Science-Policy Interaction, Constitutive Policy Making and Risk:

The Development of U.S. Nuclear Safety Study WASH 1400

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> On public policy making, many suppliers and users of social research are dissatisfied, the former because they are not listened to, the latter because they do not hear much they want to listen to.
>
> Lindblom and Cohen, *Usable Knowledge,* 1979, p. 1

The scholarly study of the science-policy relation and of the roles of experts in public policy making has been growing in the academy for some time now (Holzner *et al.*, 1983; Jasanoff *et al.*, 1995). One of the central notions in this area of study is that there is some kind of information flow between people who are primarily researchers and people who are primarily policy makers (administrators, politicians). The limits to a "clear communication" and use of data, the problematic interrelationships and intervening power structures, and the politically negotiated knowledge (use) that result are all well documented aspects of this process (Weiss, 1983; Elzinga, 1985; Gibbons *et al.*, 1994).

Much of the research, especially that inspired by Science & Technology Studies (STS) has proceeded to deconstruct the epistemological distinction between science and policy and further argued the demise of "truth speaks to power" (cf. Wildavsky, 1979), or of the received conception of the role of science in policy. The critique of science as a practice guided by the norms of proper conduct as laid down by Robert Merton (1973) and guarded by the scrupulous authority of its method has had great impact on our understanding of scientific practice.[1] Scholars have paid attention to the distribution and use of scientific knowledge in political settings, and often pointed to the socio-cultural/political contingencies of science and to its negotiatedness when used as a tool of persuasion rather than enlightenment (Schomberg, 1993; Wynne, 1994).

In spite of this powerful critique of science as a disconnected entity the analysis has mostly been aimed at science as an epistemological body. A certain type of knowledge has been investigated and deconstructed rather than a functional locality. After spending years of effort showing the situatedness of scientific knowledge, the guiding image of the object under scrutiny still seems to be the republic of science, if not epistemologically certainly still with respect to its structural-functional location in relation to politics. There have been attempts with notions of hybrid communities (quangos residing in the intersection of science and policy), mandated science, post-normal science and trans-science to mention a few. These concepts denote a science operating to some degree according to the interest of corporate policy (whether state or private), an issue oriented inquiry not easily distinguished from applied science but certainly not basic science. What these concepts leave us with is a bipolar distinction between science and policy very much like the "two cultures" concept of C.P. Snow, added elements do not bring qualitatively new dimensions to the distinction, it remains bipolar. The present article will argue that this is a poor way of describing the structural and functional location of the scientific in today's policy making arena.

To make this clear the article will proceed to present some popular models of the science-policy relationship or *dialogue*. It will be shown how these models rest on a bipolar understanding of the relationship between science and policy. The article will further discuss the functions of uncertainty in policy making that come to affect these models, and posit an alternative understanding of the problem where science is functionally interwoven in a knowledge constitutive policy process. This particular outlook is then played out in a dynamic and pressing field of policy analysis, that of risk assessment and management, and as an extended example to this effect a case history of the emergence of probabilistic risk assessment in the U.S. is provided.

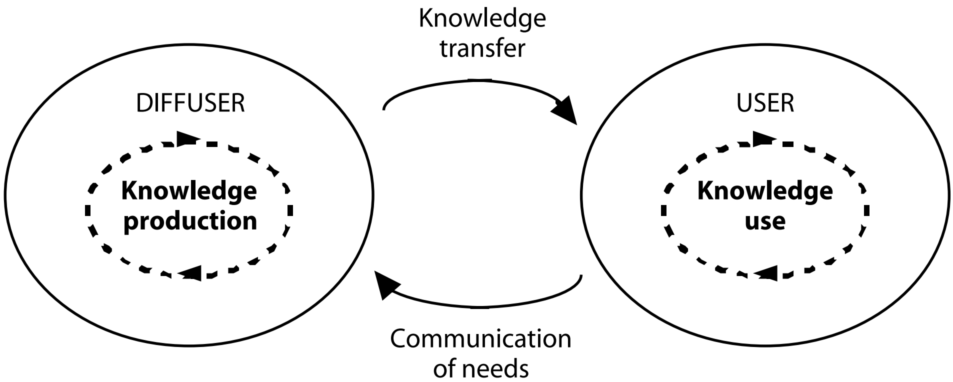
Finally, the concept of "constitutive policy making" is borrowed from Lasswell and developed for the purposes of understanding how the functional-structural relationship between science and policy is constructed under similar circumstances.

Science-Policy Communication: Traditional Accounts

What is presented here is an overview of a number of positions taken on the issue of science-policy interaction. Far from being fully exhaustive they are meant to represent a cross section of some of the more influential positions in the field.

Already in the 1930s Paul Lazarsfeld and Kurt Lewin came to represent what may be called "the received view" or a "rationalistic" account of how research is used in policy. Both believed that there was a body of knowledge, instruments and understandings that could alleviate problems of a civic nature - that science could guide social affairs and, to some extent, that policy would let it. The model that grew out of this tradition, the RDU model (Research, Dissemination, Use), consists of a simple input-output model of research use, as seen below.

Figure 1. Diffuser and user of scientific knowledge



This model of research utilization is still very much in use among policy researchers and policy practitioners, although during the last decades its simplicity has been criticized from several standpoints especially from STS camps (Nelkin, 1984; Hellström, 1997) An initial critique was that of the model creating expectations that real life science could never meet. Here the problem was often seen to lie in the distance between social problems of conflict or neglect and the ability of science to provide credible, usable and reliable solutions.

> How, for instance, could one expect that a given study would have an immediate and direct application, would be authoritative enough to alter institutional objectives, or could supersede the play of partisan interests? (Weiss, 1980).

The "classic" model has also been criticized on other accounts. If we look at it briefly, it is easy to make out its assumed progression of knowledge. The researcher "produces" knowledge, and then "transfers" it to a "user" who "uses" it. The strongest academic challenge here is directed towards the model's hyper-rationalism (Huberman, 1994). STS workers have recurrently pointed out, in different forms, that research is often carried out in the name of a single perspective or ideological frame of reference. It is then "transferred" in ways that assume its *de facto* validity. Put more dramatically, the research community can be seen as trying to define what is real for communities of practice. This points to the "bargained" nature of research knowledge, the use of which is invariably strategic in the social setting in which it is introduced (Knorr-Cetina, 1992).

This critique has been backed up by another research-to-practice dilemma. Lindblom (1990) and Lindblom and Cohen (1979) have argued that scientific research hardly ever provides direct answers to policy questions. At best, it provides ideas and insights; it highlights new features of a situation; it gives new conceptual handles to look at familiar problems. In this view, knowledge does not "transfer" directly into policy decision, and its use or non-use cannot be predicted. Carol Weiss, in a study of political decisions, calls this process "knowledge creep". New ideas and concepts *percolate* gradually throughout the policy community and come to shape the way decision makers think about their work (Weiss, 1980). The various factors of *uncertainty* that are present in this perhaps more realistic account of research utilization are not dealt with in the traditional model.

Lazarsfeld and Lewin's model spawned a critical discussion of the science-policy relationship where many aspects of the original RDU idea were contested. In spite of this, the bipolarity of the model continued to be taken for granted in successive studies. Weinberg (1972; 1993) wrote influentially on the phenomenon of *trans-science*, a concept that would gain much attention in STS work (cf. Jasanoff, 1990). *Trans-science* Weinberg argues, is an area of politically relevant inquiry where scientists often disagree on issues. The reason they disagree is that science has formulated questions that can only be appropriately answered by employing political (value) judgment. Accordingly, politics does not impose on science, only on trans-science. In a way then, the concept of *trans-science* socially immunizes the "real" scientific sphere from being colonized by politics, even on controversial issues. The resulting idea comes close to and emphasizes what Don K. Price (1967) called "the scientific estate". In this respect, Weinberg (1993) and Shrader-Frechette (1995) has written extensively on regulatory issues concerning nuclear power, an area where a well-defined scientific community has the ability to lend political strength to contested standpoints.

In a much-cited text, *Science Speaks to Power* by Collingridge and Reeve (1986) the two worlds of science and policy are recounted in what they refer to as the *over-critical model* and the *under-critical model*. Collingridge and Reeve challenge the view that policy authority depends on scientific consensus to make knowledge dependent regulatory decisions. Instead of taking science to be an independent prior variable to such decisions, scientific uncertainty or disagreements will not necessarily compromise policy effectiveness. They argue instead that science is used either to legitimate a policy already supported for political reasons, or if it fails to do so, new knowledge will simply be discarded. As a result, science constantly finds itself supporting more than one standpoint in, for instance, regulatory disputes. Notable examples here are the ozone-controversy and the global warming debate (Nolin, 1995; Elzinga, 1996).

Thomas Gieryn has coined the term *boundary work* to depict the ongoing attempts by the scientific community to prevent the control of science by outside powers (e.g. the policy community). This *boundary work* aims at erecting a wall between science and policy that will ensure scientists privileges and at the same time secure their resources and to some degree control policy decisions (Gieryn, 1983; 1995). Gieryn calls it "keeping politics near but out", a kind of demarcation technique based on social structure rather than an epistemology of method. The fact that the scientist has to negotiate credibility and support with both scientific peers and policy makers has been taken up by Star and Griesemer (1989). They propose the mechanism of a *boundary object* as being vital to the understanding of how heterogeneity in perspectives and practices of different actors (scientists and policy makers) does not necessarily get in the way of science-policy communication. The *boundary object* then may be a concept with an envelope of interpretation that fits both scientific and policy interests. It might be a flexible concept such as "greenhouse gases" or a catchword like "sustainability". It enables science to gain clout in the policy sphere, and lends policy makers scientific legitimacy with a minimum of constriction. The notions of *boundary work* and *boundary objects* certainly confound the picture of the science-policy dialogue substantially, but still retains the bipolarity of the two communities.

Sheila Jasanoff has studied policy relevant research and extended the *boundary work* concept in two directions (Jasanoff, 1987). She depicts two strategies that both point to some tension of legitimacy between the scientific and the political. In some situations, uncertain and controversial areas are excluded from the scientific domain and deemed unscientific. This limits the scope of science, but makes it, at the same time more authoritative. In other situations science may extend its domain as it tries to turn political questions into scientific ones. At the same time the policy field makes use of scientific expertise in an advocacy like fashion, pushing for ideas emanating out of political interest. In this way, science and policy are involved in a constant *co-production* of factual accounts and scientifically labeled policies (Jasanoff, 1990).

From having seen a model of science-policy dialogue based on distinct community boundaries and clear communication (RDU), a picture based on mutual negotiation, contestation and overlapping has evolved. The simplicity of RDU has certainly had great impact, but has also been well critiqued, and rightly so. In spite of this however, we are still dealing with two communities, two areas of inquiry and conduct. Why, one might ask, do we need to keep this duality given that scientists and politicians seems to be doing basically the same thing anyway? The truth is that while epistemologically the two communities gradually are analytically equated, their "informational" location within societies decision making structure still remains the same as Lazarsfeld's RDU, a situation that does not necessarily map reality. In the following an alternative account of the location of scientific authority and of its position in policy making will be given. This account is based on a notion of a process of integration between forms of knowledge and policy authority, where uncertainty as to *information*, *norms* and *action* plays a strategic role. The area of risk regulation is outlined and used as an example of such integration.

Risk, Uncertainty and Policy Making

The concept of risk is first of all a regulatory concept, and in that maybe more than anything else an expression of political structures and norms. Identifying what is "risky" is only a small step in the regulatory process, and does not in itself produce a *de facto* political outcome. Risk assessments have to be evaluated by politicians and administrators before they can gain operational significance.

Questions such as "what hazards are relevant for whom?", "what should the trade-offs look like, should there be any", and "what kind of uncertainty in assessment data can be justified if risk mitigation turns out to be costly?" are typical of what Alvin Weinberg calls *trans-scientific* questions. Questions like these are initiated by science, but can only be answered in a political language (Weinberg 1971; 1993). The very nature of risk also implies scientific uncertainty (risk has actually been said to be attempts at "quantified ignorance" (Morgan & Henrion, 1990), it implies a moral uncertainty (whose well-being takes priority when trade-offs have to be made?), and finally it implies managerial difficulties since the above tend to lead to stakeholder dissensus and conflicting decisions in the policy process.

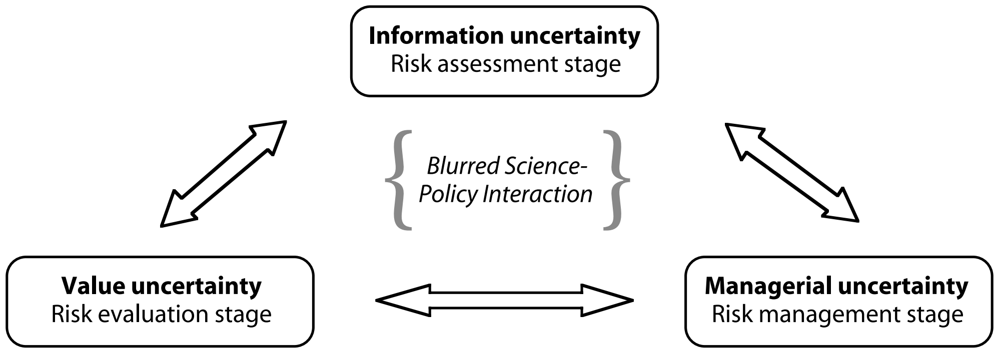
These forms of uncertainty may be connected to a three-stage model of the risk analysis process, where a distinction is made between assessment, evaluation and management. *Risk assessment* is taken to be the stage on which identification, measurement and characterization of threats to human (and environmental) welfare are made. Procedures are employed that canvass the spectrum of threat that could exceed maximum impact thresholds, determine location of risks and assess potential consequences. Risk assessment has traditionally been regarded as a predominantly scientific activity with little or no political infusion (Shrader-Frechette, 1993).

Risk evaluation on the other hand is a clear cut socio-political process where risk information is brought together from expert and lay sources for the purpose of making regulatory decisions. This stage corresponds to our moral dimension in that policy makers now are required to consider laws, customs, values, attitudes, ethics and preferences in deciding how the risk material should be treated.

The output from this process goes into implementing *risk management* strategies, which basically is the carrying out of policies and techniques to influence the generation and impacts of hazards. In the management stage, individual hazards may be prevented by eliminating agents of loss, improving preparedness, and by modifying risks, exposure and vulnerability. Risk management is also often carried out "after the fact", by mitigating losses via relief-and-recovery measures (Blaikie *et al.*, 1994).

These model stages are not independent. The potential of the management phase influences the focus of data assembly and must be considered when evaluating risk assessments. In the same way, effective management is always dependent on assessment data and *vice versa*, assessment data relies heavily on past records of hazard management for vulnerability analysis. Thus it is easy to see how these stages are practically and analytically intertwined, constantly interacting and mutually determining each other. Scientific assessment tends to be framed in political language, and political decision ends up being "baseline data" for scientific analysis. Thus there is a "mutual dependence" of risk research and political context (Rip, 1986). Interaction between science and policy becomes blurred and uncertain and leaves the decision maker with a demand for other sources of information, heuristics etc. to fill the gap.

Figure 2. The uncertainties of risk analysis, and blurred science-policy interaction



The "risk analysis cycle" above denotes an interaction between analysis and policy making clearly seen in the *insurance sector*. Here, the risk management stage may be represented by the management of insurance portfolios. Establishment of fee structures is related to the "riskiness" of the client (*evaluation*) in terms of activity and property, and is accessed via a limited form of risk assessment (normally interpretation of actuarial statistics etc.). Potential reduction of insurance fees has proven to be a good incentive for risk limiting measures (*management*) on the part of the client (Kunreuther, 1982). Furthermore, the monitoring of risk reduction presupposes measurement of risk, not only at the level of data gathering but sometimes through the development of new methods such as surveillance and time series analysis (*assessment*). This peculiar interaction of scientific information and policy interest will be exemplified in greater detail below.

Pre-History of the "Rasmussen Report"

There is a clear similarity between insurance rate setters and environmental regulators when it comes to measure "riskiness", evaluating "client performance" and providing risk reducing incentives/managing risks. In fact, the two areas intersect in the field of risk analysis most notable for the development of modern probabilistic risk assessment, i.e. the nuclear energy field and the "Rasmussen report". (Atomic Energy Commission, 1975). Rasmussen's classic 1975 study on the safety of commercial light-water reactors was in part stimulated by the need to set a damage level for the insurance industry to base their rates on, but the development of this particular study had a pre-history. In this case a risk assessment for US nuclear reactors was available already in 1957 (Atomic Energy Commission, 1957). The WASH-740 report prepared by Brookhaven National Laboratory was commissioned by the Atomic Energy Commission and had as a main purpose to establish the number of people killed and maimed and amount of property damage incurred should a worst case scenario reactor accident take place close to a large city.

The purpose of this study was to assess the economic feasibility of the US government going in with a so-called war-risk insurance to cover for potential damage since insurance companies only where prepared to cover small parts of the loss. The nuclear industry on the other hand refused to develop civilian nuclear power production unless it was assured adequate coverage. A proposition on the maximum coverage for nuclear energy related loss was put forward by Congressman Price and Senator Anderson, i.e. the Price-Anderson Act (cf. Marrone, 1977). The amount, $560 million would be guaranteed by the Federal Government with $500 million and the rest by private companies (the insurance industry put up $56 million). Congress however was reluctant to carry this responsibility, and thus some assessment had to be made of the quantitative uncertainties surrounding a potential disaster.

So, to achieve a specification on the amount of money actually to be risked by the federal government some assessment of probability of damage was necessary. This proved to be a difficulty for the Brookhaven group and it was never achieved in the WASH-740, yet a couple of months after its release in September 1957, the Price-Anderson Act was passed. In 1967, a revision of the WASH-740 report was initiated by the renewal of the Price-Anderson Act and by a proposed expansion of civil nuclear power. The Atomic Energy Commission was again the commissioning authority. This time a major task for the Brookhaven group was to estimate, not only maximum damage to people and property, but also the probabilities of a disaster. Such an estimation was considered necessary for a proper monetization of the risks. Brookhaven refused to do this, arguing that data was too scarce to make probability estimates, and that only "fringe members of the statistical community" would attempt such a task (Fuller, 1976).

The Atomic Energy Commission noted that an extensive report on the assumed low probability of an accident was necessary to compensate for its expected consequences as laid out by the Brookhaven group, not to mention the benefits for liability experts of having dollar estimates assigned to such an event and distributed over reactor/years. In spite of extensive pressures put on the Brookhaven group to conform to Atomic Energy Commission demands this did not happen, and eventually cooperation between Atomic Energy Commission and the Brookhaven National Laboratories was put to rest in this respect. But the quest for probabilities went on and in 1975 the Rasmussen report (WASH-1400) was released.

The Rasmussen report was prepared as background for the second renewal of the Price-Andersson Act in 1977. Atomic Energy Commission allotted $3 million for a probabilistic risk assessment to be carried out on civil nuclear reactors under the direction of MIT physical engineering professor Norman Rasmussen. The report (WASH-1400) attracted much critique as well as praise, its assessment techniques being both new and controversial and Rasmussen himself being a known proponent of nuclear power. Rasmussen took some of the techniques of probabilistic risk assessment that had been developed within other areas of industry and applied them systematically on a sample of US nuclear power facilities. His group made use of cutting edge knowledge in fault-tree analysis, where generic failure data for individual components could be aggregated to calculate the probability for a series of events leading up to reactor malfunction and major accidents.

Even though the WASH-1400 study has been recognized as a landmark in modern risk assessment, its scientific reception was somewhat divided. A number of criticisms was launched, some of valid technical nature, and some more adversarial. Therefore the US Nuclear Regulatory Commission (NRC) initiated an evaluation study led by Harold W. Lewis, a professor of physics from University of California at Santa Barbara. The resulting "Lewis report" (cf. Lewis *et al.*, 1975), lent strength to many of the technical criticisms directed at the Rasmussen document. Among other things criticism included the ways in which WASH-1400 had overlooked multiple failures resulting from common causes, and the ways in which uncertainties were propagated and interpreted in the analysis. It also emphasized the role of the human element, such as crisis reactions and adaptability. As much as being a critique, this second report firmly embraced the methodology of the Rasmussen team, and functioned more as a "refiner" than as a "debunker". So even if a result of the "Lewis report" was that the NRC distanced itself from WASH-1400, today's NRC is firmly dedicated to probabilistic "safety objectives" apart from their traditional "qualitative safety goals" (Fuller, 1976).

The interplay between stakeholders such as the nuclear industry, Congress and other governmental bodies (NRC, Atomic Energy Commission etc.), the insurance industry and academe is complex, the actors mutually determining how risk assessments should be made, how they should be evaluated and, by extension, how risks should be managed. As seen in the example above, the regulatory, uncertain and political nature of risk makes it necessary to operationalize as part of a process of "mutual adjustment" between involved stakeholder groups.

Current risk research and management therefore, is not an outgrowth of an RDU-like input-output process where science "speaks truth to power" and where analytical scientific knowledge is "converted" into regulatory action. It is also a clear example of how the above mentioned bipolarity breaks down and ceases to be an accurate description of science-policy interaction. Understanding how risk information comes to be used in the policy process may instead be to understand the process of creating policy knowledge out of stakeholder positionings. In the next section we will look closer at how knowledge is distributed in policy making.

Knowledge and the Policy Process

The above contingencies seem to confound an assumption underlying the RDU-model, namely that more knowledge about a given risk, even if that knowledge is packaged to aid decision making, will necessarily lead to greater rationality, more consensual decisions or speedier utilization. It also confounds the assumption made by opponents of RDU, that science retains integrity and agency and has the given ability to change policy outcome, not necessarily in a rational direction, but change it *per se*. Why might this be? One important insight to be drawn from the Rasmussen example is that the development of ever more precise methods for creating policy data is far from enough when solutions to real problems are sought. Instead the critical focus should lie on understanding how knowledge is incorporated in real-world policy making. A long held insight in the policy sciences is that decisions never stem from one single source or actor in the policy process. Rather, decisions emanate as part of a multi-stage process involving several contending interest groups and stakeholders all trying to advance their particular preference (Lindblom, 1980; Stone, 1988).

Information may be created independently, but disseminated in a form suitable to the people using it. Further, scientific information may not affect individual decisions, but alter *the very process* of making policy within a certain area. As seen above, this can lead to a situation where an attempt to keep the "scientific purity" of a risk analysis (the Brookhaven group) actually counteracts such input into the policy process. The Brookhaven group's unwillingness to assign probability estimates marginalized them as information sources, and indirectly set the stage for the paradigmatic influence of WASH-1400.

Information is indeed important to the policy process, but not necessarily in the traditional decisionist sense that underlies the bipolar distinction. Possession of information may be a source of political power, but for that very reason information must be able to support a multitude of potential standpoints (Brewer & deLeon, 1983). As a result scientific information has little integrity when put to use in the decision making process. In the quest for political advantage, interest groups may distort and exaggerate information, put information forward that from an internal scientific perspective may be of low technical quality. In this sense, information is never neutral and is seldom found in a neutral spot as far as institutional structures and power relationships are concerned.

When information is sought in order to settle trans-scientific issues of risk, where the scope is both of a scientific and moral (distributive) nature, or generally where both decision stakes and systems uncertainty are high (Funtowicz & Ravetz, 1992), the policy process may be said to take two distinct routes. Harold Lasswell has formulated a distinction between "ordinary policy making" and "constitutive policy making" to this effect (Lasswell, 1971). Ordinary policy making comprises deliberations and decisions on issues within a *given* structure where the role of stakeholder positions in the process are fairly recognized. In constitutive policy making on the other hand, deliberations and choices focus on *how* policies should be made and *who* should be included in the process. Theorizing about constitutive policy making implies going beyond the day to day operations of ordinary policy making and instead focusing on how institutions, analytical techniques, actors and procedures get selected (Lasswell, 1971).

The pre-history of the Rasmussen report was very much an example of constitutive policy making in the sense that information came to affect the policy process itself that then in turn changed information. According to Lasswell, process constitutive changes include the following:

- demands for greater use of certain cost-benefit/risk-benefit or optimization routines;
- changes in the decision making arenas;
- changes in the relative power of administrative or political institutions;
- changes in the skills required by actors (researchers or otherwise) to be able to influence policy;
- changes in what is considered to be legitimate values, and thereby in the kind of arguments that are considered persuasive.

The first point is particularly relevant to the example above. An obvious result of commissioning the Rasmussen report was a greater dominance of probabilistic techniques in nuclear risk assessment which as an extension came to lay the ground for risk-benefit analysis in hazard appraisal inside and outside of the nuclear field (Gillroy, 1993).

Technical analysis of this kind is, of course, only one part of the informational input. Parallel runs the expression of opinions of policy entrepreneurs and a host of other non-technical sources for molding decision makers attention. As quantitative forms of risk assessment become more dominant, the perception of the policy makers may shift on vital issues as a result of the perceived importance of the new tool at hand. The presence of credible risk assessment techniques will in turn increase the demands on project managers and policy evaluators to use these techniques in an integrated way. Decision routines that fail to incorporate the new techniques run the risk of losing credibility. The technique originally developed by scientific researchers has now created a policy culture, with its own forms of information, decision making routines, and claims.

Turning to Lasswell's remaining sources for constitutive policy making, we see a shift in perspective to more *prima facie* institutional forms of change. The arena of decision making may change as a result of introducing new forms of information. When specific policy assessment routines are adopted explicitly in, for instance, governmental agencies, policy deliberations tend to become more centered on actors within government, especially on experts and top-level administrators (Hellström, 1997). The capacity of a decision maker to invoke one single formalized ("algorithmic") procedure for establishing the right choice, may increase the legitimacy of a decision, in contrast to a situation where several actors agitate on behalf of "their" own formalized procedures.

In this way a decision model lends security to decision makers. Certain uncomfortable information may be integrated in, for instance, the risk-benefit analysis, without necessarily having to pose a threat to the preferred decision as such. The anti-nuclear lobby may want an estimate of the cost of deep disposal of nuclear waste to be recognized in a risk-benefit analysis, but if this estimate is used in a "comprehensive" assessment where such costs are assigned a low weighting, then the political impact of such information is eroded (Whittington & MacRea, 1986) .

In spite of governmental willingness to capture and incorporate weights that would be assigned by certain non-governmental actors, their preferences may be truncated. If a government agency wants to incorporate the "public view" on a certain issue, one often used technique is *contingent valuation*. This technique uses sophisticated survey instruments to pin down how the public values different resources and benefits of the commons (such as safety). Contingent valuation is a much applied technique, and when government actors consider the valuation adequately performed they may characterize it as a fair and sufficient input into the cost-benefit calculation. At this stage additional input from stakeholders left out of the valuation becomes extraneous to the valuation. This is an example of how a certain type of data generation comes to develop a kind of political integrity in its own right.

The constitutive policy process involves the relative importance of *skills* and their incorporation in the policy framework. Skills associated with measuring risk and manipulating the risk calculus will count for more than skills required to interpret society's responses to risk generating activities, such as location of a nuclear facility. This in turn may lead to a "market response" for competitive advantage where interest groups that are threatened by exclusion from the debate hire the necessary expertise themselves, thereby inadvertently forcing an adoption of new skills to take place in government in order to retain authority.

This brings us to Lasswell's last point, namely the legitimacy of assertion of rights. Cost-benefit and risk-benefit analysis incorporates a quantitative framework in the policy process, and in doing so also comes to undermine the legitimacy of certain values. For instance, in pollution control the value of protecting certain species of animals and plants can only be accepted on faith. A right, by its very definition implies that it retains a priority independent of a valuation framework based on monetary trade-offs. To assert that a species has the right to survive is to claim the irrelevance of weighting based on costs and benefits, and even the most savvy cost-benefit analyst can not quantify either costs or benefits for non-human entities. Even without taking this bio-centric aspect into account, one may argue that no risk assessment can fully and truly reflect all aspects of social welfare. The inability of the risk-benefit analysis to deal with the concept of rights leaves the policy maker with a host of ethical, cultural-psychological and legal aspects that will have to be confronted before the assessment can be used.

Knowledge about risks, be they associated with nuclear power or otherwise, is constitutive of, and constituted by, evaluative and managerial components of the policy process. This forces our original question: what role does science really play in policy making? Should we talk about a rational constitutive scientific component when assessing issues like risk, or should we re-assess the role of science and focus on risk analysis as a policy complex of emergent norms and decisions where science and politics are mutually dependent? In the next section some suggestions will be made as to where some of the actual sources of information for policy makers might be found.

What Constitutes Constitutive Policy Making?

If not science, then what drives constitutive policy making? The idea of dividing the policy process into two streams, one that deals in everyday activities and well defined areas of policy making, and one that aims at constituting the very process itself, is that when setting the premises for real life decision making strict routines like risk analysis always leave a "window of opportunity" open for the policy maker. This window of opportunity is created where analysis leaves questions to be answered. Where does the policy maker engaged in constitutive policy making turn for these answers? Shortly, what constitutes constitutive policy making?

This question will be addressed in two steps, first by looking at why policy makers need policy related knowledge at all, what their motives are for utilizing such knowledge once it is available, and second, what kind of questions policy makers need to pose to be able to make use of knowledge. The first issue is dealt with by Caplan *et al.* (1975), in an interview study with federal bureaucratic officials in the US. In this study a distinction is made between "instrumental utilization" and "conceptual utilization" of policy knowledge. The study reports the following frequency ordering as to reasons to use information.

1. Sensitizing policy makers to social needs,
2. evaluating ongoing problems,
3. structuring alternative policies,
4. implementing programs,
5. justifying policy decisions, and
6. providing basis for choosing policy alternatives.

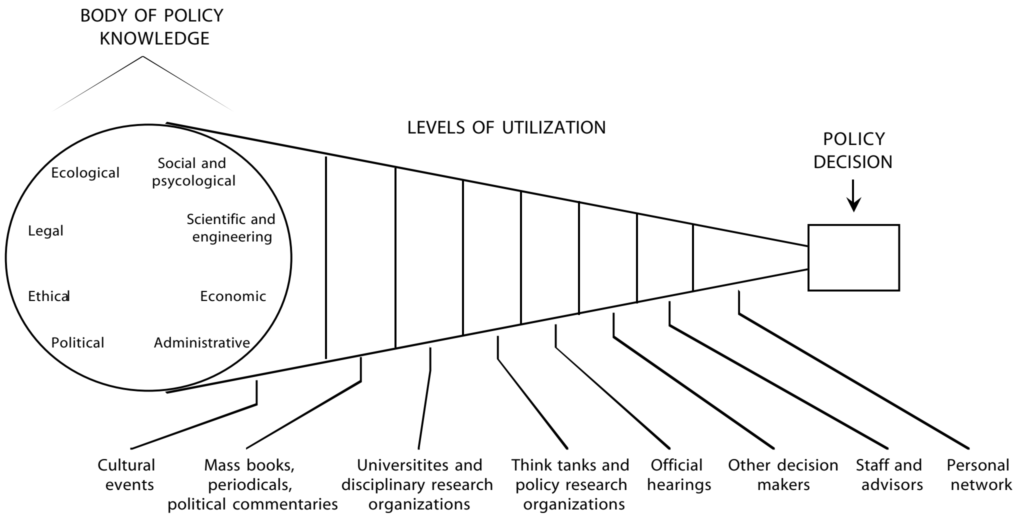
This ordering tells us that "use" of knowledge, more often addresses issues of conceptual nature than of instrumental. Even so, instrumental issues in this context do not seem to be clear cut information dependent, but rather dependent on a notion of a *policy craft*.

Turning to our second step, one may then say that the policy maker needs to operationalize two different kind of information in order to make use of, and properly "transform" scientific input. When faced with policy alternatives, (1) *Political* information will be needed to address ethical, ideological, distributive, and political issues, and (2) *policy* information that describes how policy alternatives will actually operate. Policy knowledge then becomes a very broad concept. David Webber (1991) has suggested a useful model that well depicts the range of informational input necessary to drive constitutional policy making.

Figure 3 identifies many of the influences that shape constitutive policy making, and also shows how discursively these influences narrow down as vital decisions are about to emerge from the policy system. The *body of policy knowledge* in the figure contains forms of knowledge and understanding that influence the levels of utilization to different degree. The influence of knowledge on decision in the model is understood not to be uni-directional, but rather interactive in the broadest sense of the word.

This model suggests that instead of a bipolar relation between scientific

Figure 3. Distribution model of policy knowledge. (Adapted from Webber, 1991).



knowledge and policy, different forms of knowledge are not only used by policy makers, but also narrow and expand the scope and influence of *science* to that process. They, so to speak, both come to provide the window of opportunity for a weary decision maker faced with too little information, and impose that window of opportunity on any decision part of a constitutive policy process.

Conclusion

In studying science-policy interaction, the boundaries of what constitutes policy knowledge must be broadened. This is not only because a wider flora of information than that of the scientific is available to policy makers, but also because of the mutual dependence of the two fields, where policy more often than not seems to be at the defining end. The distribution of policy analytical knowledge is a systemic process in every sense of the word (Kelly, 1986). Scholars working out of the bipolar distinction envision a communicative process where single decisions and behavior attributed to discrete policy makers are focused. The present article has suggested that this might be a too narrow view of how policy information is created, communicated and used. Often instead, dissemination and diffusion activities are entangled with creating and transforming a meaning for knowledge. As seen in our discussion of risk analysis, this meaning has epistemological, evaluative and managerial implications over which one single policy maker exerts little control. The result of this "multi-dimensional" process is that practical know-how and real political demands intertwined with moral evaluative knowledge acts back on the production of factual knowledge. This creates an "epistemological backdraft" as seen in the context of risk analysis, where science shows epistemological concern in the face of policy demand, but is eventually captured by managerial aspects of the policy process and proceeds to co-produce tools for political issue building with policy. In the case of risk analysis, this phenomenon has probably been defining for the relation of science to the regulatory machinery.

Notes

1 In this article the concept of "science" is loosely taken to refer to (1) propositional academic input into the policy process of actors working from a set of practices and styles of knowledge legitimation emanating from relevant academic fields, and (2) actors filling the function of experts in the policy process. Expert is here simply taken to be a person who possesses some scientific credentials and who is simultaneously utilized by political or capital interests by dint of these credentials to inform, create, evaluate, sustain or legitimate policy.

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