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**Clearly Recognized and Suitably Porous:
A Review of the Literature on the
Science-Policy Interface
in International Environmental Management**

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Executive Summary

This document's purpose is to review the use of scientific advice by international environmental management regimes in hopes of identifying lessons that will be of use to the Common Fisheries Policy.

The attributes that make a set of scientific findings useful for policy are saliency, credibility and legitimacy:

"Saliency reflects whether an actor perceives the assessment to be addressing questions relevant to their policy or behavioural choices;

Credibility reflects whether an actor perceives the assessment's arguments to meet standards of scientific plausibility and technical adequacy; and

Legitimacy reflects whether an actor perceives the assessment as unbiased and meeting standards of political fairness."

There are often trade-offs between these attributes and efforts to bolster one often only succeed at the expense of another. The main differences among science policy institutions are how they shape the tradeoffs among saliency, credibility and legitimacy.

The boundary between what is science and what is not science must be recognized and respected. It cannot be treated naively, neither by assuming that the distinction between what is science and what is policy, advocacy or values is easily made in concrete situations, nor by assuming that it does not really exist. Well designed boundary spanning arrangements are the most essential tool for maintaining salience, credibility and legitimacy. Two such arrangements are boundary organizations, where scientists and other stakeholders are able to interact, and boundary objects. Examples of such objects would be models, indicators, collaborative research designs and data collection efforts that are used to provide a way to structure discussions.

The review examines three established theories of the science-policy interface. Epistemic communities are a particular type of policy network that is characterised by scientific consensus. The Epistemic Community approach posits an ideal situation: a strong consensus among scientists reflecting truth about nature that has clear normative implications and policy alternatives that all of the scientists can gather around. This describes what has happened in several successfully negotiated environmental protection regimes. These examples, while few, do show that success is possible and provide a set of experiences of success from which lessons can be drawn. The weakness of the approach is that it does not address the majority of environmental problems where uncertainty is high and consensus difficult to obtain.

The second well established theory Post-Normal Science (PNS), which restricts itself to high uncertainty / high stakes policy arenas. A central concept is the "extended peer community" where an open dialogue is required because the quality of the science depends on an "extended peer review". The other important concept is that within the high stakes, high uncertainty context, it is scientific skills rather than scientific knowledge that becomes more important. This means that scientists must act more like consultants than traditional scientists to help address high stakes uncertainty.

The third theory is Mode Two Science. The argument here is that science is shifting from a search for truth to a more pragmatic aim of providing a provisional empirical understanding of the world that works in a practical sense. This has forced science, particularly though the way it is financed, to

become less privileged, more flexible and less disciplinary. In this new way of producing knowledge quality control based on results is replaced with quality control based on procedures and processes. Such quality control is more dependent on non-scientific mechanisms.

The majority of ways to help maintain credibility, legitimacy and salience in situations of high uncertainty allow scientists to work closely with non-scientists without sacrificing legitimacy and credibility. One such mechanism is “polycentric networks”. These are multiple connections between researchers and decision makers which cut across various political and organizational levels. They are able provide methodological coherence across scale levels while still allowing local specialization and provide for multiple pathways to encourage innovation and flexibility. A polycentric network also facilitates stakeholder capacity building and involvement. It is an institutional design that gives form to the consultant-like relationships envisioned in Post-Normal Science while mobilizing transparency and the inclusion of multiple interests to keep these relationships from being so close legitimacy and credibility are impaired. The multi-scale level nature of the problems that science-based policy must address also suggests a polycentric approach.

The review concludes with a discussion of four principles distilled from a number of the studies. The first principle is built in evaluation and reflection. Many potential problems can be anticipated even in conditions of uncertainty and the taking tentative, reversible action is important. A close relationship between science and policy facilitates the treatment of management measures as tentative tests of hypotheses about the social and natural environment.

The second principle is to search for consensus. The examples that are the basis of the Epistemic Community approach show that where consensus is possible it is a powerful tool for effective policy. Boundary spanning arrangements should facilitate a consensus among scientists and decision makers over the questions to be addressed, the evidence and expertise needs, and the processes that scientific assessments employ. However, many scholars warn against too strong an emphasis of consensus. We must not overemphasize the finding of common ground or create a situation in which people feel it is simply in their best interest to follow the consensus.

The third principle is participation. How to involve stakeholders in science-based policy remains a challenge. The reason for doing so is the legitimacy that can be gained though public buy-it, and in some cases such buy-in can be critical to outcomes. In the past the public has often been approached ineffectively. Much of the literature exaggerates or mischaracterizes the public’s lack of knowledge. Almost all of it misses the importance of the public’s own experience-based knowledge in arenas such as fisheries.

The fourth principle is transparency, which becomes critical where uncertainty is high because of the need for openness about the limits of scientific knowledge. Transparency, in the form of involving several parties with different interests in the “consultancy” relationship, seems to be the only way to gain the advantages in terms of saliency and uncertainty while maintaining legitimacy and credibility. However, when transparency is poorly structured, especially with formal “openness” requirements, it can reduce legitimacy.

This reviewed contains many possible lessons for the CFP and a tentative list if offered in the conclusion. Some highlights in include the ways that interactions between and among scientists and RACs are already creating ad-hoc working groups of various sizes that take the form of a polycentric network and perhaps they should be encouraged and supported. The nature of fisheries means that it is easier than in many arenas to develop boundary objects and this is happening rapidly through collaborative research, developing scenario-based modeling. New kinds of boundary organizations such as the North Sea Commission Fisheries Partnership are also possible.

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1 Introduction

This document is Deliverable One of the Scientific Advice for Fisheries Management at Multiple Scales (SAFMAMS) Project. Its purpose is to review the use of scientific advice by international environmental management regimes in hopes of identifying lessons that will be of use to the European Union as it seeks to manage European fisheries under the Common Fisheries Policy (CFP). The research area reviewed here is still a fairly recent one. All its major theoretical approaches have their genesis in the 1980s and even the early 1990s. While a number of tentative lessons have emerged there are few undisputed conclusions. The test of the usefulness of this material for fisheries will lie mainly in the degree to which fisheries professionals find it coherent and reflective of their own experiences.

A good question to begin with is if scientific advice matters at all in international relations. Is it true, as Haas (2004) claims, that “knowledge can speak volumes to power” (587)? It is certainly a plausible hypothesis that international institutions are driven by power politics reflecting national interests and that science’s real role is that of a legitimation device for decisions based on other factors. The question “is it science or is it power that guides the international environmental efforts” is a false question, however, because it misses the internal connection between knowledge and power. Knowledge, of course, is a source of power for those who have it. Furthermore, as game theory has shown for competitive situations in general, all competition assume some kind of common knowledge among the players and uncertainty leads to sub-optimal results for all (Sebenius 1992).

Clark et al.’s (2006) review of international scientific assessments does conclude that the majority of the assessments are not effective in influencing policy, and even the most influential assessments do not directly determine policy choices. What they do do, however, is influence the direction of the long term development of an issue through such mechanisms as influencing the issue’s visibility, the stakeholders who will take an interest, the way questions and objectives are framed, and the selection of management alternatives (see also van der Hove 2007). These mechanisms add up to a considerable long term influence on the outcomes of the international political games. Hence, an approach to understanding international policy that examines the creation of common understandings within a political process is more instructive than simply examining issues of power and interests (Haas 2004).

This influence of science on policy is reflected back on science through the influences of the policy process on scientists and their work. “Negotiated science”, where debates about the credibility and uncertainty of results form an important part of the negotiations is a very common feature of diplomacy related to environmental issues (Backstrand 2004). As Young (2004) argues international institutions have a strong influence on the topics being studied, even to the extent of influencing the kinds of models being created. Policy influences science through funding decisions, changes in standards of quality control and the validation of scientific institutions (van der Hove 2007). These considerations mean that scientists deciding what directions to take in the creation of new knowledge are strongly influenced by their experiences with what has proved useful and interesting to policy makers in the past. Desire for policy influence can also leads to downplaying disagreements in the search for consensus, in spite of the fact that “within reason, disagreement is a positive force in the scientific world” (Young 2004:223). The plethora of scientific assessments aimed at influencing policy also use tremendous resources to essentially collate existing knowledge, resources that are very likely deflected from creating new knowledge (Young 2004).

Among those convinced of the importance of science within the discipline of international relations, two approaches to understanding the use of expertise in environmental governance have commonly been employed (Backstrand 2004). One is an institutional approach, exemplified by the work of

Oran Young and, especially, Peter Haas. The institutional approach focuses on the institutional conditions for the uptake of scientific knowledge in international environmental management. A key emphasis in this approach is the importance of scientific consensus. The second, which Backstrand (2004) terms “constructivist” focuses on how the usefulness of scientific advice is built and emphasizes the three dimensions of salience, credibility and legitimacy. One of the most useful examples is the Global Environmental Assessment Project carried out under the leadership of the Kennedy School. This was five year comparative research project on the policy uptake of results from global scientific assessments (Cash and Clark 2001, Cash et al. 2002, Clark et al. 2002, Clark et al. 2006). Examining both of these approaches plays an important part in this review.

These approaches need to be supplemented, however, with relevant approaches in science and technology studies that give greater emphasis to differences in scientific practice and working conditions. This is particularly true of the institutionalist perspective. Consensus is the basis of Haas’ notion of the “Epistemic Community”, which is discussed at length below. Haas (2004) argues that when there is no consensus the scientific and policy developments must be kept separate from one another until consensus is achieved. Throughout Haas’ work are found convincing examples of the ways that scientific communities that have reached agreement on policy directions are able to wield a great deal of influence on policy outcomes. However, he fails to give sufficient attention to how scientific communities operate in the much more common situation in which scientific consensus is patchy and incomplete. An important task in reviewing this literature is to compare Haas’ approach to two other important perspectives on science policy communities that are found outside of the international relations literature. These are the notion of “Mode Two” science (Gibbons et al. 1994) and “Post-Normal Science” (Funtowicz and Ravetz 1992).

While Haas and these other theorists of policy communities give us competing pictures of the structure of science and policy networks, the group that Backstrand (2004) terms the “constructivists” gives us an equally important tool for understanding the attributes that make a set of scientific findings useful for policy: saliency, credibility and legitimacy. Their empirical work on scientific assessments and their influence on policy outcome suggest that these are the three main attributes that determine such influence. Clark et al. (2002 p7) define these three as follows:

“Saliency reflects whether an actor perceives the assessment to be addressing questions relevant to their policy or behavioural choices;

Credibility reflects whether an actor perceives the assessment's arguments to meet standards of scientific plausibility and technical adequacy; and

Legitimacy reflects whether an actor perceives the assessment as unbiased and meeting standards of political fairness.”

All three attributes are needed in some if such influence is to be found. The difficulty, Clark et al. (2002) argue is that there are trade-offs between the three dimensions. Indeed they suggest that efforts to bolster one often succeed at the expense of another. For example, efforts to increase saliency by narrowing the questions to be investigated can decrease legitimacy by making the process appear to have a political bias. Conversely, adding stakeholders to increase legitimacy can reduce saliency as issues are raised that are outside of policy maker’s remit. While these relationships may usually involve trade-offs, they can also lead to mutual reinforcement and complementarities. For example an effort to increase credibility by including new knowledge may also increase salience and legitimacy. These observations form the basis of their empirical investigations of science policy institutions, suggesting that their main differences are in the ways that they shape and balance the tradeoffs among saliency, credibility and legitimacy.

The institutionalist and constructivist approaches from international studies, and related observations from science and technology studies, provide central themes for this review. It is broken into two main parts.

The first part addresses the boundary around what is and is not science. It begins with an examination, from a sociological perspective, of some of the underlying tensions among science, society, and policy. Then after returning again briefly to the three dimensions of salience, credibility and legitimacy, the discussion turns to the “fly in the ointment”: the nature and implications for policy of scientific uncertainty. These two sections sets the stage for a more in-depth discussion of the science boundary in three concluding sub-sections. The main content of the literature reviewed in this first part is that the key to effective science policy in the midst of uncertainty, i.e. a well balanced support for the salience, credibility and legitimacy of science, is to be found in the question that the “constructivists” such as Haas have already introduced us to: what are the practices and understandings that make up the boundary between policy and science? To a greater and greater degree, rather than functioning as two separate arenas, scientists and policy makers are “co-producing” science and policy.

The second part examines the theory and practice of this co-production of science and policy. It reviews three different approaches to this boundary - the Epistemic Community, Post-Normal Science and Mode Two science approaches mentioned above. This section is brought to a close with a discussion of the practice of co-production and an attempt to distil some of the practical lessons offered by the various scholars that have contributed to this literature.

2 The Science Boundary

2.1 Underlying Tensions among Science, Society and Policy

Underneath the day-to-day governance questions we try to understand with our “what works and what doesn’t” observations, culture and science are bound together in ways we are often barely conscious of. While the more radical reflections on science and society may not have much practical use, they remind us of weight of the cultural baggage on the science we are trying to harness to practical ends. Perhaps the most illuminating of these radical reflections come from of the Frankfurt School, a group whose work blended sociology and philosophy in trying to explain the rise of fascism. For them, science was the ultimate expression of “instrumental rationality”, i.e. rationality focussed on achieving ends. Their concern was that validity based on instrumental rationality was pushing out claims to validity based on non-instrumental values. Science, and instrumental rationality in general, changes everything into representatives of a category, removing their individual concrete reality where practical and moral reasoning operates. Instrumental rationality applied to society, as in some forms of social science, makes social relationships appear as natural and inevitable rather than as the concrete results of real decisions made by real people who could have chosen another route. Habermas (1984) warns against a tendency, routed in a need for control, to try to redefine culture phenomena into technical ones.

Science is powerful rhetoric. Hard, technical arguments are more convincing than soft appeals to values. Pelletier et al. (2000) offer a study where they did intensive ex-ante and ex-post attitude measurements of stakeholders involved in a participatory action conference. Shifts in attitude as a result of the discussion were much stronger in response to technical arguments than value-based arguments. Credible science can shift the discussion even in the most political of fora. Lenhard et al. (2006) give as an example an argument around the Intergovernmental Panel on Climate Change’s Second Assessment Report during which sceptics from political wing of OPEC withdrew their veto in response to climatologists who were able to show that they were using the excepted methods and standards of their field.

These observations suggest that the common scepticism about the inability of science to influence policy is exaggerated, but they also suggest the danger of misuse. Irwin (1995) citing Ulrich Beck, suggests that the presentation of something as “science” can and is used to silence rather than address people’s concerns. Again, the more radical arguments illuminate the importance of this danger. Herbert Marcuse (1964) points out that instrumental rationality is built into the very heart of the natural science, because the method of hypothesis and experiment consists of prediction and manipulation, and hence the domination of nature; science is inescapably linked to control. These concerns are echoed in the so-called “biocentric” critique of environmental management that questions whether humans should be trying to dominate and control nature at all (Ehrenfeld 1981, Stanley 1995). Those of us interested in managing fisheries can and should respond “of course we want to dominate nature, there are people to be fed!” These scholars do remind us, however, that suggesting that something is the appropriate object of a scientific analysis casts that object into a particular and subordinate cultural role. This casting can and is done inappropriately to the detriment of both science and policy.

The temptation to try to change political, social or cultural phenomena into technical ones is always present. Environmental management requires us to address these things and so we search for *techniques* to do so. Management involves manipulation and it is out of this tension that the question of governance arises when democratic societies seek to address social and environmental problems. This problem is exacerbated when actors seek to obscure rather than clarify the distinction between technical and cultural phenomena. The usual motivation for this is to make a policy choice appear as a technical necessity (Wilson and Hegland 2005). Scholars contribute to this obscurantism as well, arguing, e.g., that society and technology are not two ontologically distinct entities but more like phases of the same essential action (Latour 1991). In some very extreme examples of “Actor Network Theory” an equation is made between society and nature as such, e.g. Callon (1986) argues that the fish in a research sample should be thought of as a social actor. These sorts of statements equating society and technology are usually illustrated by relatively benign examples pointing out how technology expresses society, rather than using the illustrations of society being reduced to technology that their theoretical assertions are just as clearly suggesting. It is the hiding of social relations behind a technical claims that is the real problem. As one scientist involved in environmental management put it “If....a manager suggests that a decision is based solely on scientifically-derived biological considerations, the manager either misunderstands the nature of science....or is deliberately trying to disguise....a value judgement” (Decker et al. 1991 quoted in Minnia and McPeake 2001).

Cultural and technical phenomena are always linked but this does not vitiate the critical distinction that Habermas and the other Frankfurt School scholars point us toward: some phenomena are expressions of matter and energy and governed by the laws of thermodynamics and some phenomena are expressions of meaning. To treat one of these basic categories as if were the other leads to illusion, and to poor governance. As a basis for policy, it raises a real danger of backfiring and making the problem worse. As Evans argues natural science certainly draws on “elements of theatre, politics and/or rhetoric, but it is not reducible to any of them” (2005:11). Reducing the manipulation of the symbolic aspects of science requires us to put greater weight on methods, expertise, and the appropriate selection of objects of study, leaving science in “a delimited but still privileged role for science in society” (Evans 2005:13).

Closely linked to the cultural role of science is the cultural role of quantification. The basic importance of quantification for science has been long recognized. Kant (1783) argued that judgements of experience are always synthesized out of individual observations, while mathematical judgements are *a priori* and not based on experience. Quantitative methods are the paradigmatic form of science because they tap into the power of these *a priori* judgements.

Quantitative methods begin with in the accurate and consistent measurement of comparable units. When meaningful comparability and accurate measurement can be achieved mathematical laws are mapped onto the phenomenon under study. Disputes must focus either on the comparability or the measurement of the units. This mapping of mathematical laws therefore attains the greatest possible transparency of argument. It is this transparency that makes quantitative methods the ideal of science because science based in the ability of someone to explain how they know something, in principle with such clarity that others will make exactly the same discovery if they repeat the process described.

Understanding that quantification is basically about transparency also illuminates its importance in respect to policy and governance as well as science. Porter (1995) links the rising importance of quantification to democracy. When decisions are made by bureaucrats and politicians that are open to public criticism, rather than by aristocrats who stand above such criticism, they need to be able to provide objective, transparent demonstrations of the basis of their decisions. The rule of law cannot rely on aristocratic judgement (Porter 1995).

Arithmetical language actually has a great deal in common with ordinary, informal language (Funtowicz and Ravetz 1990). The formality of the mathematical language is lost when expressed in normal speech and numbers are used in ordinary speech to mean very different things. This informality, however, is often obscured and the aura of formality remains. In this boundary area between the formal numbers of arithmetical language and the informal numbers of every day speech new ways of thinking about things emerge. This can be clearly seen when things are measured and then these measured things are debated and discussed. In some ways the measured things are wholly new phenomena. Porter (1995) argues that the concept of "society" itself was in part a statistical construct and that people didn't really talk about "society" before there were statistical handles to do so. Crime rates, unemployment rates made it possible to talk about a society involving collective responsibility instead of just the condition of individuals. The measured items often evolve into standardized categories where individual variation is obscured. Porter (1995:28) quotes an official of the US Bureau of Standards: "We have now reached the stage where there is a federally mandated method for measuring almost every physical, chemical or biological phenomenon". Accommodating variation in measurement practices is impossible for a bureaucracy, even improvements are unhelpful unless made universal.

This combination of the hidden informality of numbers in speech and the ways that measurement transforms and invents phenomena has problematic consequences. A large part of the literature on local knowledge points to communication breakdowns that emerge when the day-to-day experiences of user groups are presented to them in the form of unfamiliar numerical categories. Roepstorff (2000) in his study of fisheries science in Greenland suggests that fishers "focus on fish as a living being" and think of them as "mass nouns" while the scientist sees the fish as a "count noun," meaning that the individual fish is a representative of the stock in the sense that the stock is the arithmetic sum of the single fish. These empirical findings echo the Frankfurt School concern noted above about the reduction of concrete phenomena to pure representations of categories.

Just as in daily speech numbers lose their precision while maintaining their aura, the quantitative presentation of material may also obscure underlying uncertainty. Mathematical models have been called a "technology of hubris" that suggest that managed use is feasible where precaution might be the wiser choice. They overstate the known and downplay ignorance, uncertainty and conflict (Harremoes et al. 2001, Jasanoff 2002). In many policy contexts some stakeholders will be distrustful of the models at the same time that other are praising them (Jasanoff 1986).

Beyond these dangers of what might be called an "overly scientific" approach to policy, policy making places demands on science and scientists that challenge their modes of operation. Policy processes use different standards of evidence and burden of proof than science. Scientists want to

see that there is a small probability of a null hypothesis, while policy makers are more concerned with the costs of being wrong (Kinzig et al. 2003). As an exaggerated but illustrative example, a policy maker would hardly want to reject a potential cure for cancer because of a less than 95% chance that it might work. Managers employ scientists, but they see things through different cultural glasses. Cullen (1990) reports his observations of interactions between water use planners and limnologists. He observed that managers viewed limnology as a curative practice while scientist saw it as preventative. Planners saw scientists as unable to agree on a conclusion and driven by a need to publish rather than getting a planning process done. They also thought the scientists were poor communicators. The scientists felt isolated and underutilised, they saw the planners as unable to interpret data or even find information, indeed as so ignorant that they did not know what they didn't know. They also thought the planners were poor communicators.

Salter (1988) explores in some detail what happens to scientific practice when it is mandated as part of a policy process. The burden is placed on the scientist to produce work that is sufficiently credible, salient and legitimate to support the policy. Policy makers want science that is intelligible to non-scientific audiences, and in doing so represents a clear body of evidence and appears to be rational. The ideal it is meant to project is that of something free of value judgments, using clear methods that produce credible results. At best it is characterized by open debate, anonymous peer review, and academic publication. In addition to projecting this sterling public image, it must present the policy makers with clear policy choices. Of course, as Salter points out, real science in support of policy conforms to none of these ideals. It makes moral dilemmas explicit, produces conflicting results that cannot be resolved by further studies, and is often seen as corrupt.

Where these differences really come to the fore is when science is drawn into legal proceedings. As Smith and Wynne (1989) argue, legal institutions have their own ways of defining what counts as a fact and they are not the ways found in science. In court it is the law that decides what the factual question is that the scientist must answer. They further argue that 'adequate evidence' is fundamentally problematic in courts because of the unremitting skepticism. In Latour's (1987) terminology, opposing lawyers always push scientific facts back towards the conditions of their production and expose the role that scientists' tacit assumptions, experimental skills, and professional judgements play in the production of knowledge. Within the scientific community these perfectly normal aspects of scientific work are handled by expectations of trust in intellectual integrity and the resulting importance of scientific reputations (Barnes et al. 1996). In court they can be portrayed by opposing council as simply unprofessional. Legal processes undermine the value system of the scientific culture (Wynne 1989a). One result has been the emergence of the "professional" expert science witnesses. For these people the translation of scientific facts into legal facts is seen as a normal part of professional culture, but this not the way many other scientist see the matter.

Some studies can give us some insight, directly or indirectly, into scientists' reaction to these tensions between science and policy. One interesting example is Dietz et al. (1989) who looked at how different participants in disputes over environmental disputes characterized what the dispute was really about. The four main characterisations that emerged were that the underlying problem was driven by a) different levels of knowledge, b) the vested interests of the parties c) disagreements over values and d) a mistrust of expert knowledge. They examined the roles of both professional identity and institutional affiliation in determining which characterization was more likely to be adopted and found that affiliation was the stronger of the two. They found that each group defined the dispute in a way that their particular resources were the best tool to resolve. For the natural scientists that meant adopting the different levels of knowledge characterization and rejecting the idea that the problem was differences in values. Cohen et al. (2001) did 55 semi-structured interviews with scientists working in eight public sector research institutions in the UK.

A majority of their respondents objected to the idea of being accountable to politicians and managers whose purposes they saw as being at odds with science.

Other researchers point to a sort of “devil’s pact” between scientists and policy makers in respect to reactions to uncertainty. In respect to global climate change, for example, space agencies are in search of new tasks while governments wanted to postpone action and therefore welcomed uncertainties. Governments want to fund more research rather than change energy policies; scientists are happy to play up uncertainties to keep funding coming. The making of policy more science dependent reflects a convergence of interests between opponents to policy change and the scientific bureaucracy (Boehmer-Christiansen 1994). Shackley and Wynne (1996) also argue that policy makers like ambiguity because it does not challenge their authority. They use the reduction of uncertainty as a rhetorical device to proceed with current knowledge while the scientists use it to get both get funding and because areas on policy uncertainty are also areas where they find interesting research questions.

2.2 Credibility, Legitimacy and Salience

2.2.1 Perceptions of Credibility, Legitimacy and Salience

The attributes that Clark et al. (2002) offer as a basis for the analysis of science for international policy support also emerge in other research on science and society. Risk studies have shown that people care more about how decisions are made and their fairness than they do about the magnitude of a risk (Chess and Lynn 1996). The perceived salience of an issue, and related scientific communications, also emerges as important in research on perceptions of environmental risk.

These or similar categories emerge in various ways in studies of the perception of scientific information in local contexts. It is these local contexts, rather than in abstract ideas about science, that provide the tools that people use to make sense of the information (Irwin 1995). Michael (1996), for example, did a series of interviews with the general public about radon. They were interested in how people understood the categories of “ignorance” and “expertise”. They found that three kinds of responses were the most common and these three corresponded roughly to credibility, legitimacy and salience. One was the basic, unsurprising idea of that some people know more than others and the experts should be listened to. The second was the notion that understanding radon is 'not my job' suggesting an understanding of ignorance and expertise that was based on a legitimate division of labour. The third main response was in terms of salience, the information about radon was simply not interesting to them. Yearley (1999) did a series of focus groups with different stakeholder groups around air pollution models being used by a local government agency. They found three factors influenced the public understanding of the model's output. The first two corresponded to legitimacy and credibility. The first was the respondents’ assessment of the trustworthiness and agenda of the agency and the second was their confidence in their own technical knowledge and ability to judge the technical merits of the model. The third was also related to credibility and reflected the importance that people attach to direct experience: they judged the models in terms of their evaluation of the assumptions about social behaviour which underlay them. All three of these factors were more important than the 'face credibility' of the models based on realistic looking simulations and projections.

Legitimacy and source are linked in ways that are filtered through local experience. People dealing directly with the public have completely different ideas than scientists about what makes a communication credible. Chess et al. (1995) interviews 145 risk communication researchers and practitioners about what scientists and people dealing with policy thought was important in the presentation of risk information. The technical people saw the question of how to express probabilities and communicating evidence to back of their assertions as the heart of the problem.

The practitioners were much more concerned with how to integrating political and value based concerns into the policy responses.

2.2.2 Credibility, Legitimacy and the Source of the Information

Credibility and legitimacy are both closely tied to the source of the information. This is not surprising when we consider what might be the most basic observation from the sociology of knowledge: the social location of facts is what determines their effective validity. For example, as Collins and Pinch (1998) point out, you and I base our personal knowledge about nature, that it is not possible to travel faster than light, entirely on our personal knowledge about society, that such information resides with physicists rather than Star Trek scriptwriters. One of the corollaries of this observation, which has been well documented in research, is that in the event people do have personal experience of a phenomena they give that experience extremely high weight in comparison to information that is merely communicated.

Credibility is tied to the source of the information most often by questions about the interests of the researcher. Science as an abstract category is often seen as credible but seen as a different category from the technical advice that is encountered frequently. The technical advice is seen as co-opted. In fact the notion that science is an expression of political interests is a form of “common sense” among much of the general population, claims of factuality and objectivity, rather than increasing credibility, are seen as rhetorical devices (Irwin 1995).

This common sense has a factual basis. It is becoming increasingly common for scientists to have direct financial stakes in outcomes (Guston 2001). Comparative, statistical studies of the outcomes of drug trials have found that drug company funding has a strong influence on bias result from drug company funding: 89% of company funded studies showed new drugs more effective compared to 61% of those not company funded. Moreover, the authorship of articles in major medical journals is routinely hidden, one study found that 29% of articles used such devices as guest authors or ghost authors. Accusations of inadequate safety checks in using human subjects in research are also increasing (Guston 2001).

Frewer et al. (1996) did a series of semi-structured interviews related to the source of information about environmental risks. They found that trusted sources are characterized by multiple positive attributes. Interestingly, while observers of policy process often give high weight to “independent” sources of information, the people interviewed in this study found that sources which operate under moderate accountability, rather than complete freedom, are more trusted. Television current affairs programs and quality newspapers are among the most trusted with the tabloids and government the least trusted. McKechnie (1996) in field work in a rural area found that sources are seen as credible only if their self- presentation is consistent with local values and that assertions about having esoteric knowledge actually undercuts credibility as the source is seen to lack “common sense”.

2.2.3 Perceptions of Salience

Salience of scientific information for policy could be seen as involving only the narrow question of whether or not the information is directly relevant to the decisions being made. However research on risk perception has found some patterns that are relevant for understanding how stakeholders are likely to judge the importance of information about environmental hazards. What is perhaps most interesting is the importance of an internal connection between salience, legitimacy and the need for policy.

Where salience is at issue among the public it may have a positive relationship to credibility, and example of a complementarity rather than a trade-off between these two attributes. One study done

with college students gives a weak indication of such a relationship. They examined the implications for the perceived credibility of scientific claims of the funding source of the scientist and the degree of the students' prior involvement in the issue, a measure of the communication's salience to the student rather than actual behaviour. The source of funding has no impact (only university scientists were used) but prior involvement led to higher perceived credibility (Sprecker 2002). This finding may also indicate a prior disposition to agree with the scientist, and the paper is unclear about the implications of involvement in the issue in a direction contrary to the implication of the finding.

Perceptions of risk in general are higher when they are more salient to the perceiver, for example if they know somebody personally who suffered the adverse event (Kolker and Burke 1993). The inverse, however, is not true; higher risks are not necessarily the more salient ones. In fact, people see risks having middle-level probabilities as the most salient and high-probability dangers are overlooked because they are more accepted, for example traffic accidents (Douglas 1985).

In another example of exaggerated trust in one's own experience, people overestimate their ability to detect risk themselves. Many non-specialists believe that they should be able to detect risks with their own senses, for example one study of risk perception found that non-scientists thought they could taste pollution (Johnson and Griffith 1996). Judgments about whether scientists and others are aware of risks are not correlated with concern about seriousness of risks (Fife-Schaw and Rowe 1996).

Whether or not the risk is seen as under one's own control, or as forced on one by another part is also important, not only in perceptions of fairness and legitimacy but in respect to salience as well. A risk that becomes well known is generally connected to some issue of legitimacy, therefore moral concerns are involved not just in the response to risk but in its perception (Douglas 1985).

I don't know of any research that directly links these findings about salience and risk to the question of how stakeholders will perceive the salience of scientific advice for policy. It is a reasonable hypothesis, however, that advice based on findings that are linked to experience and previous knowledge of an issue will not only be seen as more credible, but will likely also be seen as more salient. The connection between the perceived legitimacy of a risk and its salience may indicate that moral evaluations of a policy area may be a key prior disposition to the evaluation of scientific findings for policy.

These potential linkages between risk perception research take us close to the central issue of uncertainty, as risk is a quantification of certain types of uncertainty. Jasanoff (1986) points out that officially sanctioned risk assessments focus mainly on technical questions and their guidelines emphasize using multiple sources of uncertainty to make numerical assessments. This in itself has a political dimension as environmentalists feel that risk assessment it creates a false impression of certainty. Shackley and Wynne (1996) are concerned that scientists are motivated to demonstrate a control over uncertainty, such as its quantification as risk, because they see uncertainty as a challenge to the authority and use of science in policy making. Risk assessment is deeply enmeshed in the politics of uncertainty, which has a central place in the overall subject of science and policy.

2.3 Uncertainty, Society and Science for Policy

In the 1980s, prominent social theorists began to argue that the West has become a 'risk society' in which anxiety over uncertainty is the driving force in the development of both the self (Giddens 1991) and institutions (Beck 1992) and that science is the key institution that we look to in order to relieve these anxieties. This was seen as a profound shift away from the notion, rooted in the historical experience of industrial society, that competition between industrial classes was the main driver of social change. This move was linked to the idea of a "post-modern" society characterized by inescapable uncertainty due to both information overload and the loss of the ability to trust

traditional sources of valid knowledge. The rise of the risk society meant that personal identities have been challenged and the self has become fragmented and exposed (Giddens 1991). One result being the rise to prominence of identity politics. The institutions and belief systems that protected us from what Giddens (1991) termed 'ontological insecurity', including the traditional authority of science, are now challenged.

Suggesting that struggles over the distribution of risks were becoming the key social driver, Beck (1992) concluded that science in practice is often an expression of social power that acts to silence rather than illuminate concerns. A more accurate portrayal, Irwin (1995) argues however, would be that science is the arena where disagreements over how to respond to risk and uncertainty are played out. Power certainly plays a part but so do scientific realities.

The point here is not to introduce the reader to recent changes in social theory but to emphasize how important uncertainty has become for modern life. The phenomena these theorists point to have had an impact not just on how to do science but on scientist's own lives. Scientist cast in this central role as arbiters of uncertainty have their own identities as scientists challenged in ways that have a direct impact on their morale (Wilson and Hegland 2005). The automatic authority of science is a thing of the past. In any policy arena where the stakes are high, "the political manipulation of uncertainty is now the focus of any relevant epistemology" (Funtowicz and Ravetz 1992:251).

Uncertainty is by no means simply a social phenomenon. Indeterminacy is at the very heart of our scientific understanding of nature and is a basic characteristic of many natural systems (van der Hove 2007). Moreover, even in laboratory science, where a commitment to the replication of results is feasible, scientific knowledge is finally based in some context and process that may or may not apply to other situations (Wynne 1993). This is, of course, all the more true for those of us relying on science to describe the condition of natural resources *in situ*. In spite of fears of relativism and loss of authority that can be found in both policy and scientific communities, transparency and explicit statements in relation to the limits of scientific knowledge can strengthen science and reinforce scientific quality (van der Hove 2007).

Any number of categories of that phenomenon for which I am using the generic term "scientific uncertainty" have been proposed. Harwood and Stokes (2003), for example, suggest a fairly simple and concrete classification of uncertainty from their experience directly with fisheries management. They suggest uncertainty in fisheries consists of:

- *process uncertainty*, which is also called natural variation or stochasticity;
- *observation error*, in which data input is faulty;
- *model error*, in which the model does not accurately specify the causal processes; and
- *implementation error*, in which the resulting measures are not translated into actual behaviour in the way the models had foreseen.

To this list Kell et al. (2007) adds error related to the estimation of parameters as a form of model error, as distinct from that related to accurately reflecting system dynamics. Harwood and Stokes (2003) also contrast epistemic uncertainty, which is related to things that can be measured and linguistic uncertainty which is uncertainty in the language used to describe or classify desired states. This kind of reasoning is taken a bit further by Wynne (1993) who suggests that the term:

- *Risk* be used when behaviour is known and that outcomes can be assigned probabilities;
- *Uncertainty* be used when important system parameters are known, but not their probability distributions;
- *Ignorance* be used when what is not known is not known; and,
- *Indeterminacy* be used when causal chains, networks or processes are open and thus defy prediction.

Funtowicz and Ravetz (1990) take a philosophical tact in respect to the uncertainties around any particular quantification. They begin by arguing that there are scientific tools available to deal with three kinds of uncertainty in respect to quantities. The first deals with measurement errors and assesses the reliability of instruments. The second deals with the probabilities that arise through the combinations of different variables. The third are the error terms related to statistical sampling. They relate these three types of tools to what they call the three limits of scientific knowledge: errors in measurement made by real instruments over and above that which can be handled by reliability testing; the randomness that stems from the limits to causal understanding in the real world, and the correspondence between descriptive objects and the reality to which they refer.

Funtowicz and Ravetz (1990) conclude by proposing three kinds of uncertainty:

- *Inexactness*, which is usually expressed by numeric confidence limits and reflects the spread found in any data set;
- *Unreliability*, which is expressed by the level of the confidence to be placed around a statement and is often expressed by semi-qualitative statements such as "conservative by a factor of 10", and;
- *Border with ignorance*, which can only be addressed by describing the state of the art that produced the quantity.

Three kinds of responses correspond to the three kinds of uncertainty. Technical responses can be made to improve exactness, and for this standard routines, such as statistical techniques, are usually adequate. Unreliability is addressed in the search for improved methodologies. Border with ignorance, they argue, calls for significant shifts in the way that science is done (Funtowicz and Ravetz 1990).

Scientists, especially those involved in supporting policy find themselves required to deal with uncertainty. This is especially problematic at the "border with ignorance" because this moves them beyond their training and even their understanding of what it means to do "science". This can mean a direct challenge to their professional identities and for some even anomie resulting from the experience of being underused (Cullen 1990) or being asked to play roles that conflict with their scientific standards, values, or ideals. The anomie arises from being asked to play a difficult role under sometimes trying conditions and then having the results of these efforts "disembedded" from the cultural context that produced it, in other words from the background understandings that make that product, for the scientists, "science" (Wilson and Hegland 2005). Because of the centrality of science in the risk-society, scientists are more vulnerable to Giddens' (1991) challenges to identity than their relatively good working conditions and high status would suggest.

Scientists look for ways to respond to these challenges while maintaining their self-image of doing quality science. One basic choice that they can make is that they can choose to simplify uncertainty or they can choose to emphasize the uncertainty. Both of these involve tradeoffs. If they simplify the uncertainty the caveats they offer may be ignored, if they emphasize the uncertainty their advice may be ignored entirely (Harwood and Stokes 2003).

Some responses to uncertainty have a clear element of defending scientists' authority and sometimes material interests. Shackley and Wynne (1996) review some of the strategies that scientists have used in their presentation of uncertainty in the context of global climate change. One approach they term the "clarification and management of uncertainty". When they take this approach, scientists are selective about the uncertainty they are willing to communicate to policy makers and understandings with the scientific community allow the other scientists to be satisfied with this. Policy makers are supportive because the ambiguity allows them to avoid challenges to their authority. The second approach is "reduction of uncertainty". This is the "devil's pact" mentioned above where policy makers are able to avoid changes while scientists use uncertainty to justify research budgets. The third approach they is "transformation of uncertainty". The phenomena Wynne (1993) terms indeterminacy is recast as uncertainty and ignorance is recast as

risk. This makes things appear more tractable to the managers while helping the scientists to maintain authority. The last strategy they term the “condensation of uncertainty”. In this approach all the uncertainty is collapsed into one category, in their example this is risk, and the other forms are treated as superfluous in communications outside the scientific community.

Again using Wynne’s (1993) categories, Harremoes et al. (2001) argue that it is ignorance, not risk, which is the main issue that precautionary decision making must address. They examine what scientists and policy makers can do to address ignorance in a precautionary way. They suggest an emphasis on the reversibility it key, as is focussing research on indicators for monitoring and early warnings. They also argue the benefits of casting a wide net for knowledge to address ignorance by reduce obstacles to interdisciplinary learning and making use of lay and local knowledge.

Harremoes et al. (2001) also focus on the role of public perceptions. Scientists and policy makers often mischaracterize how the public understands the problem. Research on public responses to policy questions involving scientific uncertainty have found that non-experts are usually able to make a distinction between ignorance and other kinds of uncertainty. Public concerns are centred on a perception of unacknowledged ignorance behind publicly presented science. They further argue that the most common policy response, to reassure the public, has been to intensify research on identified uncertainties to demonstrate that the concerns are being considered and are well in hand. This is because by policy makers believe, wrongly, that the public expect certainty (Harremoes et al. 2001).

The limits that science faces in the “risk society” challenge scientists’ identities and require some fundamental shifts in how scientists operate. Funtowicz and Ravetz (1992) argue that addressing the limits of ignorance requires skills and practice in the form of a "learned art" that resembles the activity of a consultant more than that of a research scientist Funtowicz and Ravetz (1990) often make use of the contract between "knowing-that" v "knowing-how" when discussing scientific quality control in contexts of high uncertainty. Knowing-that is about the ultimate attainment of truth, while knowing-how is about practice. Knowing-how is skilled, it is rooted in tacit knowledge and not part of traditional philosophy of science. In resource management scientists playing this kind of role can be seen in certification programmes where management programmes are evaluated according to a complex set of criteria. Issues of uncertainty become areas of ongoing negotiations between the scientists and those who desire certification.

This is one “new” model of scientific action in respect to uncertainty that is directly relevant to resource management. There are others. What they have in common is a focus on the boundary between science and policy. Various scholars have emphasized boundary organizations and institutions where people and information from both science and policy interact, certification would be an example of a boundary organization. Others scholars have pointed to “boundary objects” that are the focus of the “co-production” of new kinds of knowledge products that reflect scientific knowledge. Most critically for our purposes here, maintaining boundaries that are both clearly recognized and suitably porous is the key to balancing the tradeoffs between the credibility, legitimacy and salience for science in support of policy (Cash et al. 2002). This work on boundaries has been carried out from perspectives ranging from purely philosophical reflections to empirical sociological research. The next section explores a number of the approaches.

2.4 Boundary Organizations and Objects

Within science studies the original work on “boundary maintenance”, particularly Gieryn (1983), has now achieved a seminal and classic status. The initial, now much more nuanced, argument was that within policy deliberations, scientists work to enhance their authority by defining what knowledge is really science, who is really a scientist (Jasanoff 1990, Gieryn 1983) and which questions are to be considered scientific (Dietz et al. 1989). When something is labelled as science those who are not scientists are de facto barred from having anything to say about its substance (Jasanoff 1990). When this labelling is successful knowledge becomes a ‘black box’ for the non-scientist until another expert challenges it.

This constant focus on maintaining boundaries around science is critical to the functioning of both domains. Boundary work allows scientists and policy makers to bring some objectivity into policy decisions and this need for objectivity and the status distinctions that support it should not be discounted because they cannot be perfect (Collins and Evans 2002, van Zwaneberg and Millstone 2000). The demand for objectivity and the conferral of a special status to protect it is not a denial of democracy, it is a product of it (Porter 1995, Ozawa 1991). One of the most basic insights from science and policy studies is that boundary work must be taken seriously. It cannot be treated naively, neither by assuming that the distinction between what is science and what is policy, advocacy or values is easily made in concrete situations, nor by assuming that it does not really exist. Boundary work as it emerges within policy debates is not an all or nothing affair (Guston 2001b). In more participatory decision making processes it is called upon to support specific judgments in circumstances where there are checks and balances on its use.

In the lessons they compiled from their comparative analysis of the uptake of global assessments into the policy process, Cash et al. (2002) placed the structuring of the science boundary in the centre. They found that science based policy is most effective when use is made of “boundary objects”. Examples of such objects would be models, indicators, collaborative research designs and data collection efforts that are used to provide a way to structure discussions between different perspectives. The most effective science policy efforts were found where boundaries were recognized and respected but not used as a rigid barrier to interaction. Participation was allowed across a recognized and selectively permeable boundary. The effective structuring of science-policy boundaries is delicate and complex. It depends on the nature of the problem to be addressed, and the degree of consensus over policies and values as well as scientific facts (Engles et al. 2006).

Pressures on the science boundary emerge through the encounter with uncertainty, on the one hand, and pressures in policy processes for finding “scientific” answers to more and more kinds of questions (Wilson and Hegland 2005). Boundary work is difficult. Ozawa (1991) points to the constant pressure to ‘miscast roles’ as various stakeholders seek to make the political ‘objective’. Boundaries that are too rigidly maintained, however, can lead to scientific elites being shielded from accountability until a crisis is required to expose the problems (Waller 1994). These difficulties have led to contradictory messages. Irwin (2006) offers an interesting analysis of current statements about science and policy being made by responsible institutions in the UK. The statements being made still remain rooted in a traditional understanding of science as indisputable facts about nature and of scientists as an objective other standing apart from the policy process, while simultaneously the statements are promising a more democratic approach to science. The two narratives of inclusion and otherness go on at the same time from the same sources without resolution.

Boundary work is a response to the repeated failure of methodological definitions to delineate science in practical contexts (Evans 2005). Science in practice can no longer be easily reduced to a

single methodology or ascribed to a privileged subculture (Gibbons 1999). Halffman (2003) argues that neither the “cage model” nor the “seamless network” model seem to accurately describe the policy science interface. The 'cage model' is based on the idea that it is possible *a priori* to establish criteria about what is 'science' and what is 'politics'. In the “seamless network” model, for which Halffman (2003) cites Actor Network Theory as the most extreme example, the attempt to make a distinction between science and politics (indeed between the social and the natural!) is abandoned. Halffman (2003) argues that both approaches fail to describe the actual division of labour of regulatory regimes and they both reduce the real efforts to negotiate the boundary to “misrepresentations,” i.e. *independent science* on the one hand or *false dichotomies* on the other.

Gibbons (1999) argues that there is nothing really new about the boundary problem, reliable knowledge has always been reliable within certain boundaries. What is happening in policy contexts with the risk society is an increase in contestation. Knowledge that is incomplete now is not just knowledge that is waiting for better science, it is knowledge that will be contested. This contestation then puts further strain on the science boundary. Evans (2005) observes one case of scientific controversy where the more one side tried show themselves as more scientific than the other the more they broke down the appearance of science and each began to appear just as unscientific as the other. Latour (1987) notes that the common pattern in scientific controversies is for each side to push the other back toward the conditions of the production of the facts to make actions of skill and judgement appear as a lack of objectivity.

Increasing competition is also exposing scientific boundaries to increasing strain. Waterton (2005) carried out a large set of interviews where she asked scientists to reflect on science. She asked about the impacts of the rise of contract-driven science and many of her examples were from scientists involved in policy support. Scientists would invoke scientific norms as something that has been lost fairly recently as science has become more and more contract driven. They are seeing science getting more competitive, one scientist mentioned having to compete for a tender based on the same ideas that he had given the ministry.

Influential work by Guston (1999) focuses on organizations and institutions around the science boundary. Boundary organizations, he argues both facilitate communication and provide important mediating functions across the shifting boundaries between politics and science (Clark et al. 2000). These arrangements can be very simple, one example of an effective boundary organization was simply a set of two-day sessions to familiarize policy-makers with acid rain transfer and deposition models developed by scientists (Haas 2004). They can also be very complex, Dalrymple (2006), for example, argues that the CGIAR network of agricultural research institutions has evolved into a boundary organization mediating between agricultural scientists and international development donors.

Other researcher has not found boundary organizations emerging where they might be expected to. Waterton (2005) was surprised by the lack of reference to institutions resembling boundary organizations in spite of the fact that the scientists she was interviewing were often working at the policy science interface. She argues that the scientists often find themselves having to negotiate the boundaries alone and she found that they are sometimes doing this self-consciously and sometimes finding this painful and difficult. She suggests that ways of maintaining the science policy boundaries are diverse and complex and we should not allow the idea of boundary organizations to obscure this complexity.

As briefly mentioned above, Clark et al.'s (2002) review concludes that boundary spanning arrangements are critical to effective policy uptake. They emphasise that in their analysis the term “policy maker” is meant to include governments around the world as well as “farmers in the American Midwest and in Zimbabwe, coastal zone managers in Maine and Hawaii, and power plant

operators in Bulgaria and Poland" (2002:9). Boundary spanning arrangements facilitate the balancing of salience, credibility and legitimacy, by providing a communication bridge that helps decision makers convey the questions they need answered and scientists to communicate their findings more effectively.

2.5 The Science Boundary and Credibility, Legitimacy and Salience

Boundary analysis was the central tool that the Global Environmental Assessment Project (Cash and Clark 2001, Cash et al. 2002, Clark et al. 2002, Clark et al. 2006) used to describe the trade-offs and synergies among credibility, legitimacy and salience. As mentioned above, maintaining strong boundaries runs the risk of decreasing salience by removing decisions maker from helping to define questions. But mechanisms that limit participation of decision makers, as well as other stakeholders, skeptics and/or non-mainstream experts, to maintain credibility decrease legitimacy. Increasing legitimacy by greater inclusiveness can decrease salience by re-framing the issue in a way that is irrelevant to the most stakeholders, as well as leading to accusations that the science can be seen as "tainted" by politics.

"If boundary spanning is too strong in coupling producers and users of assessments, suspicions arise that policy makers are not only asking the questions but also determining the answers, thus reducing the credibility of the assessment and thus its influence.....Effective institutional arrangements for boundary spanning facilitate agreement between scientists and decision makers over what questions an assessment will address, what kinds of evidence and expertise it will employ, and what processes it will follow."
(Clark et al. 2002 p:9)

Transparency in respect to what is going on inside the science boundary is a critical tool as it can increase the credibility of even more inclusive processes. Transparency that is correctly structured can force participants to account clearly for how they know what the claim to know (Wilson and Pascoe 2006). Transparency is not a panacea, as Jasanoff (2002) points out openness rules alone do not provide missing information, expose all the hidden assumptions and may even dampen novel interpretations of facts. Transparency rules have negative aspects as well. Especially the very formal and comprehensive ones such as those found in policy making in the United States and hide as well as disclose important information (Jasanoff 2002). Transparency also forces stakeholders to maintain public positions that are oriented to home constituencies rather than to coming to agreement with others about the facts. Hence, the Chatham House Rule that is designed to facilitate a search for compromises is specifically a limit on transparency:

The Chatham House Rule reads as follows: "When a meeting, or part thereof, is held under the Chatham House Rule, participants are free to use the information received, but neither the identity nor the affiliation of the speaker(s), nor that of any other participant, may be revealed" (<http://www.chathamhouse.org.uk/>).

Considerations of how to structure the science boundary for maximum credibility, legitimacy and salience also needs to consider the timing of processes, in particular the timing of the use of different disciplinary perspectives and/or stakeholders. Inclusion of a wide group in order to increase legitimacy can fail on all three dimensions if the inclusion comes so late in the process that it cannot influence the questions being investigated. But this consideration must be held in tension with the roles of various disciplines in establishing credibility. Lenhard, et al. (2006) suggest that the transparency derived from using established disciplinary approaches early in the process can be used to establish scientific credibility contributions that will support the later development of scientific legitimacy. They relate this to the incident mentioned above from the negotiations over the content of the Intergovernmental Panel on Climate Change's Second Assessment Report in 1995, sceptics from the political side from OPEC withdrew their veto with the decisive factor in their decision being a frank appeal by the climatologists to the accepted methods and standards of their field.

Lenhard et al. (2006) are arguing essentially for a multi-disciplinary rather than trans-disciplinary approach to complex problems. A multi-disciplinary approach is characterised by different disciplines tackling different aspects of a problem, each using their own practices and standards. In a trans-disciplinary approach, on the other hand, cross-disciplinary teams seek to establish new practices and standards in respect to the nature of the problem. Scholars in the Mode Two tradition, discussed below, are not opposed to disciplines as such (Nowonty et al. 2001) but they do argue that new problems generate new forms of interaction and scientific identities. They also suggest that disciplinary boundaries are atrophying in the new ways that science is being organized, an assertion that has been challenged on empirical grounds, i.e. while scientists from different disciplines can certainly be seen working together the main scientific institutions are still strongly disciplinary in organization (Shinn 2002).

One area where the credibility, legitimacy and salience is often raised in respect to question of the advocacy role of scientists. Scientists have a right to participate in political interest groups just like anyone else, but there is a clear tension between this role and making a claim of producing a disinterested analysis that would have the highest credibility. Again, Young (1989) argues it is a boundary issue, what kinds of claims should scientists be making in what context. Is it possible for them to take off and put on their “scientist hat” in different situations? Will they be given permission to do so? Scott et al. (1990) document three examples of people who tried to be neutral investigators in scientific controversies. In all three cases they were pulled into the controversy in spite of trying very hard not to be. Most often their attempts at to be even-handedness were interpreted by others in the controversy as an attack on the orthodox position. Where scientists find themselves as advocates they can find themselves confronting opposing interests who offer their own science and scientists. This results not only in undermining the credibility of the original scientist’s expertise but that of science in general (Gill 2001). In the United States the emphasis on making decisions through confrontations between experts in legal fora has led to both an overblown idea of the importance of the “expert” and unlimited scepticism toward particular expert judgements (Jasanoff 2002).

The discussion of scientists as advocates leads to real boundary difficulties even at a more abstract level of definition. Minnia and McPeake (2001) are two scientists struggling with the advocacy question. Their main concern seems to be that advocacy will lead to accusations and perceptions of bias. “When the object is consonant with the personal values of the advocate, the appearance of bias enters; in other words the advocate *appears to have a personal agenda*” (Minnia and McPeake 2001:6 italics added). They do not seem to believe that in and of itself advocacy implies bias. Indeed, they differentiate between advocacy and “extremism or irrationality” suggesting that it is in the later case the bias is introduced while mere advocacy, paradoxically, can remain objective. In making this argument they also suggest a dichotomy between a 'personal' agenda and 'conservation' agenda, with the implication that a conservation agenda is more objective and scientific than a personal one would be. The science boundary in their imagination seems at least partly a question of maintaining a reasonable style of presentation and advocating for things that are generally accepted to be good rather than simply seen as good in someone’s personal opinion. Indeed they justify this by pointing to the mission statements of the wildlife agencies that employ them. This sort of reasoning about the science boundary is not uncommon (Wilson and Hegland 2005), nor should it be rejected because it is so far removed from what one would think if as an adequate way of defining the science boundary from a philosophy of science perspective. Within the context these scientists are operating, these distinctions may well be practical and serviceable.

2.6 The Co-production of Science and Policy

Several observers of the evolution of science-based policies argue that the patterns they see are nothing like the traditional idea of a scientific result becoming the basis of a policy. They argue that the interactions between the two domains are so close that they not only should be thought of as intersecting, but even as co-evolving domains of human activity (van der Hove 2007). Jasanoff (2004) takes the co-production argument even further, arguing that thinking of natural and social understandings being produced together provides explanatory power in many domains. Policy is so dependent on science, and the science used to support that policy is so dependent on direction and support from the policy process, that the two are developing a common product. Clark et al.'s (2002) results certainly suggest that some kind of joint activity around the creation of boundary objects has been a feature in the successful uptake of scientific knowledge into policy. Some even argue that the long-observed cultural distinctions between the two communities are weakening and that scientists working in support of policy and policy-makers are becoming part of single culture creating projects that support the legitimacy of both (Lidskog and Sundqvist 2002).

Scientists working in these hybrid science-policy communities find themselves having to negotiate their credibility not only among policy makers but also among their fellow scientists (Shackley and Wynne 1996). The credibility problem raised by these patterns of co-production of science and policy, Shackley and Wynne (1996) argue, arises not so much from what might be the common sense idea that the scientists' intellectual integrity has been "bought" as it does from the ways that uncertainty is handled. Within such tight communities scientists learn to represent uncertainty in ways that never challenge the assumption found in the policy world that risks are tractable and that current institutions are capable of managing them.

Rothstein et al. (1999), for example, argue that co-production is a good term for describing the agrochemical review process in the UK, where a network of environmental toxicologists in both government and industry has emerged. In reaction to a number of costly regulatory decisions the industry developed a substantial in-house scientific and has given them a lot of decision making power over the development of new products. This network is relatively small and informal and scientists move between government and industry positions. While this network has a positive impact on the salience of the scientific information for policy development there has been a substantial cost in credibility. Critics, especially elsewhere in Europe, have assailed the system for being too quick to accept new chemicals. The problem here may not be simply that the networking between the government and the industry is too tight, rather it is the exclusion of other stakeholders who are excluded, most pointedly environmental NGOs, that creates the real costs in credibility.

Niederberger (2005) reviews the policy successes and failures of the Advisory Group on Climate Change Research and Policy. This is a Swiss institution that was created to serve as an interface between science, government agencies and the public. She makes a distinction between a boundary organization and an independent intermediary organization, arguing that while the latter is basically about facilitating communication the former should be understood as playing an even deeper role. Where simply facilitating communications leaves intact the two separate sets of standards and forms of accountability found in science and policy institutions, co-production involves more fusion between the two. She argues that if the Advisory Group has been a true boundary organization involved in co-production it would have had a stronger policy impact. Niederberger's (2005) distinction seems to be more theoretical and philosophical than empirical; one could well imagine that the changes in policies themselves would be taken as evidence of the existence of the kind of co-production she is looking for.

However common this ideal of "co-production" may be there are clearly a large number of models of science and policy interactions in use. There are identifiable national styles in how governments seek to structure these interactions (Halffman 2005, Jasanoff 2002, 2005) as well as many instances

of individual scientists trying to negotiate boundaries without the aid of any organization or clear guidance on whose standards should apply to what kinds of decisions (Waterton 2005). The research cited here strongly suggests that the ways these interactions are structured is important. Neither a model that tries to maintain a large distance between the two based on some clear *a priori* definition of roles nor a model which places them so close together there is not credible independence seem to be as effective as those arenas where more complex interaction has evolved.

3 Theory and Practice in the Co-production of Science and Policy

3.1 Well Established Theories of Interactions Between Science and Policy

3.1.1 Epistemic Communities

Peter Haas (1989) developed the concept of the “Epistemic Community” within the discipline of international relations as a way to try to explain the successful emergence of the Mediterranean Action Plan, a pollution control regime around the Mediterranean Sea. He took the term from the philosophy of science where it was used to mean scientists who share certain assumptions about reality and epistemology. He argued that the Mediterranean Agreement came about because of the existence of an ecologically oriented epistemic community which was made up mainly of people working in the environmental ministries of the various countries. They shaped their governments’ policies, hired people who thought like them so gained international support (Haas 1989).

The idea of the epistemic community has proven useful to many observers of science-based international management regimes. Haas (1992a:p3) defines an epistemic community as

“a network of professionals with recognized expertise and competence in a particular domain and with authoritative claim to policy-relevant knowledge within that domain or issue-area. Although an epistemic community may consist of professionals from a variety of disciplines and backgrounds they have:

- (1) a shared set of normative and principled beliefs, which provide a value-based rationale for the social action of community members;
- (2) shared causal beliefs, which are derived from their analysis of practices leading or contributing to a central set of problems in their domain and which then serve as the basis for elucidating the multiple linkages between possible policy actions and desired outcomes;
- (3) shared notions of validity - that is , inter-subjective, internally defined criteria for weighing and validating knowledge in the domain of their expertise; and
- (4) a common policy enterprise - that is a set of common practices associated with a set of problems to which their professional competence is directed, presumably out of the conviction that human welfare will be enhanced as a consequence.”

Epistemic communities are a particular type of policy network that is characterised by this agreement. While a group of scientists within an agency, for example, might have a common belief in the scientific method, this does not give them solidarity. Members of an epistemic community share a strong normative orientation (Haas 1992a). Epistemic communities are an example of the "international civic society" where people divide their allegiance between domestic constituencies and international peer groups (Engles et al. 2006).

Haas (1992b) argues that the success of The Montreal Protocol on Substances That Deplete the Ozone Layer can be attributed to an epistemic community. The first countries to actively encourage control were those in which both the epistemic community and a tradition of pro-environmental

sentiment were strong. Once channels between other countries' national administrations and the epistemic community were established the other countries began to support action. His analysis found that it was these contacts that made the real difference, rather than public opinion or the actions of environmental NGOs. These things became important later in the process after government regulations had been introduced. Interestingly, for Haas (1992b) the key actor was the DuPont Corporation, which that broke ranks with other chemical companies in an act that was critically important to the momentum for international action. Haas (1992b) argues that the difference was that key decision makers on this issue in at DuPont were all chemists, modified their positions in reaction to advances in scientific understanding.

The Epistemic Community approach within international relations takes a more deferential attitude toward science than is usually found in science studies. The effectiveness of the scientific community in international regime formation is rooted in the ability of the scientists to reach a consensus and to overcome their natural inclination to extreme caution. These characteristics are seen in the formation of international regimes that Young (1989) argues were essentially science driven including The Convention on International Trade in Endangered Species and the International Union for Conservation of Nature and Natural Resources (Young 1989). The key to effectiveness is that knowledge on which the epistemic community is based "accurate, accessible, and contribute to the achievement of collective goals. It must represent consensus and be provided through a medium that is politically palatable" (Haas 2004: 575). Echoes of the Epistemic Community approach are evident as well in Cash and Clark's constructivist approach. While they do not use the term, the implicit goal of their constructive activities is very similar.

The emphasis of the mobilization of a consensus is evident even in the way Haas' (2004:578) titles his "lessons about mobilizing networks of scientific expertise for sustainable development (Table One). For example, in Number Three he suggests that "a core group sharing values and causal beliefs" be identified while Number Eight recommends the promotion of "scientific discussions on topics that are likely to lead to consensus".

Table One: Lessons about Mobilizing Networks of Scientific Expertise for Sustainable Development

From Haas (2004:p578)

1. Create standing international interdisciplinary scientific panels or committees to address specific topics.
2. Multilateral Environmental Agreements should create and rely on separate subcommittees responsible for different functions of governance, such as basic research, environmental monitoring, policy analysis and policy verification and evaluation.
3. Carefully survey the population of scientists to identify a core group sharing values and causal beliefs. For instance, in the Mediterranean a UNEP consultant spent nine months visiting national laboratories to inventory national capabilities and to personally build the scientific network.
4. Ensure that networks and international panels have interdisciplinary representation, including the social sciences. Individuals should have high regard in their own disciplines as well as be able to talk to experts from other disciplines.
5. Recruit carefully for national and regional institutions. Base judgements on professional credentials and networking ability.
6. Avoid relying on one national institution to provide or sponsor research and training.
7. Provide professional outlets for members through conferences and publications in refereed professional journals. This also elevates the domestic profile of individual scientists in the community of expertise who may then be recruited to fill positions in national administrations.
8. Promote scientific discussions on topics that are likely to lead to consensus, i.e. ripe research topics.
9. Avoid government designation of scientists to international meetings.
10. Try to make use of joint international panels for environmental risk assessment rather than relying on national assessments. Avoid capture by one scientific discipline or school of expert analysis.
11. Assure the timely submissions of scientific advice in advance of meetings. The timely submission of reports according to the legislative cycle in the major countries is also key.
12. Arrange for focused interactions between scientists and policy-makers to discuss the technical substance of the issues. For instance, in LRTAP the IIASA arranged for two-day sessions to familiarize policy-makers with acid rain transfer and deposition models developed by scientists.
13. Maintain momentum within the community by continuing to have projects and research opportunities so those members don't drift away. This avoids having to reconstitute the community each time a new problem emerges.
14. Seek funding for studies from multiple sources in order to avoid budgetary shocks if money is withheld from a principal funder. Thus economically inefficient redundancy is politically warranted.
15. Construct models so that effects are calculated at meaningful political scales, i.e. corresponding to significant political divisions that are relevant in developing policy applications. For instance, at the international level this means that models should explicitly identify effects by country, and even, if possible, by domestic districts (at least in countries without proportional representation). So far climate change models have only yielded effects at a scale of resolution sufficient to demonstrate to countries able to vote in the General Assembly that they are likely to suffer, and thus they keep the issue on the international agenda.
16. Train or recruit scientists who have a high profile within their own discipline and who are able to effectively communicate with counterparts from other disciplines, as well as with the media, politicians and popular audiences.
17. Try to recruit networks from as broad a national basis as possible, as governments are more likely to rely on experts who share their nationality than on foreign experts.

The emphasis on consensus has been the characteristic of the Epistemic Community approach that has brought the most criticism, especially from scholars trained in science studies. Lidskog and Sundqvist (2002) argue that Haas' was right about the importance in international relations of a common picture of reality to challenge the idea that national interests alone drive outcomes. However, they accuse him of a naive view of science almost to the point where he introduces a consensus notion of truth as part of the Epistemic Community argument. Others question the strong programmatic emphasis on consensus, as seen especially in his lessons (Table One), because disagreement is not only going to be found within any scientific community; it is a positive force driving new thinking (Young 2004).

Both the strength and the weakness of the Epistemic Community approach is that it posits an ideal situation: a strong consensus among scientists reflecting truth about nature that has clear normative implications and policy alternatives that all of the scientists can gather around. The strength of this ideal-based approach is that it describes what has happened in several successfully negotiated environmental protection regimes. These empirical examples, while limited in number, do show that success is possible and provide a set of experiences of success from which lessons can be drawn. For this reason the Epistemic Community approach does a better job than the other two theories of interactions between science and policy of generating "how to" advice for negotiating effective environmental protection. Hence, Haas (2004) is better able to produce lists such as the one in Table One.

The weakness of Epistemic Community approach is also rooted in its focus on an ideal situation. What about the majority of environmental problems where the degree of agreement among scientists is a mixed bag of agreement about some facts (and some values) but not others? Haas' (2004) response is that we must wait for the consensus and until that consensus emerges scientific and policy developments must be kept insulated from one another. This response is entirely inadequate. It is grounded in a naive view of both the way the science boundary works and of the degree of urgency we face in tackling many of these issues. In a way it misses the main point: that indeterminacy and uncertainty are always going to be with us, and that is the reality we have to learn to deal with.

The Epistemic Community idea sees cooperation as an either or proposition (Sebenius 1992) that fails to consider the multiple bottom lines that participants in negotiations face. Ways that policy development can move forward do not emerge all at once. What is the most critical is the people involved in ongoing negotiations are able to move forward when the opportunity arises in a way that their ability to work together in the future is enhanced rather than impaired. Such a community may become a true epistemic community when uncertainty is reduced and the way forward becomes clear, but it must continue to learn and adapt in small steps the rest of the time.

3.1.2 Post-Normal Science

The second well establish theory of interactions between science and policy is Silvio Funtowicz and Jerome Ravetz's idea of Post-Normal Science (PNS). PNS happens in policy areas characterized by both high uncertainty and high stakes. In these conditions facts are at best expressed as probabilities and it is very difficult to keep such facts separate from the interests and value positions, which are intense because of the high stakes involved, that determine how participants perceived the associated risks. The basic argument they make is that such conditions require a more participatory decision making process. A central concept in PNS is the "extended peer community". To deal with new problems in a high uncertainty / high stakes area an open dialogue is required because the quality of the science depends on this "extended peer review". The important thing to keep in mind to understand PNS is this link to quality. The idea of the extended peer community is close to, but not synonymous with, stakeholder involvement in science. The extended peer community is about quality control.

To understand what they are getting at with this form of quality control it is helpful to review another key concept in PNS: the NUSAP notation for quantitative statements. The ideas of PNS begin with a set of philosophical developments about the nature of quantification. Their arguments are mathematical ones broadly understood and are not in the first instance grounded in practical issues of science in high stakes policy arenas the way that both the Epistemic Community approach and the Mode Two science approach are. In the NUSAP notation in addition to the familiar categories of Number, Unit and Spread, Funtowicz and Ravetz (1990) introduce the new categories of Assessment and Pedigree. Assessment and pedigree are both about the characterisation of uncertainty.

Assessment relates to uncertainty rising from problems in the reliability of a quantity. It could be expressed, for example, in arguments about what level of confidence should be placed around a statement. Should it be 95%, 99% or some other level? Assessment is about the different kinds of judgements that one would see expressed by statements such as "conservative by a factor of 10". As Funtowicz and Ravetz describe it "our knowledge of the behaviour of the data gives us a spread and our knowledge of the process gives us an assessment" (1990:28).

Pedigree relates to uncertainty rising from the "border with ignorance", discussed above in the context of their categorization of types of uncertainty. Here Funtowicz and Ravetz (1990) step entirely into the qualitative aspects surrounding the quantity. Pedigree addresses the relevant epistemological, historical, sociological and institutional contexts needed if one is to understand the implications of a quantity for policy. Their approach to pedigree is to develop "pedigree matrices". These matrices are based on a normative hierarchy of modes of knowledge: a deductive argument is stronger than an inductive inference which, in turn is stronger than an analogical argument. All three of these are these are considered stronger than conventional definitions. They offer a number of examples of pedigree matrices. A very basic example is Table Two, the pedigree table for research information:

Table Two: Research Pedigree Matrix			
Theoretical Structure	Data - input	Peer-Acceptance	Colleague Consensus
Established theory	Experimental data	Total	All but cranks
Theoretically-based model	Historic / field data	High	All but rebels
Computational model	Calculated data	Medium	Competing schools
Statistical processing	Educated guesses	Low	Embryonic field
Definitions	Uneducated guesses	None	No opinion

Table is taken from Funtowicz and Ravetz (1990) page 140

The table establishes a hierarchy which allows the receiver of the quantity communicated with the NUSAP notation to evaluate basic levels of certainty. They also offer other and more complex examples of pedigree matrices for such fields as environmental modelling or evaluating statistical information. The NUSAP notation and the pedigree table help clarify what they are pointing at with the idea that it is in the “extended peer community” that the relevant forms of quality control for the quantity arises in contexts of high stakes, high uncertainty. The extended peer community is made up of the various groups that their perspectives on the policy and their own knowledge to contribute. The extended peer community is the people who have the knowledge to fill out the pedigree table, knowledge that is found in both scientific disciplines themselves and in the sociology of knowledge of the policy arena. Hence, Ravetz (1999) argues that effective science-based policies in arenas of high stakes and high uncertainty require an open dialogue with all those affected. The extended peer community improves quality by mobilizing "extended facts" to help develop a shared understanding of the uncertainty in areas of conflicting values and agendas (Healy 1999).

This extended peer community is not meant to reduce the authority of science or to make it explicitly political. It addresses the problem that in these high stakes, high uncertainty areas the traditional mechanisms for assuring quality are not adequate. Accredited experts need the assistance of the extended community to get the necessary job done. Establishing the pedigree of a quantity requires a broad understanding of its salience, credibility and legitimacy that only a broader group can provide. Ravetz (1999) expresses concern that seen out of context these ideas may appear to reduce the authority of science, he stresses, however, that PNS is not about traditional areas of research but a new areas with high social and economic importance where traditional mechanisms for assuring quality are not adequate. PNS is not meant to be an attack on accredited experts but to be a way of describing the kind of assistance needed (Ravetz 1999)

The other important concept in PNS is the contrast between ‘knowing-how’ and ‘knowing-that’. Traditional science has seen itself basically as the second, but PNS requires a new emphasis on the first. Within the high stakes, high uncertainty context, it is scientific skills, in respect to providing “rubrics, guidelines and elicitation procedures, for the expression of uncertainty, for the assessment of quality, and also for the training in both skills” (190:68), that come to the fore. Scientists are not trained to be consultants but it is the skills of the consultant that are required here. These are the skills to work with policy makers and other stakeholders in a process linking the uncertainty and quality of the information with the needs of the policy. They point out that where experts in consulting professions normally have very long practical, apprentice-type training (e.g. doctors) after their formal educations scientists generally do one major research project under supervision and are then certified as able to operate as an independent scientist. They argue that the ideas of skill and craftsmanship can be the basis of a way to reformulate science boundary in important areas of high uncertainty (Funtowicz and Ravetz 1990). This argument is further grounded in the theory of the risk society by Healy (1999) who related it to both Beck's and Giddens' ideas about the design of political spaces. The shift is from trusting science as a 'truth machine' to trusting the scientific institutions and procedures to make science that works in practical situations (Healy 1999).

Again the strengths and weaknesses of the approach are closely related. Because they begin from an essentially philosophical perspective Funtowicz and Ravetz provide a major contribution with a very clear set of rationales grounding the science boundary in epistemological rather than practical considerations. While at the same time, they also suggest an appealing programme for implementing science in areas where boundary work can become very difficult. However, while their argument is to some extent reflected in general experiences with what makes science-based policies effective, the idea of the “extended peer community” needs sociological examination. The extended communities involved in science-based policies include groups with conflicting objectives

who bring their own sets of relevant facts, values and interests into the discussion. Given the level of social conflict involved what are the conditions under which an extended peer community adds to the quality of the science rather than just adding some chaos to the complexity and uncertainty?

3.1.3 Mode Two Science

The third well established theory of interactions between science and policy is Mode Two Science, which is most closely associated with Michael Gibbons. He suggests that science is operating in a new mode that is trans-disciplinary, characterized by shifting rather than permanent institutions, and constantly moving back and forth between the fundamental and the applied (Gibbons et al. 1994). The driving force here is that society has learned to speak back to science. Society is transforming science by demanding that it be produced and respond to specific contexts where innovations are being demanded that require “trans-disciplinary” efforts. Cohen et al.’s (2001) survey found a consensus that discipline-based approaches could not deal with complex environmental issues that were the central concern of the organizations where the scientists they studied were working.

Science is shifting from a search for truth to a more pragmatic aim of providing a provisional empirical understanding of the world that works in a practical sense. Reliability has been redefined as what works (Gibbons 1999). This has forced science, particularly through the way it is financed, to become more flexible. “The epistemological core of science has, over time, become crowded with norms and practices that cannot be reduced easily to a single generic methodology, or more broadly, to privileged cultures of scientific inquiry”(Gibbons 1999:83). Scientists have had to learn to fit their research agendas into the agendas of funding agencies and firms (Gibbons et al. 1994). Mode Two points at the dominance of goal orientation as science is controlled by managers and funding agencies who reduced scientists to fungible units of manpower in ways that are very different from the ways they were previously employed (Ravetz 1999). Universities are now crowded with researchers with financial stakes in outcomes (Guston 2001a). Scientific labour has become much more like a commodity driven by contracts offered through tenders that treat scientists as interchangeable (Waterton 2005). This is arguably also part of a general move toward greater social accountability for science that can also be seen in changes in research misconduct, data availability, conflicts of interest, human subjects research, and cultural limits being placed on the pursuit of knowledge (Guston 2001a).

The Mode Two phenomenon is international, but takes quite different form in different countries. Jasanoff (2005) examined the different ways that the United States, the United Kingdom and Germany went about promoting innovation in bio-technology. The United States focussed on incentives and the strengthening of the patenting process, particularly making it possible for universities to patent work done with federal research grants. The United Kingdom took quite a different route and bypassed the universities to try to encourage the scientists themselves operate more like businesses. This was met with some success, but the scientists tended to set up their businesses around universities and remained part of the academic culture. The Germans created large corporatist structures that were a new hybridisation of science and society, where control and the enforcement of standards were critical (Jasanoff 2005)

3.1.3.1 Mode Two and Quality Control

The question of quality control has emerged as central to the Mode Two approach and the research efforts guided by it. Gibbons et al. (1994) argue that quality control has become more localized and rooted in the institutions where the research is taking place. Mode Two science results in heterogeneous rather than homogeneous growth, meaning that growth is based on new arrangements of a rearrangement of component elements rather than their replication. This makes

the application of widely accepted standards in quality control much more difficult. More intensive communication, both between practitioners and between science and society, has arisen as people seek to address issues of quality (Gibbons et al. 1994).

The trans-disciplinary nature of much Mode Two science is another source of quality control challenges. Because the intent of trans-disciplinary work is to move into realms where disciplinary standards for testing new knowledge are no longer applicable new kinds of standards have to be developed as part of the research process. In this case external quality control focuses on the procedural mechanisms and the research process itself becomes the object of evaluation (Guggenheim 2006).

The transition to a consumer-contractor relationship has been the 'reform' which has had the biggest impact on work and organization. This includes new structures of accountability that replace a collegial environment of trust (Cohen et al. 2001) and it is in these structures that new forms of quality control arise. Guggenheim (2006) studied 20 environmental consulting companies in Switzerland as an extended empirical example of Mode Two science. Guggenheim's research illuminates some of the dynamics in Mode Two research in the extremely well developed form of the environmental consulting firm. While he suggests that his results are generally applicable to trans-disciplinary work organized around research projects, it is perhaps better to understand the consulting firms as an ideal type of Mode Two science which illuminates tendencies that will not be nearly as well developed in more traditional science institutions such as universities, non-profit research organizations, and public sector research institutions.

In the companies he studied the scientists have no time or incentives to write journal articles and traditional mechanisms of peer review do not apply. The quality control mechanisms they do use are mainly internal - quality management systems (QMS) and time sheets. They also must have supervisory oversight from clients and universities. Because the companies are not organizationally bound to a traditional scientific institution they have to be monitored by "real" scientists.

The QMS are quite the opposite of a requirement to use transparent methods. They are proprietary, secret and aimed primarily at singling out the company from their competitors. They are based on internationally recognized ISO variables but also on the companies own standards in respect to these variables. They are usually formalisations of previously existing rules. The companies keep them secret because they believe that it is their organizational procedures which distinguish them from their competitors. Real accountability is in the usefulness of the actual products, this is where reputations are build (Guggenheim 2006).

Time sheets are the main mechanisms used to coordinate projects and the key expression of the Mode Two phenomenon of reducing scientists' work to fungible units of labour. Researchers are interchangeable through the medium of the time sheet. Time sheets have a profound effect on science because they decouple the research activities, expressed as the hours spent on a project, and the intellectual object of the research (Guggenheim 2006). Indeed, in Cohen et al.'s (2001) survey of scientists working increasingly under contract arrangements respondents reported that they missed the opportunity to focus on one question for a sustained period of time.

This new way of producing knowledge replaces quality control based on results with quality control based on procedures and processes. This grounds the knowledge even more deeply in the circumstances of its production. These are "non-scientific" mechanisms in the sense that the quality control is shifted to agents and mechanisms outside of scientific organizations (Guggenheim 2006).

3.1.3.2 Questions about the Mode Two Approach

Authors using the Mode Two approach seem to waver between offering explanation of empirical changes in science institutions and advocating their encouragement. This advocacy seems driven by

the idea that science has been operating as an unaccountable elite and the changes pointed to by the Mode Two approach are a timely corrective to this unfortunate situation (Elam and Glimell 2004).

Some recent scholarship has challenged the empirical argument. One important thread suggests that Gibbon's claims about the frequency and importance of trans-disciplinary research are exaggerated. Universities, industry and the government remain highly differentiated and that there are strong structural reasons why the current basic division of labour between the three will continue (Shinn 2002). Indeed, some of the ground that Mode Two is argued to have gained in respect to disciplinary science may even be being lost. One piece of evidence for this is provided by a study of the recent history of science funding in Sweden. A number of political challenges to the value of funding independent basic science arose in the early 90s. A special fund was set up to fund work addressing specific problems of concern industry to industry and environmental regulation. These funds grew to the point where they were seen as a challenge to the university establishment who were able to strongly reassert a commitment to basic research as an independent activity (Elam and Glimell 2004). Nor has the agricultural chemical regulatory network of scientists described by Rothstein et al. (1999) been developed an academic grounding in the form of a teaching program or academic research centre.

In spite of the many impacts on their working life that scientists reported resulting from the switch to more contract-driven research, Cohen et al. (2001) also conclude that their survey research did not uncover what they could call a clear, smooth transition to Mode Two science although this is what they were looking for and expected to find. Scientists recognized the importance of the changes but also argued that scientific discovery can happen anywhere, it doesn't matter that much where you start on the problem. While some scientists saw the shift to 'deliverables' as an unwelcome intrusion and a barrier to professional freedom and autonomy, others said that it improved efficiency, productivity, and relevance. Others scientists argued that the purpose of science is multifaceted and the patterns we are calling Mode Two science does not entirely preclude traditional activities (Cohen et al. 2001). Executive summaries and bullet-point recommendations were to some poor quality science while to others, when such reports were produced with scientific integrity, they were a prized new capability (Cohen et al. 2001).

3.1.4 The Practices of Co-production

In the literature reviewed here, boundary work and the various styles of co-production of science-based policies have emerged as the central concepts. Boundary work requires careful attention because what is required is a boundary that is both recognized and porous, but not too porous (Cash et al. 2002, Halfman 2003, Guston 2001b). Where this careful boundary work is being carried out is in the practices of co-production, practices that are often, though not always, carried out in institutions that were designed as, or more commonly evolved into, boundary organizations. Such organizations take many forms ranging from simple clearing houses that simply facilitate communications all the way to true science policy hybrids where the separate standards and forms of accountability of the two arenas are partially merged (Niederberger 2005). The practical foci of these hybrid institutions are boundary objects, i.e. the models, indicators, and collaborative research that are used to provide a way to structure discussions between scientists, stakeholders and policy makers and give practical form to a well balanced science boundary.

This final section focuses on the practices of co-production. Many of the authors being reviewed offer lessons about what works and what doesn't in trying to do co-production. I examine these lessons on three levels. The first part looks at the professional practice of individual scientists, the second at the level of institutional design, and the third part examines lessons about programmatic practice and relating to broader publics.

3.1.4.1 Individual Practice - A Learned Art

Of the three main theoretical perspectives we examined it is the philosophical Post-Normal Science (PNS) perspective that places the most focus on the individual scientist's practice of co-production. The Epistemic Community avoids boundary questions by bracketing uncertainty, while Mode Two science examines scientists in terms of how they respond to larger structural forces rather than in the details of practice. Funtowicz and Ravetz (1992) true to their philosophical form, cast the question of practice in epistemological terms. In a situation where uncertainty arises from inexactness (see Section 2.3 above) standard routines, i.e., statistical techniques or other conventions, are adequate and so technical practice is carried out in the traditional way. But when uncertainty arises from unreliability or boarder with ignorance then personal judgements based as much or more on scientific skills rather than on scientific knowledge are needed. In this case practice becomes a "learned art" a kind of scientific consultancy where the science boundary consists of scientists working closely with others helping them think through how to deal with the uncertainty (Funtowicz and Ravetz 1992).

The importance of this kind of activity is practically grounded, for example, in a need for continuous interactions to ensure salience in the face of uncertainties. The failure of the global biodiversity assessment, for example, was a result of a focus on the state of the art to the exclusion of needs of potential users (Cash and Clark 2001). There are several dangers in trying to maintain traditional styles of practice in uncertain conditions. Scientists over-sell their science, feeling that they have to show more confidence and authority than the situation warrants. This leaves them, in turn, facing exactly what they were trying to avoid: a continuous decline in respect for expert claims (Irwin 1995). Science policy settings in Europe and Canada often involve ongoing discussions with interest groups and these discussions help guard against attempts to justify value-based choices with post-hoc scientific arguments (Jasanoff 1986).

There is a real danger of loss of legitimacy, however, if scientists are seen as too close to one interest group to the exclusion of others. Gill (2001) argues that scientists must have strong influence on the overall process, which is not always possible, particularly in legal situations where many kinds of experts are used and scientists are simply specialists who represent the interest of the side for which they are witness (Wynne 1989a). Indeed, appropriate inclusion is particularly important if the science - policy issues are to avoid ending up in court, which one can think of as a boundary organization with an exceptionally poor design.

Consultant-type interactions between scientists, policy makers or stakeholders are clearly desirable for increasing the salience of results. However just as clearly these kinds of interactions immediately raise questions of credibility and legitimacy. The question is what kind of institution can be designed to allow this quality of cross-boundary interaction while maintaining credibility and legitimacy.

3.1.4.2 Institutional Design: Polycentric Cross-Scale Integration

For Cash and Clark (2001) the key concept that emerged from their review of international scientific assessments was integration expressed in two dimensions: the integration of science and decision making and the cross-scale integration of both. They argue that movement in these dimensions has been promoted on the technical side, while the social processes and institutional structures required have remained under studied. They, and others, call for increased attention from social science to help determine the role and effective means for science to contribute to problem solving (Niederberger 2005). The social science research required is comparative research on institutional design.

Cash and Clark's (2001p11) argue that the assessments that work have three common institutional features:

- 1) multiple connections between researchers and decision makers which cut across various political and organizational levels (polycentric networks);
- 2) boundary organizations - institutions that act as mediators between science and policy and across levels (sometimes the nodes in the network); and
- 3) sustained and adaptive organizations that allow for iterated interactions between scientists and decision makers."

Point two had already been extensively discussed here, and point three is in many ways a restatement of the need for intensive consultant / client-like interactions across the science boundary. Polycentrism, however, is a critically important idea not yet discussed in this review.

The concept of polycentrism comes from political science and is most closely associated with the work of the Workshop in Political Theory and Policy Analysis at Indiana University, a group which enjoys considerable influence on natural resource social science (McGinnis 1999). Ostrom (2001), working from a forestry model, defines polycentric systems as "the organisation of small-, medium-, and large-scale democratic units that each may exercise considerable independence to make and enforce rules within a circumscribed scope of authority for a specified geographical area." For Ostrom the strength of a polycentric governance mechanism is its ability to experiment with diverse approaches and provide for a range of responses to external shocks. There is a clear analogy to science-based policy making in conditions of uncertainty. Indeed, information processing including the access of the smaller units to various knowledge sources, their ability to process feedback from policy changes, and communicate these experiences to the parallel units is a key part of the strength of polycentric polities. Their down side is that they are complex, redundant and hence expensive, and resistant to top-down steering (Ostrom 2001).

The polycentric networks that Cash and Clark (2001) see characterising effective use of science in policy do so for quite similar reasons. They are able provide methodological coherence across scale levels while still allowing local specialization, the redundancy of the system provides for multiple pathways to encourage innovation and flexibility. A polycentric network also facilitates stakeholder capacity building and involvement (Cash and Clark 2001). It is an institutional design that gives form to the continuously operating process of debate and dialogue Funtowicz and Ravetz (1990) see as the next higher level in their description of the scientific practice needed for addressing uncertainty.

Many if not most of Haas (2004) lessons for the effective development of epistemic communities echo the polycentric approach (Table One). For example, number Five suggests emphasizing "networking ability" in recruitment. Number Six counsils scientists to "avoid relying on one national institution" while Number Nine suggests that they "avoid government designation of scientists to international meetings". Number 10 warns against the capture of the network by one scientific discipline or school. There are several others that point to polycentrism as well.

One question where the practicality of a polycentric approach arises is the degree to which policy makers should employ an "in-house" science capacity. In the private sector in-house science has actually declined because of costs and because high uncertainty means that firms are sure what knowledge they need (Gibbons et al. 1994). However, some have suggested that in policy matters this is a very important variable. Clark et al. (2002) report that how much a scientific assessment is carried out by or under the control of the subsequent users is one of the top three features in determining the future use of that scientific assessment. Alcock (2004) makes a similar claim in a study of the organization of fisheries management, while simultaneously pointing out that fisheries science under control of the management agencies also raises suspicion among stakeholders. His analysis, however, conflates in-house (in his term embedded) science with top-down management. He characterises pre-cod collapse Canada as embedded while calling the US system disembedded

because of the existence of the management councils. This characterization underestimates both the degree to which scientists have influence within NOAA Fisheries and the degree to which NOAA Fisheries has influence over the councils. His suggestion that the more independent science has a higher legitimacy among stakeholders would also fail in a comparison between North America and the EU where ICES is formally entirely independent of the Commission and where the legitimacy crisis in fisheries is particularly focussed on science, although it would be supported in respect to Norway where the legitimacy of science is high and the scientific capacity is formally independent of management (Schwach et al. 2007, Wilson and Hegland 2005). In all cases stock assessment research is mainly funded by the managers and in all cases many other factors are involved. The bottom line seems to be that what is really important in respect to saliency is the degree of control that policy makers have over scientists' activities, and, in respect to legitimacy, the perceptions of independence, rather than in either case the legal relationship between the two groups.

It is also the multi-level nature of the problems that science-based policy must address that suggests that a polycentric approach may be more effective. Even global changes are linked to local ecosystems and communities, and this implies a need for distributed research, monitoring and decision support systems (Cash and Clark 2001). The role of the higher levels in to hold the lower levels accountable (Wilson and Pascoe 2006) and to translate and communicate between the lower units. This latter role is one that Jasanoff (2002) argues would make the European Union a more effective governance structure that disseminated best practices rather than imposed uniformity. Haas' (2004) lessons (Table One) also suggest that it is critical to construct models so that effects are calculated at meaningful political scales.

Polycentric approaches involving science-based decision making are found in a number of arenas. Agricultural extension services have existed for a century and proven extremely beneficial. Independent certifiers in eco-labelling certification schemes are another good example. Cash et al (2002) point to the way water management on the United States great plains has developed a system of extension agents and management districts that act as brokers. These models may be of use in other science-policy arenas.

3.1.4.3 Programme Design: Evaluation, Consensus, Participation and Transparency

Developing effective institutional forms for science and policy only sets the stage, the design and implementation of actual programmes is where a difference can be made. Yet, as Harremoes et al. (2001) ask, how can programmes, strategies and actions be devised to deal with uncertainty, especially at the border with ignorance where things are not known?

An important planning principle is built in evaluation and reflection. Many potential problems can be anticipated even in conditions of uncertainty and the taking tentative, reversible action is important. Linked to that is specific research aimed at producing early warnings and addressed to blind spots in knowledge, and plan for the inclusion of multiple sources of knowledge (Harremoes et al. 2001). Provisions need to be built in to assessment programmes for learning and critical self-reflection that strike a balance among continuity in observation, cumulative experience in the assessment process, the current state of knowledge and the changing needs of decision makers (Clark et al. 2002).

A second principle is to search for consensus. The basis of Haas' approach is the power that a scientific consensus lends to problem solving. It is also reasonable to treat consensus as a programmatic goal. Several of Haas' lessons (Table One) point in this direction. Consensus is also one of the goals that Clark et al. (2002) suggest for boundary spanning arrangement. They should facilitate a consensus among scientists and decision makers over the questions to be addressed, the evidence and expertise needs, and the processes that scientific assessments employ. On the more public level, Jasanoff (2002) among others (Young 2004) warns against too strong an emphasis of

consensus. She believes that programmes like consensus conferences may overemphasize the finding of common ground. "Creative discensus", well managed by the authorities, might be more desirable (Jasanoff 2002). Others warn that the consensus can be imposed in the sense that everyone can be placed in a position where it is in their best interest to follow the consensus (Engles et al. 2006) and a fully public search for a consensus may even be something that can be manipulated to silence activists or other critics (Irwin 2006).

The third principle is participation. How to involve stakeholders, and especially the general public in science-based policy remains in many ways the most problematic question. The reason for doing so is the legitimacy that can be gained though public buy it, in some cases, such as medical information (or the management of dispersed resources), such buy-in can be critical to outcomes. In the past the public has often been approached ineffectively. In what is often referred to as the "deficit model" stakeholders and the public are treated as a group of ignorant individuals who can and must be educated to agree with existing science-based policies. Irwin and Wynne (1996) showed that that people draw their own distinctions about which subjects they feel they should or should not be ignorant about. Furthermore, while lay people are often assumed to desire and expect certainty and a lack of risk, they found no such naiveté. The public understanding of science literature in general tends to report on research that begins with what scientists (and the government) think they the public should know. The literature sees gaps in knowledge as undesirable almost by definition, even as a threat to science and invariably assumes that improved communication will raise scientific awareness (Jasanoff 2005). Yet sociological research has shown that the public is culturally knowledgeable and able to master complex issues when those issues are important to them (Michael 1996, Jasanoff 2005, Yearley 1999). Irwin (1995), for example examined an announcement designed to allay public fears of mad cow disease, a famous case of a failed programme of dealing with public in respect to a scientific issue. He found that the announcement relied on authority derived from sounding certain and assumed that everyone would agree that 'science' was the central aspect of the problem. The tone of the message was an implicit hierarchy that said basically, we are in the 'best position to know' so trust us.

Her analysis and much of the literature on public participation in science, however, suffers from a heavy reliance on Wynne's (1989b) analysis of sheep farmers and radiation. This example often appears in the literature as an example of the "public" in general, but the groups involved actually had a great deal of experience-based knowledge of the questions involved. Where stakeholders have considerable experience-based knowledge, as will be the case in many arenas of science policy involving industries, farmers, fishers, etc. boundary objects such as collaborative research and data collection efforts take on a great deal of importance.

The fourth principle is transparency. The need for transparent decision making in a democracy is the reason for having a science boundary in the first place (Porter 1995, Ozawa 1991, Wilson and Pascoe 2006). It becomes particularly critical where uncertainty is high because of the need for openness about the limits of scientific knowledge (van der Hove 2007). This is a substantial challenge, given Funtowicz and Ravetz (1992) argument that uncertainty shifts the scientific role from "knowing-that" to "knowing-how". While knowing-that can be easily articulated, knowing-how is about skills and is rooted in tacit knowledge. Knowing-how finds expression in consultant-type roles and this kind of role is so close to advocacy that it threatens to entirely undermine the legitimacy of the science, and even raise questions about its credibility. Transparency, in the form of involving several parties with different interests involved in the "consultancy" relationship, seems to be the only way to gain the advantages in terms of dealing with uncertainty while maintaining legitimacy and credibility. Hence this principle is a good way to understand the motivation behind establishing a broad, poly-centric network. However, Lenhard, et al. (2006) transparency is also derived from using established disciplinary standards, and as Jasanoff (2002)

points when poorly structured, especially with formal “openness” requirements, it can reduce legitimacy.

4 Conclusion

The central idea to have emerged in this review is that the science boundary must be recognized and respected while at the same time remaining porous.

The purpose of recognizing the science boundary is not to maintain scientists’ authority, even if it too often gets constructed that way. The purpose of the science boundary is in the contribution it makes to democratic decision making. Trends in recent scholarship to the contrary, there is a distinction, an extremely important distinction, between statements about nature, which can be established *in principle* as objective facts, and statements about the shared meanings that make up social phenomena, which will always have the status of interpretations. The “post-modern” confusion seems to be that the existence of uncertainty reduces or even vitiates the importance of this distinction. The error in this interpretation is that it misses the fact that the “in principle” is what is important for democracy: if we are to all consent to a rule about how we are going to live together the basis of that rule must be a fact in nature not the interpretation of a shared meaning. In the legal system, the duty of a court is to establish facts not to gauge opinion, even if “beyond a reasonable doubt” recognizes that uncertainty is unavoidable. In the end policy is about making effective laws, and recognizing and respecting science is about ensuring that those laws meet the standards of justice and democracy.

The science boundary must also remain porous, but not too porous. In policy areas dealing with uncertain science we cannot establish for any given concrete policy decision *a priori* on methodological grounds where the science boundary is going to lie. There are too many questions that can be asked, too many indicators that can be selected, and too many political problems needing facts to aid their resolution. Where this has been attempted the effective uptake of science to policy has been weak or even failed. With the possible exception of arenas where there is little or no uncertainty and therefore full scientific consensus, scientific deliberations need to include groups beyond scientists if policy is to be successful. This inclusiveness and the discussions it creates is the concrete expression of the metaphor of the porous science boundary.

The three values of credibility, salience and legitimacy have emerged as important tools for maintaining this balance between recognizing and respecting the science boundary and ensuring that it is appropriately porous. All three are required for effective science-based policy and all three are affected who is included and how much they are included in scientific deliberations. These linkages often take the form of trade-offs between the three values, providing a sort of negative feedback indicating that that science boundary has become too rigid or too inclusive.

The three main theoretical approaches discussed treat the science boundary quite differently and the strengths and weaknesses are illuminated though examining their implications for credibility, salience and legitimacy.

The Epistemic Community approach has demonstrated that in areas where there is a strong consensus among scientists of both the facts and their policy implications that communities of scientists are able to bring about effective environmental management regimes even in the very difficult area of international agreements. The Epistemic Community approach, however, also suggests that if uncertainty is high that the only real response is more research. So the implication from a credibility, salience and legitimacy perspective is that extremely high credibility is a

resource that can be used, given sufficient organizational skills (Table One) to create the salience and legitimacy needed to bring about effective policy.

The Mode Two approach argues that there is a high and growing degree of external control over science in ways that have not been true in the past, and explores the implications this control for science. The main implications are greater goal orientation and more trans-disciplinary science. The external control itself ensures that the salience of the science for the user groups will be high. However, the lack of scientific independence pushes in the direction of low legitimacy. The discussion Mode Two science and quality control also raises serious questions of credibility. The observations that suggest that Mode Two science may not be as widespread or radical as initially argued is perhaps a hopeful sign for developing effective policy.

Post-normal science only purports to be valid in situations where both policy stakes and uncertainty are high. In such a situation, it calls for a very inclusive science boundary focused specifically on ensuring scientific quality through both an “extended peer community” and by calling on scientists to emphasize their skilled “knowing-how” rather than their learned “knowing-that” in working with policy makers. Like Mode Two, PNS sets the stage for high salience though a close working relationship, but also potentially high credibility through the emphasis on increased quality. The very close interaction – an extremely porous border – however raises serious questions about legitimacy. The idea of polycentric networks that is highlighted by the work of the Global Environmental Assessment Project is one way of addressing this problem. A number of dispersed interactions that are fairly but not overly transparent, and in which all of the main interest groups are involved, would be an institutional form that may allow close interaction without a damaging loss of legitimacy.

The other two important ideas to emerge from this review are boundary organizations and boundary objects. The idea here is to give concrete form to a well balanced science boundary. Many scholars have noted the emergence of such organizations along the policy interface that facilitate communication and the co-production of science and policy. Others have noted that where scientists have found themselves trying to negotiate these things as individuals that it is a difficult and painful process. The use of boundary objects where scientists, policy makers and other stakeholders work together on common projects has also been found to be effective in a number of studies.

Part of the mandate of SAFMAMS is to distill lessons from this review for science in the support of the CFP. I believe there are many such lessons, many of which I have not seen and others which a larger group would be needed to develop. Fisheries is also in many ways a unique case that has much to teach as well as much to learn from other policy arenas. Hence I hesitate to make any statements in the form of “global experience shows that we should do X in the CFP”. So I will content myself at this point to simply raise some questions about possible connections between the ideas that emerged in this review and my observations about what is happening in the CFP.

1. Is STECF a proto-boundary organization and if so should it be encouraged to develop more intentionally in that direction? What about ICES which has tried very hard to maintain itself as an independent science organization? Should some activities of these organizations seek out more non-scientists?

2. Interactions between and among scientists and RACs are already creating ad-hoc working groups of various sizes that are responding in a flexible way to specific management problems. A polycentric network is emerging. In my view it is particularly impressive how these structures are able to mobilize knowledge at specific scales appropriate to the problem. Yet their ad-hoc nature already means they are straining for even fairly low levels of resources. Should DG Fisheries, ICES or STECF take a formal relationship to these informal working groups? If they do is there a danger they would lose the flexibility? How can these groups be supported and made more effective?

3. The nature of fisheries means that it is easier than in many arenas to develop boundary objects and this is happening rapidly in the CFP. The Fisheries Science Partnerships and other forms of collaborative research and the development of FLR scenario-based models are two important examples. How, if at all, should ICES and STECF structure their relationship to these efforts?

4. We have had at least one extended, positive experience with a true boundary organization in the form of the activities around the North Sea Commission Fisheries Partnership. Should this be continued? Should this kind of effort be replicated elsewhere in Europe?

5. Science in support of the CFP has a number of issues with control. The relationship between ICES and STECF has remained unclear. The ICES working group structure has often proven inflexible and difficult to guide due to its size, multiple mandates and complexity. This is complexity that will only increase as we move toward an ecosystem approach and its demands for cross-disciplinary work. Are there ways the scientific support can become more focused and disciplined without running into the problems with credibility and legitimacy pointed to by the literature on Mode Two science?

These are all good questions and other questions will emerge. I believe it is well worth the while of people addressing fisheries to keep an eye on what is happening in other arenas where science and policy meet.

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