignorance (cf. Wynne 1992). In the EEA report the “risk” category denotes *known risks* (i.e., the “classical” meaning of “risk”), for which we do not need a precautionary principle, since the uncertainty about the risks is assumed to be small. We do need the precautionary principle to deal with *uncertain risks* (i.e., the “new” and broader meaning of “risk”). The EEA report gives examples of preventive and precautionary action for each of the three mentioned categories (see Table 2).

Situation	State and dates of knowledge	Examples of action
<b>Risk</b>	‘Known’ impacts; ‘known probabilities,’ e.g. asbestos causing respiratory disease, lung and mesothelioma cancer, 1965–present	<b>Prevention:</b> action taken to reduce known risks, e.g. eliminate exposure to asbestos dust
<b>Uncertainty</b>	‘Known’ impacts; ‘unknown’ probabilities, e.g. antibiotics in animal feed and associated human resistance to those antibiotics, 1969–present	<b>Precautionary prevention:</b> action taken to reduce potential hazards, e.g. reduce/eliminate human exposure to antibiotics in animal feed
<b>Ignorance</b>	‘Unknown’ impacts and therefore ‘unknown’ probabilities, e.g. the ‘surprises’ of chlorofluorocarbons (CFCs) and ozone layer damage prior to 1974; asbestos mesothelioma cancer prior to 1959	<b>Precaution:</b> action to anticipate, identify and reduce the impact of ‘surprises,’ e.g. use of properties of chemicals such as persistence or bioaccumulation as ‘predictors’ of potential harm; use of the broadest possible sources of information, including long-term monitoring; promotion of robust, diverse and adaptable technologies and social arrangements to meet needs, with fewer technological ‘monopolies’ such as asbestos and CFCs

Table 2: Uncertainty and precaution—towards a clarification of terms (source: EEA)

Note that the “uncertainty” category of the EEA report is a bit narrowly defined. It corresponds roughly to a situation in which results are deemed reliable (e.g., we do know that a certain causal link exists), but inexact (e.g., we cannot make exact estimates of the effect). In principle, precautionary prevention might also be triggered for results which are considered less reliable (but still being of a quality that exceeds a certain threshold). The impacts of actions can then be called “uncertain.” Finally, the EEA report emphasizes the importance of acknowledging ignorance. Societies must learn to think from the “border of ignorance.” For scientific advisers the political relevance of ignorance implies that they must clearly express uncertainty also by acknowledging ignorance.

To conclude, for a knowledge-and-uncertainty broker, such as the Office for Environmental Assessment of RIVM, the increasing importance of the precautionary principle in policy making implies that the assessment and communication of uncertainty should be considered a top priority. This should not be an exercise for experts only, however. Based on the earlier discussion on science for policy, we can add that societal stakeholders should participate in assessing the uncertainty of scientific results, and that “full account [must be taken] of the assumptions and values of different social groups” (lesson nine of the EEA report).

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