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Consensus-Based Approaches to Handling Science

Foundations for Change

Conventional methods for soliciting scientific information and resolving disputed points in public decision making often leave those who choose to become involved frustrated and confused. Interest groups struggling to have their claims heard and addressed may feel shunted aside, their concerns ignored, or may feel they are listened to, but only after expending tremendous effort and financial resources. Decision makers may do their best to hear contending arguments, but may be at a loss when it comes to distinguishing between "good" science or flawed science, or making sense out of seemingly contradictory evidence. Confusion on scientific factors may even lead to the complete exclusion of technical considerations: a decision maker unable to understand critical scientific arguments may opt instead to react to the emotionalism of arguments. Disregarding technical parameters he believes are inconclusive anyway, he may choose to make a politically expedient decision, simply one that pleases a valued constituency.

Neither letting the science decide nor leaving the science out of decisions is desirable if decision making is to be both democratic and scientifically sound. There are alternatives to conventional methods for handling scientific information in public decision making, however. These procedures, grouped together roughly for their shared reliance on a consensus-based approach to dealing with scientific information, are promising because of the opportunities they offer stakeholders for expressing their political claims as well as contributing to the understanding of critical technical and scientific factors. Importantly, consensus-based approaches are theoretically compatible not with logical positivist empiricism, but with a "new" philosophy of science that has emerged over the past two decades. This "new" philosophy of science tolerates scientific disagreement, recognizes its political roots, and invites accommodation in decision making.

An Alternative Philosophy of Science

If the ideal of science, as depicted by the logical positivist empiricists could be met, public decision making would be simplified considerably. Debate would occur over the formulation and selection of policy alternatives (because of their unique distributive effects), but, as long as scientific endeavors were accepted as yielding one truth, a singular interpretation of reality, disputes over appropriate public actions would at least have a common starting point, as defined by technical constraints and considerations.

In fact, however, this ideal is not often met. Especially in work lying at the frontiers of knowledge, scientific efforts fail to answer many questions, partly because of the inherent difficulties with what Weinberg has called, "trans-scientific issues," and also because, as recent social studies by Albury, Mulkay, and others have argued, science is not monolithic.[1] The scientific method (including data collection, experimentation, and theory building), is performed within a web of value-bound assumptions and choices. Multiple branches of inquiry develop concurrently, sometimes ultimately converging on a common construction of reality, sometimes remaining at odds for extended periods of time. Consequently, scientific inquiry guarantees neither a singular way of knowing nor a solitary and absolute image of reality.

In contrast to logical positivist empiricism, an alternative philosophy of science acknowledges the social nature of scientific efforts and far greater degrees of ambiguity in scientific work. This "new philosophy of science," began to develop during the 1960s. Writers, such as Thomas Kuhn, pointed out the importance of paradigms, disciplinary lenses, and the "problem solving" nature of science. For example, Kuhn argued that scientists are trained, in a sense, indoctrinated, to accept a set of assumptions concerning models of theory and procedure. Even the determination of what constitutes a "fit" between plotted experimental data points and the curve suggested by a theoretical model is learned (Kuhn, 1982). Scientists work within this tightly constrained framework on "problems" until a given "paradigm" reaches a point of intellectual exhaustion and no longer provides a fruitful map for resolving unsettling questions (Ben-David). At such points, a "revolution" occurs, and a new theory or set of theories replaces the former (Kuhn, 1962). Scientific "truth" has a much more tentative ring in this context, being far more contingent on the conditions of observation and the theoretical framework within which the scientist works.

Science viewed this way, confers new meanings to disagreements among scientists. Differences may represent a turning point in a single line of scientific thought, (a "revolution" in Kuhn's language), or two alternative avenues for seeking truth, (distinct "paradigms" or "lenses" for viewing), rather than simply errors or faulty logic. Disagreements among scientists may represent alternative disciplinary training rather than incompetence.

Importantly, scientific disagreement in this framework does not necessarily indicate that one analysis is "correct" and another "incorrect," but rather represent two separate, both incomplete, "slices" of reality. That is, investigators may be examining different constellations of elements of a "system" or observing the same elements from significantly varied perspectives. These divergences may be particularly salient when questions lie at the frontier of existing knowledge.

Kuhn's seminal work coincided with a shift within the social studies of science. Other examinations of the activities of contemporary scientists suggest the influence of factors external to the laboratory on methodological choices made in the course of laboratory work (Latour; Knorr-Cetina). Such "external factors" affect the selection of research topics (Hubbard; Longino) and the communication of scientific work (Brooks; Mazur). These studies suggested that factors, such as personal experiential histories, employers and funding sources, and disciplinary tradition play key roles in shaping the products of scientific research, without contradicting the canons of the scientific method.

Technical Bases of Conflicting Scientific Advice

How do the characteristics of the investigator and the objectives of an investigation shape the findings of scientific research? Over the past twenty years, historians, philosophers and other social scientists have devoted considerable attention to understanding why scientists disagree and how divergent analysis can result from two equally "scientific" courses of investigation even within a single disciplinary tradition. Close examinations of what scientists do (e.g., laboratory experiment, analysis based on statistical data, or a review of existing reports), suggest that researchers repeatedly confront decision choices that are not strictly prescribed by their disciplinary training. The choice is mostly a function of personal judgment. Different judgments made at these critical junctures can produce notably dissimilar, even contradictory, research findings. The importance of personal judgment and discretionary decisions in various kinds of policy analysis (e.g., environmental impact assessment, risk assessment, cost-benefit analysis) has been noted by a number of authors (Bacow; Susskind and Dunlap), while others have performed similar analyses of laboratory conduct (Latour; Knorr-Cetina;).

Their findings suggest five reasons why scientists often proffer very different advice.[2]

Differences in research design include such steps as the framing of hypotheses, specification of assumptions (such as time frames, geographical boundaries, and functional definitions), and data selection (National Research Council; Mazur). The framing of hypotheses varies across different disciplines depending on the primary objectives and perspectives of the field. In predictive analysis, the specification of assumptions, especially the projection of future conditions, is critical. Even in laboratory science, the recording of data is dependent on functional definitions that may vary from one experiment to another, or from one laboratory to another. For example, the detection of "change" in a subject under study is dependent on the technology available for measuring change and the conventions used to define "change." In fields of rapid innovations in technological aids, similar experiments conducted over even a relatively short time interval may yield data in forms that are not comparable.

Differences in the interpretation of data or findings can arise in cases in which scientists may agree on a given piece of evidence, but disagree about its significance. In the anti-smoking policy debate, for example, some scientists viewed statistics on the association of lung cancer and smoking habits as a strong indication of a causal relationship. Others viewed the same statistics as supporting the hypothesis that lung cancer and smoking are both indications of a third condition, which is actually the causal factor inducing both disease and smoking in individuals. Interpretative differences arise from dissimilar choices of theory, or more directly, from contrary value orientations. Individuals who hold human health as the primary objective will often have a different calculus on interpretative issues from persons relatively more concerned with the stability of productive, economic activities, for example.

Confusing communication refers to the packaging of scientific information. Scientists, or the messengers who report scientific work, often employ rhetorical devices in their attempt to persuade decision makers and potential supporters of the policy implications of their scientific studies. For example, in the anti-smoking debate, one of the favorite phrases of tobacco supporters for many years was "there is no evidence to show that smoking causes lung cancer in humans." Technically speaking, this was true, since controlled experiments on humans were difficult to carry out because of ethical and other reasons, and the experiments on laboratory animals could be faulted for failing to accurately simulate human habits and living conditions.

Other confusing communication tactics include the representation of probabilities or statistical figures in ways which most favorably dramatize the numbers. For example, in the debate over nuclear weapons testing in the Pacific in the 1960s, scientists who supported testing expressed health dangers in terms of the increased chance of cancer for an individual exposed to fallout. The increased cancer risk to an individual appeared minute. In contrast, critics of testing expressed the same estimates in terms of actual deaths that would occur worldwide over a 50-year period as a result of expected fallout. These figures appeared very high (Brooks, 1980). Thus, while it appeared as though one analysis suggested a low health risk and the other a high risk, in fact, the estimates of likely increases in the incidence of human cancers cited by the two groups and the interpretation of these figures, were identical. For dramatic purposes, however, the scientists quite intentionally chose to express the risk estimates with different reference points (i.e., the individual in one case, the population in the other). The "disagreement" was, hence, purely attributable to differences in communication tactics.

Inappropriate policy prescriptions sometimes are surreptitiously inserted during the reporting of scientific information. Although a scientist may be asked solely to report on a particular "scientific" question concerning a given policy issue, the scientist may nonetheless include statements about his "personal" opinion, as the quote in Chapter 1 illustrated. As argued in Chapter 1, this distinction between "personal opinion," or values, and "scientific advice," or fact, is somewhat illusionary. While statements about policy prescriptions, in fact, simply reflect value orientations that are inherent in the advice anyway, the explicit statement of policy preference nonetheless further exacerbates the perception of disagreement among decision makers and others listening to the conflicting scientific testimony. Despite the fact that many decision alternatives may be consistent with a given identification of scientific and technical parameters, the expert voicing his own preference directs attention and, possibly, undue certification to that position.

Error remains a factor in the presentation of conflicting scientific information in public decision making (Wessel). While no studies indicate the degree to which error accounts for debate, it is conceivable that some scientists may retract opinions after additional data that brings into questions their earlier assumptions becomes available. Finally, a closely related issue, and one that has gained notice in recent years, is the outright falsification of data and research in the scientific community. Alternative views of the scientific enterprise and the nature of scientific disagreement have profound implications for the use of science in public decision making. If conflicting scientific evidence or analyses are considered legitimate from a scientific perspective, understanding the value choices that lead one investigator to one conclusion and another to a different conclusion from a similar starting point is critical to a decision maker (and others) wishing to assess the compatibility of competing scientific arguments with her own values and policy choices. Rather than simply dismissing science as "not useful" in informing policy decisions when experts disagree, decision makers are faced with the challenge of devising decisions that address multiple estimates of reality, or of explaining why they accept one version and not others. Moreover, if scientists are not politically neutral and dispassionate, then scientific analysis and the advice of scientists can not be held up as authoritative in public decision making without obfuscating the underlying political conflicts and usurping political power.

Can public decision making procedures be adjusted to account for the biases of the scientist when scientific information, particularly contradictory analyses, is presented? If decision makers and other non-scientists could decipher why scientists submit conflicting testimony, would they be better equipped to comprehend the value orientations embodied in each analysis or report? Would a recognition of the vulnerability of scientific work to politics and values clear the path for a more straightforward discussion of the interests and values at stake in the decision? In short, can procedures be instigated to defuse the disruptive and destructive effects of scientific disagreement in public debates?

Consensus-Based Methods for Science-Intensive Public Decisions

Many writers have speculated on the theoretical compatibility of consensus-based procedures, such as negotiation and mediation, with science-intensive public disputes. They have cited such techniques as joint fact-finding or data collection, collaborative model building, and the assistance of an intervenor,[3] and the flexible structure as particular features of consensus-based methods that are likely to contribute to less adversarial uses of scientific information, greater opportunities for understanding the basis of disagreements, and higher probabilities of reaching an agreement on technical issues intimately linked to public decisions. They contend that constructing a common understanding of technical points contributes to an environment in which participants can then debate more explicitly political decisions (Bacow and Wheeler; Cormick and Knaster; Susskind and McCreary).

In fact, the actual applications of consensus-based methods in public decision making up to the present has varied considerably, complicating efforts to perform a systematic study of the impact of these methods on public decision making. Many of the 160 cases of consensus-based interventions in environmental disputes between 1973–1983 in the United States occurred outside of conventional, institutionalized proceedings. The *ad hoc* nature of negotiation and mediation efforts in public disputes, together with differences on a multitude of potentially significant dimensions such as the nature of the dispute, its intensity, the specific interests and resources of stakeholding parties, their incentives to resolve the dispute, relationships among parties, particular techniques employed, and characteristics and objectives of the intervenor further complicates an assessment of the efficacy of consensus-based approaches to resolving science-intensive public disputes.

The apparent consistency of consensus-based approaches with a view of science that tolerates uncertainty and disagreement creates a powerful urge to explore further the relationship between these methods and science-intensive public disputes, however. The overriding question is, do consensus-based procedures affect the role of science in public disputes in any consistent or predictable pattern or direction? One step toward answering this question is a close examination of how consensus-based procedures affected the role of science in actual cases.

Three Procedures for Science-Intensive Decision Making

Referring again to the the three cases used hypothetically in the previous chapter on conventional decision making, we turn now to look at how consensus-based procedures were actually applied in each of these decision making scenarios. These three examples illustrate how consensus-based methods offer alternative ways of handling scientific and technical information and disputes. The cases suggest three distinct procedures: One case shows how understanding the causes of scientific disagreement can move decision making forward; another shows how building a consensus on technical aspects of a decision can lead to agreement on policy; the third case portrays a procedure for reaching a policy agreement in the presence of substantial technical uncertainty. Importantly, in all three cases, the procedures permitted a far greater degree of flexibility in dealing with technical and political uncertainty, as compared to the conventional "decide-announce-defend" approach.

Procedure 1: Understanding the Basis of Scientific Disagreement

The New York Academy of Sciences facilitated policy dialogue represents a rather narrow form of intervention. The objective, as described by Don Straus, chair of the Science and Society Committee of the Academy, was not "to solve or even suggest solutions to how to solve waste disposal" but to "help representatives of the BOE to walk through scientific issues concerned with how to solve solid waste disposal" (New York Academy of Sciences, 1984b). In fact, even this seemingly limited objective overstates the actual accomplishment of the 8-hour, one-day session. The achievement was modest: simply to trace the basis for the discrepant risk assessments performed in respect to one solid waste management option, namely the proposed Brooklyn Navy Yard facility. Nonetheless, even this relatively minor accomplishment might not have occurred without a consensus-based procedure.

A Close Look at the Scientific Disagreement. The New York Academy of Sciences policy dialogue was undertaken in response to an urgent plea for assistance from the New York City Department of Sanitation (Konkel). One month after the Department of Sanitation (DOS) announced its plan to construct a resource recovery facility at the Brooklyn Navy Yard site, the Center for the Biology of Natural Systems (CBNS) issued the first of four reports condemning the project for exposing the City's residents to an increased risk of developing cancer. The city's governing Board of Estimate (BOE) instructed the DOS to conduct further study, which was embodied in a report by Fred C. Hart and Associates, Inc. Under what each called a "worst-case" scenario, the Hart report estimated an increase of 5.9 cancer cases per 1 million population exposed over a 70-year lifetime while the CBNS report predicted a range of 29 to 1,430 additional cases of cancer per million population (Commoner, 1984).

What accounted for this startling, 240-fold discrepancy? Briefly, the different figures can be traced to differing opinions on two main factors: (1) predicted dioxin emission levels, and (2) the effectiveness of proposed pollution control technologies. These two factors, in turn, are inextricably bound to a theory of the mechanisms of dioxin formation in municipal solid waste (MSW) incinerators.

Differing assumptions about the level of dioxin emissions is the singular risk assessment variable that goes the furthest in explaining why the two cancer risk assessments differed by more than a factor of 240. If the same expected emission level is factored into each analysis, the Hart and CBNS risk analyses respectively yield values of 5.9 and 29 additional cancer cases per 1 million population exposed over a 70-year lifetime. Given the high level of uncertainty in this type of risk assessment, a less than 5-fold difference between projections is not a significant variation (Commoner, 1984: IV-18).

Estimating expected levels of dioxin emissions is an imprecise task. Although reports on dioxin emissions from municipal incinerators appeared in the mid-1970s from the works of European researchers, existing data in 1984 was still spotty, idiosyncratic, and, as a result, inconclusive. The Hart report identified data on dioxin emissions from 19 incinerators located around the world. However, monitoring protocols, the specific identity of the dioxin isomers tested, the physical state of the dioxin compounds tested, and numerous other methodological details for each of these tests varied, making the comparability of performances among these existing facilities difficult to judge. Moreover, separating valid from invalid testing results was impossible.

The different research groups took different approaches to selecting an appropriate emission estimate for their risk analyses. From those 19 sets of emissions test data listed in the appendices of the Hart report, authors of the Hart report combined two sets of testing data, from the Chicago Northwest and the Zurich-Josefstrasse facilities for their risk assessment. They justified their selective use of data on the basis of similarities between the proposed BNY facility and these two facilities with respect to furnace design, location (ie., in a large U.S. metropolitan area), and waste composition and on the basis of sampling methodology (Hart: 3-19, 3-20). Interestingly, as the authors themselves note, emissions data from these two facilities were also among the lowest reported (Hart: 3-15).

The key words in the Hart report are "data selected as the most representative." In contrast, CBNS researchers looked at the available data comprehensively, rather than exclusively. They interpreted the wide range of test results as indicating the high variability and unpredictable nature of dioxin emissions, rather than as resulting from the varying reliability of measurement techniques in different cases. They asserted that too little is understood about the dynamics of dioxin emissions to confidently judge representativeness and comparability between plants. To safeguard against such gaps in knowledge, the CBNS analysts utilized both the lowest and the highest tested emission levels in their risk assessment, (thereby yielding a range of expected increases in cancer rates, from 29 to 1,430 additional cases), without attempting to judge their relative validity (Commoner, 1984: I-9, 10).

In subsequent reports, the CBNS research team continued to refute reasons offered by the Hart group for justifying their more narrow data selection. In particular, the CBNS analysts contested the relevance of design similarities cited by the Hart group as justification for their data selection. They argued that the Chicago, Northwest and Zurich-Josefstrasse facilities are more similar to the proposed BNY plant than the other facilities for which testing data were relatively complete, only in that they utilize a Martin grate (part of the furnace system). Other potentially important features such as the size of the facility were not similar. Moreover, they contested the role of the Martin grate and furnace operating conditions in affecting dioxin levels. They cited recent testing data from a Tsushima, Japan incinerator equipped with a Martin grate, which showed emission levels ten times higher than the Chicago, Northwest test data despite furnace temperatures of 800 degrees centigrade (Commoner, 1984) and a Canadian study which indicated that emission rates were not significantly affected by temperature or other combustion factors (Commoner, 1984: IV-10).

At the core of the disagreement over the appropriate data set and the significance of the furnace system were assumptions about the formation and destruction of dioxin in MSW incinerators. There was no challenge to the proposition that dioxins are destroyed at very high temperatures (800 degrees centigrade or higher). It was uncontested that under optimum conditions of air turbulence, oxygen concentration, residence time, and high temperatures, laboratory experiments have shown about 99 percent of dioxins present are destroyed. It was also more or less undisputed that the furnace design proposed for the BNY plant would be capable of destroying a significant proportion of the dioxin in the combustion chamber, although there certainly was room for disagreement on this issue.

What was contested was whether dioxin is actually present in the combustion chamber at all. The formation of dioxin in incinerators is not well understood. In their effort to knit together the pieces of information obtained through past studies, researchers developed three alternative hypotheses to explain dioxin formation. The first is that dioxin compounds are present in the raw refuse and are volatized during incineration. Since PCDDs and PCDFs are known to have formed as byproducts and contaminants of commercial chemical goods commonly found in municipal refuse (such as polychlorinated biphenyls [PCBs]), it is reasonable to assume that municipal wastes may contain traces of dioxin. In fact, one study did detect PCDFs and PCDDs in raw wastes, although not in sufficient quantities to explain tested dioxin emission levels (given the generally accepted fact that laboratory experiments have demonstrated that about 99% of the dioxins present are destroyed at high temperatures.)

The second hypothesis, one regarded as the conventional theory, posits that PCDDs and PCDFs are formed from precursors present in the waste stream. Precursors are products (such as PCBs and chlorophenols) that contain PCDF and PCDD materials as contaminants. It is hypothesized that PCDFs and PCDDs form at temperatures sufficient to decompose precursors but too low to destroy dioxin. PCDFs and PCDDs can also volatize directly from precursor materials. Laboratory experiments have provided data consistent with this theory, although no studies have yielded conclusive data (Hart: 3-4). In fact, one experiment indicated that adding precursor materials to the waste stream did not significantly increase the PCDF and PCDD concentrations found adsorbed onto fly ash (Hart: 3-5).

The third theory proposes that PCDDs and PCDFs are synthesized *de novo* from constituents of materials commonly present in the waste stream, such as wood products and plastics. The "*de novo* synthesis" theory of the formation of dioxin has been deduced from laboratory experiments that have shown that no dioxin is emitted when certain materials are burned separately but is detected when these and related products are incinerated together. According to this theory, PCDDs and PCDFs are formed in municipal solid waste (MSW) incinerator systems by chemical reactions between carbon-ring compounds produced by the incomplete combustion of lignin (a constituent of wood and paper), and chlorine in the form of hydrochloric acid. Hydrochloric acid is produced in incinerators by the combustion of chlorine-containing plastics (such as polyvinyl chloride, or "vinyl") that are present in MSW and ordinary table salt, which provides an unknown, but apparently minor, contribution. Paper is the major source of lignin in MSW (Center for the Biology of Natural Systems: IV-8).

Distinct from the preceding two theories, the *de novo* synthesis theory further posits that syntheses of PCDDs and PCDFs do not occur in the incinerator, but at later points in the waste gas stream. The carbon-ring compounds and chlorine compounds are freed from their original state during the combustion process and adsorb onto particles of fly ash. These constituents of PCDDs and PCDFs then react under lower temperatures (in the smokestack or other points beyond the combustion chambers) to form PCDDs and PCDFs. As in the case of the conventional theory, the results of at least one experiment appear to contradict this theory (Hart: 3-5).

The authors of the Hart report acknowledged the ambiguity of existing empirical evidence by recognizing that dioxin formation may occur by more than one mechanism. They argued, however, that the 10-degree design temperature difference between the pollution control device and the stack of the proposed facility made condensation in the stack unlikely and that PCDF and PCDD materials adsorbed before entering the pollution control device would be trapped in the fabric filter (Hart: 3-23). Implicitly, they justified a narrow data set on the assumption that dioxin is present in the raw waste or is formed from precursors during the combustion step and can be destroyed under optimum heat conditions. They further assumed that any dioxin formed subsequent to the high temperature chambers is likely to form before, not in, the stack, and will therefore be contained by the fabric filter control system.

The CBNS team more adamantly subscribed to only one theory, the *de novo* synthesis theory. In their report they described tests from the Tsushima, Japan incinerator, which is similar to the proposed plant in furnace design and equipped with the same pollution control system. They claimed that tests showed that this system failed to control PCDD/PCDF emissions and that PCDDs and PCDFs were, in fact, synthesized in the control system with seven times as much PCDD/PCDF leaving the control system (emitted through the stack) as entered it (CBNS: IV-11).

The relevance of the process of dioxin formation to the BNY proposal is two-fold. First, if PCDDs and PCDFs are indeed formed by precursors in the waste stream as the conventional theory holds, then they ought to be destroyed if appropriate incineration conditions are maintained. On the other hand, if the *de novo* synthesis theory is true, and synthesis occurs only after temperatures in the waste gas stream are sufficiently cooled, then PCDDs and PCDFs would not be present in the combustion process at all and high incineration temperatures and other combustion factors such as air turbulence and oxygen balance could be expected to have no effect on emission levels. Thus, if the conventional theory is true, the importance of the Martin grate in selecting emission level data is substantiated and a lower risk estimate may be more accurate. Conversely, if the *de novo* synthesis theory is correct, this design feature would be arguably less significant in relation to data selection, and a higher risk estimate is warranted.

Theories of dioxin formation also have implications for evaluating the effectiveness of pollution control technologies. If PCDD and PCDF precursors are present as contaminants in single products, waste separation before incineration would have no effect on dioxin emission levels. If formation occurs during combustion, then increasing the effectiveness of particulate emission control systems should reduce dioxin emissions. If dioxin is formed in accordance with the *de novo* synthesis theory, "add-on" air pollution control technologies would be useless unless a significant proportion of the dioxin formation occurred before or in the control technology system. In that case, waste separation prior to incineration would appear much more promising to reduce emissions.

This account of the New York City dispute shows that discrepancies in the work of reputable scientists can occur when the scientists hold differing opinions about factors that cannot be ascertained given the present state of knowledge. These two groups of researchers reached different determinations on the appropriate data set largely because of the lack of conclusive information on the mechanism of dioxin formation in MSW incinerators. The absence of definitive theory, together with contradictory test results from dissimilar facilities obtained under unquantifiably varying conditions and findings from laboratory studies whose extrapolation to real world experiences is questionable, enabled each to construct equally plausible and persuasive scientific rationales for critically different data selections.

Without knowing the relative impact of various factors on dioxin emissions, some scientists are willing to make assumptions where others are not. Without conclusive evidence to support one theory of dioxin formation over another, scientists may intuitively find one argument more compelling than another. The willingness to make assumptions, the "intuition" that attracts an individual to one theory over others, like personal "risk aversity" levels, are intermediate manifestations of the individual's unique set of values, experiential history, and position within the current debate. When disagreement surfaces, the controversy may heat up to the point that groups intentionally (or not) engage in communicative manipulations, such as using single terms like "worst case" to convey different meanings.

These kinds of disagreements arise again and again in science-intensive public disputes, in varied renditions, as the latter two cases illustrate. In what ways did the facilitated policy dialogue function to enlighten the decision makers (in this case, their staff advisors) on the issue of dioxin emissions and solid waste incinerators?

Decoding Scientific Disagreements. The "by-invitation-only" facilitated policy dialogue was set up as a one-day session to address three specific issues concerning the proposed mass-burn incinerator (New York Academy of Sciences, 1984a). The three issues, which were selected by staff from the New York Academy of Sciences after consultation with members of the BOE, included: (1) the types of emissions and their health effects, (2) the sources of emissions in resource recovery plants, and (3) the control of emissions. After 30-minute presentations on each topic from expert panelists, who were also selected by the Academy staff in consultation with BOE staff and representatives of major environmental groups, the floor was opened to questions from other panelists, BOE staff, and the general audience.

The isolated opportunity that the one-day session offered scientists to present their views made the occasion vulnerable to attempts at "grandstanding." In some cases, presenters used the forum to defend their opinions and interpretations of study results. For example, one panelist, a chemist whose work had been cited in CBNS reports, was asked to address the issue of the sources of PCDD and PCDF emissions from mass-burn plants. He spent much of his 15 minutes rendering a carefully prepared statement condemning the "CBNS theory" (the *de novo* theory) and clarifying what he believed was the proper interpretation of the results of his research. His reinterpretation of historical data cited by CBNS provoked a strong rebuttal by Commoner during the following question and answer period. This interval of the policy dialogue, which can be characterized as highly antagonistic, seemed to constitute little more than an opportunity for the speakers to present orally their own "adventures in applied probability."[4]

In contrast, the question and answer period in other instances helped to clarify exactly what the experts, in their cautious, scientific language rich with disclaimers, were actually saying. The dialogue allowed the audience an opportunity to gain an appreciation of the contingent nature of what could be viewed as prescriptive advice (such as an estimate of the effectiveness of emission reduction technologies) and descriptive scientific theory (such as assumptions about the formation and destruction of dioxins). The mediator assisted in these interactions between decision maker representatives and experts by rephrasing questions and responses, and by reminding speakers of the focus of the discussion. In some cases, the mediator's attempt to rephrase a question helped the asker to express it more clearly himself. In other cases, the mediator's attempt to repeat a response was corrected by the respondent. All of these efforts helped to clarify the scientist's view for the non-scientist listener.

Perhaps most importantly, what was achieved was not only merely the disclosure of the technical basis for differing risk assessments, but the disengagement of two polar opposite policy positions—to build and not to build the plant—from the scientific issues. The lay-out of the issues, the question-and-answer format, and the mediator's vigilance helped to keep clear the distinction between what is known about dioxin formation and destruction and pollution control technologies, and the desirability of different technologies. Rather than a "black and white" choice between a plant with high emissions and no plant and no emissions, a richer landscape of alternatives was drawn as individuals became inspired to suggest novel ways of dealing with uncertainty. One suggestion heard was to require the builders of the mass-burn incinerator to bear the costs incurred if a plant is shut down for failure to attain agreed on emission levels, for example. This is an intriguing way to force those most confident of their assertions to gamble the hardest.

It is also significant that Dr. Barry Commoner, the leading scientist-spokesperson opposing the proposed Brooklyn Navy Yard facility and a participant at the facilitated policy dialogue, sent a letter that was published in the *New York Times* three days after the meeting. Reasserting his belief that DOS estimates of dioxin emissions were inaccurately understated, he proposed that a "good way to cut through the controversy, which was suggested by a recent action by California in response to an incinerator issue" (and a suggestion that was raised at the NYAS policy dialogue) was to require "the builder to show, by tests on the completed incinerator, that it does, in fact, emit dioxin at the low rate that the builder predicts" (*New York Times*, January 5, 1985). This statement by Commoner seems to indicate that the two issues—the question of the cancer risk posed by dioxin emissions from the proposed facility and the question of whether to build the facility or not—were successfully severed by the discussions at the policy dialogue.

One Step Toward Decision. The New York City Board of Estimate voted to approve the comprehensive waste management plan on December 20 1984, only two days after the policy dialogue (*New York Times*, December 21, 1984). Public opposition to the high-tech waste management plan persisted, but in August 1985, the BOE approved the Brooklyn Navy Yard proposal as well (*New York Times*, August 16, 1985). In response, members of the Williamsburg community organized a mass protest march to City Hall and a spokesperson was quoted as saying, "We will be at the site every single day, a single bulldozer will not enter that site" (*New York Times*, September 6, 1985). Residents subsequently filed legal suit and construction of the plant has been delayed indefinitely.

Disengaging decision alternatives from disputes over scientific or technical issues is only the first step in developing a politically acceptable decision. The facilitated policy dialogue was not designed to take the discussion beyond the point of clarifying disagreements between experts. Consequently, although potentially it reopened the discussion to new alternatives and the expression of political interests, the policy dialogue was not directed toward facilitating either process. It presented opportunities, but without strong inducements for action.

The decision alternatives that were added to the discussion partly as a result of the information that surfaced at the policy dialogue consisted of add-on air pollution control technologies and more stringent monitoring provisions to ensure expected operating conditions are not violated. One might argue also that advocates of alternatives to mass-burn incineration gained political ground since legitimization of the higher risk assessment and the *de novo* theory of dioxin formation would have made recycling and other approaches appear more appealing to decision makers hoping to allay public fears. While the actual benefits to advocates of alternative waste processing methods afforded by the policy dialogue are difficult to identify absolutely, it certainly is plausible that the policy dialogue broadened the consideration of decision alternatives to include ones more compatible with the political objectives of these groups.

On the other hand, the relatively limited scope of the policy dialogue, in terms of objectives and scheduling, did not allow for more revealing discussions of political interests. The meeting was focussed entirely on scientific issues. Although this was useful in disclosing some value choices behind divergent technical analyses, (e.g., how conservative a stance to assume in estimating variables), it did not flush out statements about the motivations of various groups involved.

For some groups, the political interests that spurred action were less clear than those of others. Although the CBNS researchers and the Williamsburg community sat on the same side in the scientific dispute, the political interests behind their involvement were probably quite distinct. The Williamsburg residents opposed the Brooklyn Navy Yard plant because it was slotted for a site adjacent to their neighborhood. Although general public health risks were certainly of concern to them, it is not clear that they would have spoken out against a similar plant had it been proposed for a location elsewhere in Brooklyn, in the Bronx, or in another state.

The motivation of the CBNS researchers can be surmised quite differently. Dr. Commoner has been part of public opposition to mass-burn incinerators in several communities outside his own. From his extensive writings, his involvement and those of his colleagues at CBNS might be understood as a manifestation of a commitment toward restructuring a "wasteful," environmentally assaultive society into a more ecologically balanced one.[5] The fact that Dr. Commoner advocated recycling, waste sorting, and source reduction in lieu of mass-burn incineration reinforces this interpretation of Dr. Commoner's motives. On the other hand, his involvement may simply be motivated by a belief in self-determination and a response to a request for assistance from a community struggling to gain control of its future.[6] In any case, his political motives were probably broader and more ideologically oriented than those of the Williamsburg community.

The interests of other groups present at the policy dialogue can be expected to differ again from these two groups. However, the policy dialogue did not encourage a discussion of the concerns and interests behind the involvement of various groups. Consequently, it provided little additional enlightenment to decision makers aiming to make a politically acceptable and technically reasonable decision.

Procedure 2: Building a Technical Consensus[7]

The woodburning stoves regulatory negotiation was a fairly comprehensive attempt to weave technical and scientific knowledge into the policy making trade-offs necessary in developing implementable technology-based pollution control regulations. In contrast to the facilitated policy dialogue and the Michigan fishing case, the regulatory negotiation preceded any well-publicized debate over the issue under consideration, emission standards for new residential wood combustion units. The participants held a wide range of concerns and were variously equipped to deal with scientific, legal, and regulatory aspects of rulemaking.

Putting the Process in Motion. The Environmental Protection Agency's objective was to develop rules that were politically palatable, enforceable, and technically feasible. EPA had previous experience with the use of negotiation in rulemaking. The agency, through its Office of Program Planning and Evaluation, had undertaken a pilot project in regulatory negotiation beginning in 1983 and by mid-1985, three of the six demonstration "reg negs" were completed or underway (Harter, 1986). The EPA process designers, through consultation with the Standards Development Branch, were also familiar with the nature and type of issues that would require consideration in the wood stoves case, and the critical limitations of the technical and scientific knowledge needed to back up decisions. Although regulations agreed upon by the negotiating parties were preferable, even without signatures, EPA would be closer to promulgating appropriate rules at the end of the negotiation effort since much of the technical and policy issues would be clarified through the discussions.

By early 1986, the Standards and Development Branch of EPA sent letters to about 20 prospective participants announcing the Agency's intention to undertake a regulatory negotiation process and inviting recipients to attend the first organizational meeting. The agency also issued a notice in the February 7, 1986 *Federal Register* informing and inviting the wider public to indicate their interest by attending the first meeting. Included in this public notice was a description of the procedure for identifying participants in the regulatory negotiation. EPA states,

> We do not believe that each potentially affected organizational [sic] or individual must necessarily have its own representative. However, we firmly believe that

> each interest must be adequately represented. Moreover, we must be satisfied that the group as a whole reflects a proper balance and mix of interests (*Federal Register*, 1986).

Fifteen individuals representing an array of interests,[8] plus the EPA negotiator were officially designated the negotiating group. Most of the members were among the original list of EPA invitees; two additional members successfully argued for a seat at the negotiating table at the first meeting and were accepted by consensus of the group overall.

An EPA staff person initially intended to serve as mediator, but the EPA-hired consultant, originally acting as "convener" quickly took over all facilitating as well as convening responsibilities in response to requests by the participants. The negotiations were structured around six two- and three-day meetings that took place at regular intervals over a period of six months. Meetings were announced in the *Federal Register* and open to the public. Observers were were encouraged to ask questions and submit additional information and comments to the negotiating group upon procedural recognition from the facilitator. Discussions were often lengthy, but rich with the engineering, legal, and regulatory knowledge of many individuals as well as intense debate between parties with competing interests.

As in the New York City waste processing plant dispute, the science and technology of wood stoves is not well understood. Emissions vary in accordance with a number of difficult-to-control factors, including user habits, such as the way one stacks wood, wood type and age, burn rates, and other such variables, as well as differences in stove design. Although stoves equipped with catalysts are widely believed to burn more "cleanly," a lack of long-term data arouses doubt about the overall performance of catalysts in reducing emission levels. Moreover, it is suspected that catalysts degrade through use, but how quickly degradation occurs and the effect of alternative catalyst materials and stove designs on degradation rates is not known. Finally, the difference between emission levels occurring during laboratory testing and actual home-use is also highly speculative.

The level of technical ambiguity surrounding wood stove emissions opened the door for analytical acrobatics and political posturing by the stakeholding parties. Instead, through the negotiations the parties apparently recognized the uncertain nature of the calculations over which they labored. Sometimes, through an iterative process, agreement would be reached on one number or one method of measurement or analysis. More often, a sort of "bounded" ambiguity prevailed. In these cases, the negotiations over "hard numbers," such as permissible emission levels, compliance dates, and so on, transpired in a climate in which negotiators had a common acceptance of the range of scientifically acceptable estimations. In determining the package of provisions that comprised the ultimate regulations, negotiators traded across issues (sometimes called "logrolling" in the negotiation literature),[9] accepting higher estimates on one variable that justified one party's preferred policy choice, in exchange for lower estimates on another variable which supported another party's preference on a different provision. What resulted was a mosaic of rules and regulations which has not been seriously criticized after publication and which most parties believe are as scientifically and technically sound as possible to develop under the prevailing time constraints.

Reaching a Technical Consensus. This scientific and technical consensus was accomplished in a number of different ways. Although EPA staff persons had appropriate technical training which was supplemented by hired consultants, the accelerated rulemaking schedule resulting from the NRDC lawsuit settlement meant that the agency would be hard pressed to generate independent scientific and technical data. By involving many parties in the rulemaking process, some of the effort and cost of gathering data were, in effect, "externalized." The negotiating parties, notably the WHA, the independent testing laboratories, and the states of Oregon and Colorado, which have operational regulatory programs, volunteered data and technical analysis on issues of their particular concern as well as in response to requests by others during meetings. As a result, the regulatory negotiation format allowed the group to assemble a massive amount of existing information rapidly in usable form.

Data and analysis presented by parties with a strong interest in a particular decision are often looked upon skeptically by the receivers—be they decision makers, other interested parties, or observers—and with good reason. While industry is commonly accused of underestimating health risks associated with the use of chemical substances in the workplace (Marcus) and overestimating the costs of proposed regulations, government has been found to exaggerate the benefits of proposed regulations (Brownstein). Hence, information contributed by parties with a strong stake in a decision is often viewed as biased, incomplete, or even inaccurate. In the regulatory negotiation setting, negotiators, their expert advisors, and observers were able to freely question the party presenting the information about data sources, assumptions of the methodology, and others details of research design. When EPA presented an econometric model to predict the impact of exempting small manufacturers from the regulations, for example, skeptical negotiators were invited to submit alternative input values, or assumptions, to yield predictions under varying conditions.

Error in data or analysis could be detected as group members carefully scrutinized each item submitted. Even if no flaws or inconsistencies were uncovered in the cross-examination, the listeners, experts and non-experts alike, gained a sense of the data's validity, an understanding of the underlying assumptions of the analysis, and general significance of the information simply by the tenor of the discussion.[10] With the stakeholders physically together, technical arguments were "on trial" to be judged by the group as a whole, not only by EPA. Overall, the credibility of data and analyses subjected to careful, open, and interactive viewing in this way was increased significantly.[11]

The structure of the negotiation sessions also allowed for the presentation of contradictory, inconsistent, and complementary scientific and technical evidence and arguments in a way that maximized the opportunity for understanding how and why they differ. When technical disagreements and uncertainties seemed too unwieldy for the mixed group to handle, subcommittees formed (comprising representatives from each major coalition) to examine the issue more closely and come back to the larger group with some kind of clarification, if not a consensus. Because the negotiations were structured so that all issues were introduced in the earlier sessions and then "revisited" during the later meetings of final deliberations and bargaining, participants also had an opportunity to seek independent reviews and consultations (US EPA, 1984) and to submit additional materials for consideration by the group through mailings and over the wires. Thus, a considerable amount of debate over the technical aspects occurred, allowing a full airing of multiple sides of the issues (alternative interpretation, inconsistent data, competing theories, etc.).

Also, as in the NYAS policy dialogue, the participation of both "expert" and "non-experts" in a variety of specialized fields forced individuals to maintain a language that was relatively clean of jargon, rhetoric, and deceptive manipulations. In addition to the fact that many participants indicated that they were not shy about demanding clarification on points even at the risk of revealing their ignorance, the facilitator also made deliberate efforts to pull in the reins on any speaker who rambled on in technical jargon or without clear explanations. It is interesting to note that despite conspicuous efforts to keep the discussions comprehensible to all participants, inevitably certain topics were overly complex for everyone to follow. Surprisingly, however, individuals who later admitted the discussions sometimes went over their heads, claimed that they did not feel they had been "snowed." Their confidence in the strength of the bonding among members of their coalition apparently provided sufficient reassurance that if their interests were threatened by any of the discussions, coalition members more competent on the technical aspects of the case would alert them accordingly.[12]

It seemed that the participants were satisfied at the end of the negotiations with the scientific validity and technical feasibility of the rules they collaborated in writing. Participants commented that political positions (policy options) were always grounded in what was technically possible. The inclusion of technically expert persons in each major coalition meant that individuals with a particular concern could thrash it out during a caucus and the coalition members together could develop a technically-sound proposal to suggest to the larger negotiating group.

Although there seems to have been a considerable amount of give-and-take during this regulatory negotiation including a substantial amount of information sharing and debate over methodological assumptions and technical ambiguities, many participants also noted that they did not believe that EPA had relinquished any real control over the rulemaking procedure. A number of participants commented that, throughout the negotiations, EPA seemed to draw certain lines over which they would not cross, regardless of the technical or political arguments proffered.[13] One person interviewed described the lead EPA negotiator's attitude on particular issues as being one of "Don't confuse me with the facts." In other words, the respondents indicated a certain close-mindedness on the part of the EPA in regard to hearing scientific or technical arguments in support of positions the agency (apparently for political reasons) was not prepared to back. Negotiators seemed not seriously discouraged by EPA's behavior, however, and instead showed a sort of appreciation of the agency's own bureaucratic and political tightrope (constraints resulting from provisions such as the Office of Management and Budget's oversight role in rulemaking, which is to assess the economic impact of proposed rules as required under Executive Order 12991).

Managing Science to Forge a Political Consensus. The consensus-based procedure employed in the EPA rulemaking negotiation was a comprehensive and deliberate attempt by EPA to orchestrate the submission of technical information and the expression of political interests. While the agency retained a considerable degree of control over the process through its success at unilaterally invoking limits to discussions and, at times, refusing to entertain further technical arguments, negotiators nonetheless expressed a sense of participation in decision making unusual under conventional proceedings. Discussions on relevant scientific and technical points were adversarial and competitive, but not destructive or unproductive.

Three factors contributed to this treatment of scientific and technical components of the rule's development. First, although negotiators freely submitted technical information and analysis in a way that might have explicitly supported or challenged certain policy alternatives, the discussion format of the negotiations provided opportunities for ample questioning and clarification. As in the NYAS policy dialogue, participants developed a more thorough understanding of the basis for differences in data and analyses and a mutual appreciation of the uncertain nature of both the scientific and technical premises and the actual effects of various regulatory actions. Importantly, both scientific and regulatory "uncertainty" were accepted as facts of life given the current state of knowledge and as the necessary basis for policy rather than as an opportunity for casting doubt on the desirability and suitability of a proposed action.

Second, the timing of the consensus-based intervention was significant. Since the negotiations occurred prior to a complete formulation of the rule by any party, participants did not begin the procedure reacting against certain options. That is, because more or less the entire rule was yet to be developed, participants recognized the contingent nature of their initial positions on various provisions of the rule and refrained from explicitly ranking policy options, rather viewing the issues as a package. A stricter emission standard would be more reasonable from the manufacturers' perspective if the compliance date was coordinated to coincide with production cycles so that design changes could be made without interruption in production, for example. In contrast, if manufacturers instead had been presented with a fully formulated rule proposed by EPA, they would have likely launched an attack on the scientific merits or technical feasibility of the numerical standards rather than suggest adjustments to other portions of the rule. The positions of both EPA and the manufacturers would have hardened around specific emission level figures and a full-blown technical dispute likely would have erupted.

Finally, the negotiators in the wood stove regulatory negotiation shared a common desire to generate rules. Each party had their own incentives to promulgate federal rules, and each negotiator, other than those from EPA, had a strong interest in the group developing the rules rather than the agency alone. This shared goal provided the focus and impetus necessary to move the group along and away from protracted, contentious uses of technical argumentation.

Because other negotiators apparently deferred to EPA negotiators in the proceedings, an alternative interpretation of the rulemaking effort might contend that the agency was, in fact, imposing its view of scientific and technical parameters on the other participants and using this dominance to guide the development of the rules along a relatively narrow course. After all, EPA led off discussions with technical reports written by their consultants according to EPA specifications and circulated written summaries of the meetings, in effect, etching their version of discussions into the group's collective memory. More alarmingly, participants commented on EPA's refusal to consider additional evidence and arguments on certain issues. There are features of the procedure that suggest that this interpretation is not likely to be true, however.

First, while EPA may have held an advantage in regard to the initial presentation of technical information, other participants (and observers) were encouraged to present additional information or analysis. Participants, especially negotiators representing the manufacturers, the independent testing laboratories, and Oregon state, frequently did submit supplementary data and analysis on points relevant to their areas of experience and expertise and such submissions were appropriately weighed and integrated into the rulemaking.

Second, it might also be argued that the degree to which the agency tended to reject evidence counter to its own in the negotiation was no greater than its exercise of discretion in normal rulemaking. In fact, in the negotiation setting, failure by EPA to consider evidence was openly visible to participants and was thus potentially more politically costly. Participants could rebel *en masse*, if necessary, by withdrawing from the negotiation altogether. Since the parties soon organized themselves into coalitions, the displeasure of one party could result in many parties registering complaint by walking out. Thus, the damage EPA would incur by openly refusing to consider scientific evidence that contradicted their own would be substantially greater in a consensual procedure than under conventional rulemaking procedures, and the agency would be less likely to blithely overrule or neglect contrary arguments.

In any case, participants believed that their interests were better expressed and met through the negotiated rulemaking procedure in comparison to conventional proceedings. As two persons described it, "Each group got something" and "No one gave away something they really wanted."[14] Similarly, no negotiator interviewed criticized the scientific or technical soundness of the rule, although many noted gaps in information they believe might have helped to refine the rule. In fact, several participants described the resulting rules as highly creative and wise in ways that EPA would have been unable to duplicate on its own.[15]

Procedure 3: Proceeding Despite Uncertainty[16]

The mediation effort in the Michigan fishing dispute occurred as a result of a court order and came at a relatively late stage in the evolution of the dispute. Unlike the New York City case, it was unclear how prominent technical issues would become in the negotiations. Like the wood stoves case, however, key contenders in the legal battle had access to a sizable scientific and technical arsenal. Any settlement was likely to hinge critically on the perceptions of various parties with respect to major scientific assumptions.

Building Communication Linkages. The principal parties, the Michigan Department of Natural Resources (DNR), representing commercial and sports fishers, the three tribes, and the federal Department of the Interior had been engaged in legal battle for more than a decade. Relations among the parties were strained. The tribes felt the DNR only dealt with them grudgingly, treating them with increasing respect only as a result of their victories in the courts (Doherty). A series of attempts had been made over the years to negotiate a settlement to the conflict over the Great Lakes fishery, including an effort in 1982 that produced an "agreement in principle" among the key parties. But, the agreement had fallen apart when attorneys began drafting and the parties began reviewing the document (*Legal Times*).

When the special master arrived on the scene, he was greeted with a number of parties with a long history of distrust and difficult relations. He was given instructions from the court to assist the parties to reach a negotiated settlement and to manage the discovery process leading to a court trial, which was set for April 22, 1985, in the event that negotiations failed.

Special Master McGovern's strategy was built upon three elements: (1) fostering a sense of urgency to settle the dispute, (2) cultivating among the litigants a desire to have a direct hand in shaping the settlement and, (3) de-escalating the hostile use of scientific arguments. Between the months of January to March 1985, the special master met with the attorneys representing the parties on an accelerated discovery schedule. At least one attorney recalled billing his client conservatively for 250 hours per month during that period, and spending three out of four weeks obtaining depositions from witnesses for the case.

During this interval, McGovern also called a meeting inviting all interested parties, the biologists, and the attorneys, to hear remarks by participants in a similar case of litigation concerning a state-tribal fishery dispute in Washington state. The primary message at this gathering was not subtle. Many of the listeners reported that the intent was to drive home to the disputants the idea that litigation was a horrendous affair to be avoided at all costs. By convening this meeting, McGovern was apparently attempting to increase the parties' perceptions of the attractiveness of their alternative to litigation, a negotiated settlement.

Finally, McGovern brought together for several meetings biologists from the key parties (replicating almost to a person the TTWG), a nonpartisan convener, and a fish biologist with modelling expertise from the state-funded University of Michigan Institute for Fisheries Research. The stated purpose of convening the biologists was to develop a common model for predicting the impact on the fishery of varied allocation proposals.

The mediation effort culminated in an intense, three-and-a-half day negotiation set at a college in Sault St. Marie in late March 1985. More than 50 persons representing the litigants as well as interested individuals representing only themselves attended the negotiations. This sizable group was divided into two, and the smaller core, comprising representatives of the litigants, hammered out an agreement that eventually became an order of the court. At the end of a round-the-clock session that extended some 36-hours, this core group of negotiators posed for the press cameras standing behind the settlement draft that bore their signatures.

The impact of the meetings of the biologists, the special master's focussed attempt to resolve important technical issues, cannot be appraised in isolation from the other activities undertaken during the first three months of 1985 to move the parties closer to agreement. Through the discovery process, the litigants were gaining an understanding of their opponents' lines of argumentation, on both legal and technical issues having to do with the fisheries, and were culling a more refined estimate of their chances of prevailing in court. The statements from the Washington state litigants exerted subtle pressure on the parties to settle out-of-court. Nonetheless, what was achieved by McGovern's attempt to separate and zero in on the biology of the Great Lakes fisheries was both a common recognition among the litigants of the uncertainties of the biologists' assessments and recommendations, and the concurrent construction of his own evaluation of the resource, which was not particularly "expert," but which had the potential to become authoritative if the negotiators failed to reach an agreement.

An Unstable Scientific Consensus. Despite the difficulties faced by the policy makers, biologists working for the major parties had been cooperating on fishery projects for several years. The Great Lakes Fishery Commission, an international organization founded in 1956, established lake committees comprising representatives of all government agencies (in Canada and the U.S.) holding resource management responsibilities on each of the Great Lakes to study indigenous lake species and coordinate population rejuvenation efforts. In 1980, the Tripartite Technical Working Group with biologists from the Michigan DNR, U.S. Fish and Wildlife Service (U.S. Department of Interior), and the tribes' newly-formed Chippewa-Ottawa Fishery Management Authority began meeting to compile data and set annual total allowable catch (TAC) levels on certain fish species in portions of Lakes Huron, Superior, and Michigan within the boundaries of the state of Michigan. The TACs represented a published consensus on recommended levels of fish catch by zones.[17]

It would seem that the TACs published in the annual *Status of the Fishery* reports compiled by the TTWG signalled the end of any adversarial or combative uses of scientific information or advisors. The TACs determined the "size of the pie" and biologists had little to say about into whose buckets the fish should fall. In fact, however, the reports represented not a true collaborative scientific finding, but a fragile compromise that could easily shatter if placed too close to any discussion on resource allocation. The matter of who should catch the fish was only thinly disguised behind more technically drawn arguments involved in establishing TACs.

Like many so-called "technical issues," the determination of TACs requires a mix of explicitly policy decisions and less conspicuous, value-bound, professional judgments. To begin with, TAC is dependent on a prior policy decision about the desired condition of the population under consideration. If population growth (as opposed to a stable or declining population size) is desired, a rate of growth must be targeted. For example, the federal Fish and Wildlife Service placed high priority on lake trout rehabilitation. For this species, they would tend to favor policies that would foster high population growth rates, such as a low TAC level, on the presumption that lower catch levels will reduce overall mortality rates and increase the probability that the lake trout population will reproduce. On the other hand, a group less concerned about lake trout rejuvenation might favor a much higher TAC, since their concern is short-term gains associated with catching fish.

Selecting a targeted growth rate for specific fish populations is clearly a decision guided by values, interests, and policy objectives. It is only the first of a series of negotiated points the TTWG members faced along the path to determining TACs, however. The next tier of issues concerned assumptions about variables used to establish TACs given a particular growth rate target, factors such as current population size, population age structure, individual growth rates, and mortality rates. On these points, value-bound, professional judgment comes into play in a more indirect way. Although some of the factors necessary for determining TAC are less controversial than others, all are merely estimates, based on extrapolations from data from sample studies, studies of comparable populations, or multi-purpose record keeping.

The link between these assumptions and the ultimate TAC determination is quickly apparent. For example, as mentioned earlier, TAC is dependent on overall mortality rates, which are defined by two components, fish catch level and natural mortality. Fish catch levels are recorded by the Fish and Wildlife Service based on catch reports submitted by licensed fishers. The natural mortality factor is less easily ascertained, but by convention, biologists have relied on the observed mortality rates of pristine populations.

The determination of both components of fish mortality became the subject of debate among biologists whose "professional judgments" clearly reflected political values and interest considerations. The Michigan DNR staff biologists took issue with the fish catch level component in establishing the mortality rate of lake trout. While fishermen for centuries have been chided for telling "fish stories" that exaggerate their conquests, DNR policy makers conversely accused tribal fishers of seriously underreporting their incidental lake trout catches. The DNR biologists accordingly argued that the FWS figures should be inflated when determining TACs.

Increasing the catch level component of the mortality rate used to determine TACs served an obvious political purpose. In the lake trout population, the Michigan DNR argued that the incidental lake trout caught by gill nets increased overall mortality to levels that inhibited lake trout reproduction. The DNR argued that restricting gill nets would reduce lake trout mortality and foster rejuvenation, without requiring a lowering of TAC levels that would diminish recreational fishing opportunities. Since only tribal fishers use gill nets, and some tribal fishers use gill nets exclusively, this interpretation of the cause of high mortality among lake trout populations had obvious implications for the allocation contest.

The natural mortality rate was open for debate as well. In this case, the tribes' biologists argued that the proportion of overall mortality attributed to natural mortality was underestimated. They argued that the use of mortality rates of pristine populations was inappropriate to estimate natural mortality of populations in environments that have undergone significant change, such as increased chemical pollution. The political motive for this line of argumentation is also fairly obvious: tribal biologists were attempting to defend the use of gill nets by shifting some of the onus of high mortality off the incidental catch component and onto industrial society more generally.

Given the intensity of the allocation dispute, it is unlikely that the biologists were completely unaware of the distributional implications of setting high or low TACs for specific species in particular zones. Undoubtedly, even while discussing the issues in a professional manner, they were honing arguments to edge TACs upward in fishing areas favored by their respective sponsors. Nonetheless, despite such politically motivated manipulations, it seemed that as long as the issue of who is catching the fish was kept out of the discussion, the biologists were able to agree on discrete figures for the variables used to determine TACs.

Appropriating Science. Given the fragility of the apparent consensus, how did Special Master McGovern deal with the technical aspects of the dispute? The structure of this alternative dispute resolution effort differed distinctively from the previous two cases in that the "technical experts," the fishery biologists, were consciously and deliberately convened at different times and places from the attorneys or the principals. McGovern's reasoning for this was simple. First, although McGovern himself did not mention this, according to one participant, Judge Enslen believed that the biologists could talk to one another on a professional level, whereas relations among the principals were overly strained. It is likely that Judge Enslen communicated his hunch to McGovern, but whether he did or not, McGovern could easily see that the biologists had been cooperating for several years on the TTWG. He thus wanted to take advantage and not jeopardize that communication channel.

According to McGovern, he also ascertained through conversations with individual biologists that the biologists *qua* biologists were disagreeing for two reasons. First, once there was any significant uncertainty in the analysis, individuals would go off in different directions with their own estimates of the appropriate figure to assume. Secondly, and not unrelated to the first issue, the policy makers who hired them were pushing certain policies and looking to the biologists to provide supportive scientific rationales. Distancing the biologists from their employers was therefore critical in McGovern's opinion, although he could not control, of course, communications that occurred outside of these meetings.

Although the hostilities among the principals were said to have been mirrored by the biologists to some degree, McGovern hoped that he could succeed at toning down the adversarialism and political posturing by the biologists if they met without their advisees. During the series of meetings that occurred over about a three-month period, McGovern and his assisting technical facilitator, Francine Rabinowitz, an urban planning professor and member of a Los Angeles law firm, continually tried to guide the group to a consensus on technical issues based on their common commitment to the fisheries as an ecological resource and their standards of professionalism. Not insignificantly, meetings and field trips were scheduled to encourage the group to lunch, dine, and travel together. Opportunities to emphasize areas of agreement were fully exploited, as well as thoughtfully worded questions intended to "shame the biologists into recognizing their areas of agreement."[18]

McGovern attempted to deal with the first issue, disagreement among the biologists in estimating values for various variables, by encouraging the biologists from the three major parties to collaborate on building a computer-based population model of the fisheries of the Great Lakes. His strategy was to narrow the areas of disagreement on technical issues by helping the biologists to identify all the factors they could most easily agree on, insert these figures into a mutually acceptable model, and leave the variables of greatest uncertainty (and hence the most difficult to reach agreement on) for the policy makers to deal with. Ultimately, he hoped the model could be used "hands on" by the parties during negotiation to try out different allocation proposals to see who would get how much of what kind of fish in which part of the lake under differing assumptions about disputed variables. For example, negotiators would be able to compare a proposal for a straight 50 percent split of all fish stock to one based on zone assignments, or contrast two or more different zonal apportionment schemes.

The group failed to develop the model McGovern had envisioned. According to McGovern, the failure was due to two major deficiencies: a lack of resources and skepticism about models generally. His second insight was perhaps not far off the mark. At least one biologist representing a key player confided that he would never have recommended reliance on the model to his advisees because he disagreed with many of the model's assumptions.

Although the model fell short of McGovern's original expectations for it, the exercise served other important purposes. First, the exercise helped the biologists to see more clearly the points of strongest agreement and disagreement and their relative importance. For example, the degree to which gill nets increased fish mortality was a point that seemed to be beyond settlement. Suspecting the difficulty it presented and the emotional overtones of the debate, since gill nets were used exclusively by the tribal fishers, Rabinowitz encouraged the group to leave the issue unresolved. The model that was subsequently constructed was run with "high," "moderate," and "low" values for the gill net mortality variable and, surprisingly to all, the model ultimately proved insensitive to these different levels. Thus, a point that might have become a lightning rod for reopening old wounds among the biologists was adeptly circumvented.

Perhaps more importantly than creating among the biologists a common frame of reference, the collaboration of the biologists helped to develop a technical base of reference for the special master. Given the special master's privileged status before the court, the biologists would be quick to recognize the significance that the collaborative product might eventually hold. They would thus be encouraged to fight strongly for so-called technical judgments embedded in the model that have clear implications for their principals. Because the model was correlated with zones, one might suspect that biologists would fight especially hard to "win" arguments that would set technical parameters in zones important to their principals. Although the modelling effort did not bring the biologists closer to agreement on technical and scientific issues, it created an alternative "authority," that, one might argue, was a sort of composite. Consequently, the model tactically served to move the parties closer to agreement not by dissolving disagreement among the scientists, but by creating an alternative "authority" that would legitimate the special master's allocation recommendation to the court, should the parties fail to settle.

Restructuring the Use of Science

The consensus-based methods utilized in these three cases differed from one another in many respects. The degree of interaction between experts and non-experts, the duration of the consensus-based procedure, and the nature and extent of the facilitator's intervention are just three of many ways in which *techniques* differed. Nonetheless, the unifying theme for distinguishing these methods remains unmistakable. These procedures aimed to clarify, resolve, or avoid disputes on key scientific and technical aspects of a decision, while allowing political conflict to become more salient. Scientific knowledge and expertise were used to inform decisions, but without confusing debates that result from an adversarial focus on science.

As such, these methods represent substantial advances over conventional approaches toward integrating scientific information and disagreement into politically stable decisions.

Addressing Criticisms of Conventional Methods

In contrast to the methods reviewed in Chapter 2, these consensus-based methods assumed that differing scientific and technical opinions and supporting evidence can be legitimate, given the existing state of knowledge. That is, rather than to dismiss all arguments but one, or attempt to gloss over differences in scientific or technical judgments, the decision makers and stakeholders attempted to ascertain the degree of confidence that could be placed in various scientific or technical arguments. In the New York City policy dialogue, this was achieved through intense periods of questions and answers in the presence of a formidable line of individuals highly trained in relevant areas of expertise. In the wood stoves regulatory negotiation, the basis of divergent views was revealed by encouraging those with competing views to explain their interpretations or present alternative analysis. The flexible format and the longer time frame in this case allowed parties to seek and generate additional information and analysis between sessions to enrich the common knowledge base for all discussants. Importantly in these two cases, the disclosure of the basis of scientific disagreement was performed openly in the presence of contending stakeholders as well as before representatives of the decision makers. Although expert advisors hired by a particular party might share value biases that would tend to produce scientific conclusions that advantage their sponsors, the "mixed" audience format apparently operates to filter out these biases to some degree, as individuals struggle to maintain a standard of "professionalism" among their peers as well as credibility among their own clients. Thus, although stakeholders' expert advisors may concentrate on critiquing data or analysis presented by contending groups, the end result tends to be less a stand-off than a joint recognition of the limits of scientific certainty.

Largely because of similar concerns about professional standards and because consensual methods appear to generate a stronger concern about clearing the air of misdirected information among all parties, scientific disagreements that were founded in illusion rather than substance were easily decloaked. "Miscommunication" tactics, such as using the same term to describe different phenomena as in the use of "worst case scenario" in the NYC case, were readily identified by stakeholders, expert advisors, decision maker representatives, or the facilitator.

The recognition of the legitimacy of contending scientific or technical arguments and the understanding that differences result from differing value judgments, force decision makers and stakeholders alike to acknowledge the inevitable intrusion of political influences into scientific disputes. Once it became clear through the policy dialogue that the CBNS risk assessments reflected, more than anything else, a far more conservative orientation toward risk, ignoring conservative attitudes could be seen as a political action. At that point, the decision makers could chose to lose political goodwill from a segment of the population or attempt to address their concerns. But, they no longer had the choice to ignore entirely the political interests behind the movement to stop the Brooklyn Navy Yard plant.

Recognizing the political nature of scientific disputes also, in a sense, appears to encourage policy players to state their concerns more explicitly. An increase in participants' understanding of competing and conflicting interests elevates the level of discussions. Groups who initially supported competing decision alternatives might discover that their interests are different, but not conflicting. In the wood stoves case, for example, the traditional rivals were the clean air advocates and the affected industry. Clean air advocates wanted a numerical standard that would result in improved air quality while the wood stove manufacturers, on the other hand, were most concerned about a compliance date that could be accommodated within existing production schedules. As long as the standard was attainable with available technology, any standard requiring modifications in stove design would require a minimum amount of time to redesign and retool production lines. Thus, although the objectives of the clean air advocates and the industry were divergent, their interests were less in conflict than appeared at first sight. Without a climate that encourages the discussion of political interests on this level, decisions that attempt to integrate such concerns are far less probable.

Finally, perhaps one of the more salient changes evident from these examples of consensus-based methods is the consistent function assigned to scientists and technical experts. Whereas the degree of discretionary decision making authority implicitly conferred onto scientists is unclear in conventional processes that place undue weight on scientific and technical factors, the role of scientists is less ambiguous when scientific and technical components are treated as guides and aids, not determinants. Consensus-based methods that are aimed at obtaining approval from all participants appear simultaneously to bring all individuals up to a common plane of technical competency. When experts are aware that they must explain the logic of their arguments rather than simply ride on their reputations to win concurrence, they too make more serious efforts to educate the stakeholders. The division between experts and non-experts narrows.

A New Role for Science

In all three cases examined, science had been, or potentially would have been, utilized strictly to support or discredit one policy alternative. Prior to the policy dialogue, the New York City dispute was a classic case of two polar opposite policy options standing head-to-head behind inconsistent risk estimates by technical experts. Although the basis for the divergences could be gleaned from a careful reading of the competing reports, the facilitated policy dialogue opened communication between reputable technical specialists and members of the concerned public, especially staff from the decision making Board of Estimate offices, and allowed an opportunity for the experts to elaborate on the reasons why ambiguities exist. In the course of their comments, they made clear that much of the cause of the uncertainty was inherent in the scientific enterprise, and was not something that could be corrected or eliminated through additional investigation or further testing, at least not within a reasonable amount of time. Thus, the discussions ended any possibility of the decision makers deferring a decision for further study or seeking authoritative guidance from scientists and it became clear that the risk assessments represented little more than varying orientations toward risk. The decision "to build" or "not to build," similarly reflected differences in a willingness to accept (or impose) a health risk.

A somewhat different dynamic prevailed in the wood stoves regulatory negotiation. Parties entered the negotiations with a fairly strong sense of the relative scarcity of pertinent scientific data and information. The "win-win" euphoria that many popular writings and workshops on negotiation exalt did not lull stakeholders into assuming that technical arguments did not matter, however. Stakeholders with access to technical studies went fully equipped and prepared to state their arguments in a manner most flattering to their interests. Nonetheless, unless their evidence was incontrovertible, the cross-examination by adversaries reduced many studies to "good guesses" rather than definitive statements. As such, the fire power of their technical support systems was dampened and stakeholders acceded to bargaining over ranges (of estimates for technical factors) and across issues.

Finally, in the Michigan fishing case, the use of science was transformed in two, interesting and distinct ways. First, debate over scientific issues concerning the biology of the Great Lakes fishery was almost entirely absent from the final negotiations. Biologists were not present in the negotiations, except as consultants to be conferred with during caucusing. Negotiating representatives of the major stakeholders simply checked back with their biologists to assess the catch implications of different allocation proposals. Apparently, the estimations of catches in different zones were not sufficiently divergent to evoke debate.

What is meant by "sufficiently divergent?" This leads to the second point. The stakeholders were negotiating under intense pressure to settle. The fishing dispute had been ongoing for years and communities were reeling under the animosity between tribal fishers and non-tribal fishers, with outbreaks of physical violence, verbal abuse, and overtly racist media commentaries. A court trial date was approaching. Each party was aware of serious defects in their legal arguments and the outcome of a trial was highly uncertain for all parties. Most importantly, the court's appointment of a special master meant that the court most likely would rule in accordance with Special Master McGovern's settlement recommendation. If the parties did not reach an agreement, a settlement would be imposed on them.

The role that science came to play in the settlement was secondary compared to what might have occurred in the courtroom. It was not used as a weapon by the stakeholders. If a weapon in any sense, it was one in hands of Special Master McGovern who through the mediation process had gained sufficient understanding of the technical issues to provide Judge Enslen with a credible technical base for an allocation decision.

If science under conventional decision making is deployed as a weapon to persuade decision makers or the polity to accept a given decision alternative, then altering the role of science through consensual approaches will have implications for the ability of different groups to exert influence over public decisions. How are the dynamics of political power affected by the use of supplementary, consensus-based methods in decisions presumed to be informed by scientific and technical information and expertise?

Notes

1. These writers contribute to what is called the "constructivist" view of science. The constructivist view looks toward the external culture that furnishes "interpretive resources" that shape scientific knowledge for political purposes. For further elaboration, see Ditta Bartels, "Commentary: It's Good Enough for Science, but Is It Good Enough for Social Action?" *Science, Technology, and Human Values*, 10(4): 69–74, 1985.

2. For a fuller discussion of these factors, see Ozawa and Susskind.

3. I will use the term "intervenor" to mean the range of roles called "convener," "facilitator," or "mediator" in the negotiation literature.

4. This phrase was used by Walter Shaub to describe the events during the policy dialogue (New York Academy of Sciences, 1984b).

5. See, for example, Commoner's *The Closing Circle*.

6. This was a reason given by Dr. Commoner during a personal interview in October 1986, at the Center for the Biology of Natural Systems, Queens College, Flushing, New York.

7. Unless otherwise noted, the following description of the Wood Burning Stoves regulatory negotiation is based on interviews listed in Appendix 1.

8. The list of participants is provided in Appendix 2.

9. See Roy J. Lewicki and Joseph A. Litterer, *Negotiation*.

10. Based on comments made by David Doniger during a telephone interview, May 1987.

11. Based on comments by R.D. Gros Jean during telephone interview, May 1987.

12. Based on comments by John Charles during telephone interview, May 1987.

13. William Becker, John Canaday, Donnis Corn, and David Doniger were among the participants who made comments along this vein during telephone interviews, May and June 1987.

14. Statements were given by David Doniger and David Swankin during interviews, May 1987.

15. Based on comments made by William Becker, John King, and Harold Garabedian during telephone interviews, May and June 1987.

16. The description of the Michigan fishing case is drawn largely from interviews conducted by the author in 1987.

17. TACs are as much policy- as science-based, because their determination is dependent on a targeted level of population growth. In other words, a mortality rate of 60 percent or 70 percent may both protect a given population, but the lower rate will be more likely to result in a higher rate of reproduction and hence population rejuvenation. Since TAC is simply the catch level correlated with given mortality rates, a TAC determination is predicated on agreement on a targeted rehabilitation rate. Biologists favoring rejuvenation over human-oriented concerns, such as short-term economic stability, for example, may support an assumption of higher rehabilitation targets and lower catch levels. Conversely, DNR biologists familiar with the state's commitment to sports fishing may tend to endorse slower (though steady) population growth rates for popular sports species, such as lake trout in tourism-dependent locales.

18. Based on telephone interview with Francine Rabinowitz, July 1987.