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The social construction of acid rain

Some implications for science/policy assessment

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There is currently a great deal of discussion in various national and international fora about how to design global change research programmes (for an overview of part of the terrain see Price).[1] American policy analysts often invoke the National Acid Precipitation Project (NAPAP) as a textbook example of how not to do policy relevant research (see for example US Congress, Office of Technology Assessment[2]). We fear that the wrong conclusions are being drawn from the NAPAP experience. In this paper we re-examine NAPAP with a view towards discovering what this experience can teach us about global change research initiatives.

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[1]M Price, *Options for EC-Level Research Activities on the Human Dimensions of Global Change*, Environmental Change Unit, Oxford, 1994

[2]US Congress Office of Technology and Assessment, *Preparing for an Uncertain Climate*, US Government Printing Office, Washington, DC, 1993

In 1980, Congress established the National Acid Precipitation Assessment Programme (NAPAP) to investigate the causes and effects of acid deposition and to recommend strategies for the control and mitigation of adverse effects. Lasting ten years and costing nearly US$600 million, NAPAP's studies focused on the following areas: emissions of acid precursor pollutants; atmospheric transport and transformation dynamics; acid deposition regimes and air quality monitoring; and effects on surface waters, aquatic life, forests, agricultural crops, building materials and cultural artifacts, human health, and visibility degradation. The stated objective of the programme was to produce an unbiased scientific understanding upon which to develop policy. NAPAP was typical of a growing tendency to involve science in the environmental policy making process. Other examples concern electro-magnetic fields, criteria air pollutants, cyclamates, and Alar.

We will refer to this practice of enlisting science as the foundation for public policy decisions as science/policy assessment (SPA). SPA is rooted in the positivist credo that science is uniquely reliable among human institutions in identifying fundamental truths about the world. The truths discovered by science are taken to be value free and universal. However diverse in culture, gender, training, and values, scientists should reach the same conclusions if they conform to the canons of scientific method. Beyond a certain boundary, debate and disagreement mark an inadvertent failure to do good science or outright bias or distortion.[3] SPA establishes a division of labour: the policy making process, often irrational and convoluted, incorporates public values, while science, in its rigour and elegance, provides the relevant facts. By appearing to establish a view of reality that is categorical and certain rather than contingent and negotiable, science occupies a crucial role in contemporary policy debates.

In recent years the view of science implicit in SPA increasingly has been challenged.[4] Philosophers, sociologists, historians and even some scientists have emphasized the role of social and cultural factors in shaping scientific outcomes. On this emerging view, science is not characterized by its findings or discoveries so much as by the social institutions and practices which produce and validate them. Drawing on this view, we will argue that it is a mistake to suppose that 'good science' can always provide a 'right answer' for science-based policy disputes. Indeed, good science may not even be able

to provide a range of right answers for a given policy question. An intensive research programme such as NAPAP can produce a 'banquet' of findings, but cannot determine which 'dishes' should be considered by decision makers and which should weigh most heavily in the policy choice. Indeed, a wealth of high quality, peer reviewed findings may even accentuate a dispute by providing evidence for all sides of the policy debate. While our conclusions are not novel, they are especially timely and relevant as we continue to expand a multi-disciplinary, interagency global change research initiative. There is widespread agreement about the need to learn from the NAPAP experience, but it is important to see just what the correct lessons are.

The conventional critique of NAPAP

A virtual cottage industry of retrospective studies of NAPAP has sprung up in recent years.[5] Perhaps the most authoritative critique was developed by a group of distinguished scientists who served on the NAPAP Oversight Review Board (ORB). The ORB was convened by NAPAP management to provide advice and a written assessment regarding the quality, scientific integrity and credibility of the various parts of the NAPAP assessment. The ORB evaluation of NAPAP can be summarized around two main themes. First, NAPAP's scientific and technical products were of high quality and 'worthy to be used to inform public policy decisions'.[6] Second, the programme was 'guided to an excessive degree by ... interesting scientific questions rather than by the potential to improve policy decisions.[7] Put bluntly, NAPAP got high marks for scientific quality and rigour, but not as an exercise in SPA.

As part of its evaluation, the ORB also published a report on lessons learned about the interface between science and public policy. The panel's key recommendation was to give assessment primacy over research. As the ORB explained this, 'science and research findings *per se* have little to offer directly to the public policy process. Their usefulness depends on assessment, defined as the interpretation of findings relevant to decisions'.[8] In other words, 'scientific research [needs to] be moulded to contribute needed information on a schedule to meet [policy] needs'.[9]

This theme is echoed by Rubin, Lave and Morgan in a recent essay.[10] Like the ORB, Rubin and his colleagues claim that 'a major failure of NAPAP is that no serious effort was made to define policy-related research priorities and then to shape an appropriate set of projects ... to answer these questions in a meaningful way'.[11] For future programmes, such as in the area of climate research, the authors suggest four basic steps: (1) clearly identify the programmatic objectives; (2) articulate an appropriate management plan; (3) survey the state of science to reach 'informed judgements about what we know and what we don't know'; and (4) identify research needs to fill gaps and resolve uncertainties. In short, the goal should be to 'ask the right questions, influence the research agenda, and help ensure that the appropriate priorities are established.'[12] The authors seem to believe that scientific guidance will be clear-cut, sharply relevant and authoritative once we stop asking science questions and start asking policy questions.

In our opinion, the shift that Rubin *et al* advocate will not resolve uncertainty so much as shift it from one discourse to another – from the language of the physical sciences to the arcane and equally technical discourse of policy analysis. But a deeper problem with their approach is that they expect too much from science. On the one hand, they want to rely on the authority of science to provide an objective, value-free basis for the resolution of policy disputes. On the other hand, they want science to be 'communicable' and

[3]D Collingridge and C Reeve, *Science Speaks to Power: The Role of Experts in Policy Making*, St Martin's Press, New York, 1986, pp 9–10

[4]See, for example, S Jasanoff, *The Fifth Branch: Science Advisers as Policy Makers*, Harvard University Press, Cambridge, MA, 1990; H E Longino, *Science as Social Knowledge*, Princeton University Press, Princeton, NJ, 1990; C P Ozawa, *Recasting Science: Consensual Procedures in Public Policy Making*, Westview Press, Boulder, CO, 1991

[5]The Programme/Abstracts for the 1992 AAAS Annual Meeting indicates at least five seminars on NAPAP. Published critiques include: G E Likens, *Toxic Winds: Whose Responsibility?* in F H Bormann and S R Kellert (eds) *The Broken Circle: Ecology, Economics and Ethics*, Yale University Press, New Haven, CT, 1991; R W Rycroft, 'Environmentalism and science: politics and the pursuit of knowledge', *Knowledge: Creation Diffiusion, Utilization*, Vol 13, No 2, 1991, pp 150–169; D W Schindler, 'A view of NAPAP from north of the border', *Ecological Applications*, Vol 2, No 2, 1992, pp 124–130; C Y M Wong,'NAPAP, a perspective', *Water Environment and Technology*, Vol 10, No 10, 1991, pp 42–44; E S Rubin, L B Lave and M G Morgan, 'Keeping climate research relevant', *Issues in Science and Technology*, Vol 8, No 2, 1991–92, pp 47–55; ORB, *The Experience and Legacy of NAPAP*, Report to the Joint Chairs Council of the Interagency Task Force on Acidic Deposition. The Oversight Review Board of the National Acid Precipitation Assessment Programme, NAPAP Office of the Director, Washington, DC 1991

[6]ORB, *op cit*, Ref 5, p 7

[7]*Ibid*, p 8

[8]*Ibid*, p 26

[9]*Ibid*

[10]Rubin *et al*, *op cit*, Ref 5

[11]*Ibid*, p 48

[12]*Ibid*, p 51

yoked to a policy agenda. But science cannot play both of these roles simultaneously. Science gains its authority by remaining aloof from overtly normative practices. When it is hitched to policy concerns, its claims to authority based on an objective, value-free investigation of nature are eroded. Once science is seen another player in the policy game, as is increasingly the case, its special authority begins to vanish. The dilemma is this: insofar as science is left to go its own way, it fails to be decisive in resolving policy disputes; on the other hand, if science is enlisted in the service of policy making, it loses its authority. Science is an interpretive and value-laden institution whose authority is both potent and precarious. If we are to make progress on various science-based policy disputes, we will have to be much clearer about what the problems are and what counts as 'solutions'.

The problem of problem definition

Thomas Schelling once remarked that a critical element of any problem is what the problem is seen to be.[13] For instance

> if we define [a state of affairs] as a 'rural poverty' problem, a whole array of concerns, players, methods of analysis and agencies orient themselves accordingly. If we defined it some other way – as a carrying capacity issue, as an economic distribution issue, or as an agricultural or public welfare issue – then the arrangement of players and methods of analysis would alter.[14]

The SPA approach assumes that issues arise when disinterested researchers notice that something is amiss or at least worth investigating. The world poses the problem and science brings it to our attention. But as suggested by Schelling, problem definition can be viewed as a process in which we *denote* the things on which we will focus and *frame* the context in which we will study them. The world does not denote and frame itself; rather, denotation and framing are imposed by humans who have various interests and preoccupations, and are engaged in particular activities. As Donald Schon writes:

> problems do not present themselves as givens . . . When we set the problem, we select what we will treat as the 'things' of the situation, we set the boundaries of our attention to it, and we impose upon it a coherence which allows us to say what is wrong and in what direction the situation needs to be [analysed].[15]

Congress' stated objective in establishing NAPAP was to determine whether acid precipitation was a problem and if so, what could be done about it.[16] The problem with such a charge is that acid rain can be characterized from numerous valid perspectives: chemical meteorological, biological, ecological, social and others. Within each of these perspectives, further specialization is not only possible, but from a research point of view, absolutely necessary. The atmospheric dimension of acid rain involves sub-disciplinary work in the areas of emissions processes and amounts, transformation chemistry, transport dynamics, and deposition regimes. However, the problems of any of these sub-disciplines may not be relevant to other substantive areas. They are likely to involve different spatial and temporal scales; different chemical species; different degrees of analytical detail due to different levels of expertise and/or funding; different assumptions concerning non-measured parameters; broadly divergent (and sometimes inconsistent) modelling approaches and statistical testing procedures; and so on.

NAPAP was hostage to disciplinary science. Acid rain was not treated as a unified issue. It became many separate issues, each bounded by disciplines,

[13]T Schelling in P Timmerman, *Everything else will not remain equal: the challenge of social research in the face of global climate warming. Impacts of Climate Change on the Great Lakes Basin*, Joint Report No 1 of the US National Climate Programme Office and the Canadian Climate Centre, National Climate Programme Office, Washington, DC, 1989

[14]The example of rural poverty is discussed in D A Schon, *The Reflective Practitioner: How Professionals Think in Action*, Basic Books Inc, New York, 1983, pp 190–199

[15]*Ibid*, p 40

[16]Of course, the motivations for the establishment of NAPAP were complex. It is likely that different parties had different motivations, some of which may have had little to do with the desire to find out if acid rain was a problem. For an excellent history of acid rain reserch, see E B Cowling, 'Acid precipitation in historical perspective', *Environmental Science and Technology*, Vol 76, No 2, 1982, pp 110A–123

sub-disciplines, and methodological conventions. The Programme reported findings in excruciating disciplinary detail, an approach which was not helpful to non-specialist decision makers. The disciplinary pluralism of NAPAP allowed policy advocates to pick and choose among NAPAP's reported findings, emphasizing facts or uncertainties supporting a particular position and de-emphasizing others. Defining its activity in terms of disciplinary science, NAPAP lacked a supra-disciplinary perspective that would have allowed it to characterize acid rain as a problem, non-problem or something in between.

There is no such thing as a generic problem. Problems are defined, characterized and constructed, not given to us by brute nature alone. Policy needs, public values, and differing scientific perspectives are all elements in problem definition.[17]

The open texture of scientific findings

Consider your right hand. It can be described in terms of size, shape, colour, cellular structure, molecular structure or in numerous other ways without exhausting possible descriptions. Qualities that would be essential to a forensics expert would likely be irrelevant to a glove designer. True descriptions of empirical objects are inexhaustible and one is not better than another for all purposes. Waismann [18] argued that the 'open texture' of scientific findings makes it impossible to provide complete descriptions of most empirical concepts. For this reason empirical claims can rarely be verified completely. More tests can always be demanded and additional descriptions can always be given.

Because empirical concepts are open textured, a science-based assessment of a policy related issue can always be charged with 'sins of omission'. For instance, acquatic damage from acid deposition can be characterized in several ways. If damages are stated in terms of the *number of lakes affected*, then projections of decreased deposition appear to provide a substantial decrease in damages. If the same projection is expressed in terms of *percentage of affected lakes*, then the decrease in damage appears less significant. If acidity is characterized in terms of pH rather than acid neutralizing capacity, then future gains would be smaller still. Moreover, the choice of a reference pH value can radically alter the number of 'acidic' surface waters.[19] Numerous decisions must be made: A national scale assessment gives short shrift to regional 'hot spots'; a focus on chronic acidity produces a different perspective than one including short-term episodes; analyses dealing with the current situation may inadvertently miss longer-term processes threatening future degradation; and monitoring for direct effects does not preclude the possibility of indirect or synoptic effects. Still another consideration is whether chemical acidification has actually harmed aquatic life. All of these measures are valid, but no one (or combination) of them is intrinsically more correct than the others. Therefore, arguments over the characterization of aquatic damage from acid deposition have the potential to last indefinitely. Scientific considerations alone cannot compel closure.

Disciplinary boundaries, evidential adequacy and closure

Academic science limits the problem of open texture through disciplinary isolationism. Scientific disciplines provide the frameworks through which phenomena are constituted into specific research problems. In Thomas Kuhn's classic discussion of scientific paradigms, disciplines are singled out as vehicles through which nature becomes conceptualized, and hence understood.[20] As Kuhn writes, 'a problem must be characterized by . . . rules that

[17]J A Bradbury, 'The policy implications of differing concepts of risk' *Science, Technology and Human Values*, Vol 14 No 4, 1989, pp 380–399

[18]F Waismann, *Verifiability*, in G H R Parkinson (ed) *The Theory of Meaning*, Oxford University Press, Oxford, 1968

[19]Schindler, *op cit*, Ref 5, p 125

[20]Even 'basic' or academic science has to deal with boundary disputes. Bauer's observations are quite pertinent: 'You do not have to be long in a chemistry department to learn that chemists are no homogenous tribe but rather a (sometimes uneasy) confederation of several distinct tribes . . . Naturally each tribe and sub-tribe thinks its own way of doing things to be the best way, the *scientific* way. So theorists tend to believe that experimental evidence is important only insofar as it suggests new theory; and if experiment and theory happen not to agree, the theorists will often prefer to believe the theory rather than the (experimental) evidence. Experimentalists, on the other hand, regard that as perverse; they know it is observation and experiment that teach us how the world works, theories being only devices that make it easier to remember the facts', H H Bauer, *Scientific Literacy and the Myth of Scientific Method*, University of Illinois Press, Chicago, IL, 1992, p 21. A similar tension is described by Pinch in his investigation of prevalent uncertainties in solar neutrino science, with a focus on differences between its constituent sub-disciplines: radiochemistry, nuclear physics, astrophysics and neutrino physics. Pinch concludes that scientific uncertainty is quite contentious in basic science and that 'it is no surprise to find similar debates appearing in public science controversies', T J Pinch, 'The sunset: the presentation of certainty in scientific life', *Social Studies of Science*, Vol 11, 1981, pp 131–158

limit both the acceptable solutions and the steps by which they are to be obtained'.[21] Indeed, it would be difficult to imagine the advancement of science were it not for the delimiting quality of disciplines.

While disciplinary boundaries are helpful in 'normal' science, they are a difficulty in science/policy assessments.[22] Boundary issues are disputes over whose rules, constructs and methods are most relevant or 'weighty' in a given situation. The transdisciplinary character of policy issues makes these disputes difficult to avoid. This, in turn, leads to disputes concerning the strength and validity of scientific evidence. This problem of what counts as evidence bears on the question of when a hypothesis can be regarded as confirmed, or more properly, when the null hypothesis can be rejected.

Consider a homely example.[23] Suppose you return from a baseball game and say to a friend, 'Brown hit a home run'. Suppose the friend questions your claim. She might ask you to describe the impact of bat on pitched ball, the trajectory of the ball as it soared out of the park, and the incremental change in score. But suppose she deems this information inadequate and wants to know if Brown touched all four bases, whether the batting order was submitted to the umpire, whether Brown was playing with league-sanctioned shoes, and so on. The validation process could become unmanageable.

The case of Brown's home run is not fundamentally different from other instances of empirical characterization, such as whether lakes have become acidic. Disputes over the validity of a home run do not last forever because baseball has rules for defining home runs and other baseball constructs. Similarly, practices and conventions internal to scientific institutions determine evidential adequacy. Phenomena do not pronounce their own relevance or evidential adequacy.

A major impetus for acid rain control was the fear of damage to aquatic resources. Like other areas of the NAPAP Assessment, the characterization of aquatic effects was to be based on a 'weight of evidence' approach. This means that different lines of analysis were pursued, different regions considered, and an overall conclusion pieced together from the various elements of study. NAPAP's aquatic effects research team determined that 4% of US lakes and 8% of streams are currently acidic, values that vary from region to region. Florida has the highest percentage of acidic surface waters (33% of lakes and 39% of streams). In the mid-Atlantic Highlands, mid-Atlantic Coastal Plain, and the Adirondacks, 6–14% of lakes and streams are chronically acidic; about three times that number are subject to periods of temporary acidification (days to weeks) due to storms and snowmelt conditions.[24]

As soon as these facts were reported, other scientists argued that many lakes are naturally acidic because forest soils and organic decomposition produce a 'normal' acidic condition, especially for smaller lakes. In other words, lakes and streams may indeed be acidic, but not because of air pollution. Indeed, some scientists have argued that the dominant theory of surface water acidification is wrong, failing to address natural factors controlling surface water chemistry.[25] Not surprisingly, this view has been criticized for misinterpreting the results of previous studies.[26] What is significant about this debate is not so much who is right, but rather that it illustrates the interpretive nature of science.

The issue of evidential adequacy arises frequently in SPA. Indeed, one writer even suggested that 'government agencies need to explicitly consider [adopting] "stopping rules" before they embark on ... programmes of ... research. As the research progresses they need to continue to refine those rules in light of what has already been ... learned'.[27] This proposal sounds eminently sensible. However, the difficulty lies with the fact that 'what has

[21]T S Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago, IL, 1970

[22]D Fisher, 'Boundary work: a model of the relation between power and knowledge', *Knowledge: Creation, Diffusion and Utilization*, Vol 10, No 2, 1988, pp 156–176; T F Gieryn, 'Boundary work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists', *American Sociologicial Review*, Vol 48, 1983, pp 781–795

[23]J Searle, *How to derive an 'ought' from an 'is'* in W D Hudson (ed) *The Is/Ought Controversy*, Macmillan, New York, 1969

[24]NAPAP, *1990 Integrated Assessment Report*, US Government Printing Office, Washington DC, 1991

[25]E C Krug, 'Fish story: the great acid rain flimflam' *Policy Review*, Spring 1990

[26]K N Eshelman, *Synthesis of comments and issues related to the role of organic acids in the acidification of surface waters*, Final Report, submitted to Kilkelly Environmental Associates, Raleigh, NC 27622, 20 May 1990; Schindler, *op cit*, Ref 5

[27]M G Morgan, 'Risk research: when should we say 'enough'? *Science*, Vol 232, No 4753, 1986, p 917

already been . . . learned' is itself subject to interpretation. Rules governing when to draw a conclusion may be necessary, but when scientifically incontestable tests of evidential adequacy are unavailable, decidedly 'non-scientific' criteria will prevail. Science/policy assessments need explicitly to address the fact that scientific conclusions are subject to non-scientific, value-laden considerations.

The conventional critique revisited

We have argued that problem definition depends upon interest and perspective; that evidential adequacy is relative to institutional conventions; and that the open texture problem cannot be addressed in terms that are strictly scientific. As one of their basic recommendations, Rubin and his colleagues emphasize the need to survey the state of science to determine research needs; yet they neglect the fact that the outcome of such a survey is inevitably debatable. NAPAP did not fail because its founders neglected to survey the state of science and identify key uncertainties. If NAPAP failed it was in part because it placed too much emphasis on the type of authoritative inventory that Rubin and his colleagues advocate. What is regarded as unknown or uncertain depends profoundly on what one needs to know or do. To expect the scientific community to agree on research needs is to ignore the fact that 'need' is an evaluative term, highly dependent on disciplinary or theoretical orientation. The very notion of scientific uncertainty is only meaningful from a specific perspective. The conventional critiques of NAPAP are not very illuminating because they fail to consider the interplay between science, values, and policy.

In our view rather than pointing in the direction of clarity and policy relevance, Rubin and his colleagues could lead us further in the direction of obscurity and confusion. They advocate 'integrated assessments' of potential mitigation strategies 'to address the anticipated needs of policy makers'.[28] Such a recommendation presumes that policy makers will define the problem in the same terms; that their 'needs' are transparent and remain constant as time, circumstance, and political fortune change; and that they agree on the implicit assumptions, structural biases, and other technical elements of the assessment framework. These are implausible assumptions. Science/policy decisions are greatly affected by values, which are sometimes explicit in the context of ethical/political discussion but often implicit in the scientist's methods. In our opinion we would do better to address the value questions directly and upfront rather than covertly and downstream. Like many policy analysts Rubin and his colleagues seem to insist that value considerations should be driven further underground. But value differences cannot be so easily disabled. If value questions are not explicitly addressed through dialogue, they will continue to occur obliquely in the rhetoric of science (now economics and policy analysis rather than physics, chemistry and biology). The root problem has still not been addressed: the use of technical discourses to suppress the messy political process of discussion, deliberation and community-building.

The real lesson of NAPAP: more realistic expectations of SPA

If we accept the claim that problems, solutions, and evidence are constructed, contextual, and relative, then it stands to reason that different individuals and groups will have enormous incentives to constitute facts in ways that are consistent with their needs, interest and orientations. There is nothing startling or

[28]Rubin *et al*, *op cit*, Ref 5, p 53

novel in this observation. What is extraordinary is the assumption that any group, if given the choice, will passively allow another group to constitute its reality. This, however, is exactly what some of NAPAP's critics seem to assume.

If facts are understood as interpretive constructs, then SPA should come to be viewed in a much more political light, involving strong elements of negotiation and give and take. We should abandon the idea that SPA will provide us with objective or unimpeachable values, or even characterizations that are free of the biases and perceptions of their creators. But recognition of the value-bound character of science/policy assessment should not be seen as a sign that scientific tools are inappropriate for use in policy deliberations. As Rayner writes:

> By denying the possibility of directly comparing knowledge to nature, except through the culturally created categories of human thought, we are emphatically not denying the existence of any basis for validating public knowledge, of which scientific knowledge is one sort ... The point is that public knowledge can only be evaluated as a whole system, a process of production and use, and not as an artifact to be compared against nature's pattern.[29]

Science plays a crucial role in setting agendas, framing problems, and supplying concepts and vocabularies. However science is not the only player in SPA. The political nature of the problems that SPA is used to address implies that the context of scientific research may be just as important as the results, perhaps more so. When people claim that NAPAP was a failure or express frustration about policy gridlock, they often assume that the relevant actors are all addressing the same problems and have the same conception of what counts as a solution although they may have different preferred outcomes. But this is not the case. Some parties to a policy conflict may view lack of closure as success. Or success may consist of using the process to 'consciousness raise' for a particular cause, even if they have no expectation that their preferred policy will win. Some parties may not even think that a real problem is being addressed.

The observations that we have made in this paper may strike some as a brief for a science/policy irrationalism in which 'anything goes'.[30] But that is not our position. An increased sensitivity to the fact that scientists come at problems from divergent perspectives is needed; but more important is the need for widespread recognition that science cannot 'answer' policy questions. It can provoke, structure, and inform the debate, but it cannot bring it to closure. Science should be viewed more as an enabling device, helping to define problems, and less as a closure device, helping to establish and evaluate conclusions. SPA should be viewed as a front end activity that will help disputants carve out an area of commonality – a community – and hence to enable the issues of public value to be debated in the clear, not obscured behind competing technical characterizations.

What do these considerations suggest for science/policy assessment in the area of global environmental change? First, the interpenetration of facts and values in the area of global change is so pervasive that virtually every area of expertise is relevant to constructing the problem, proposing solutions, and establishing evidential relations. Second, since facts, problems and solutions are community achievements rather than gifts bestowed upon us by the nature of things, much greater attention should be paid to communication and community-building. A global change research programme should not simply fall out of what a scientific or policy community regards as important. For such a programme to be successful there must be widespread agreement on what questions are being asked, why they are important, what counts as

[29]S Rayner, *Risk and relativism in science for policy,* in B B Johnson and V T Covello (eds) *The Social and Cultural Construction of Risk*, 1987, p 7

[30]Some may draw this conclusion because they cannot imagine that discourse about values can be rational and persuasive, much less figure in object judgements. For further discussion, see D Jamieson, *Managing the future: public policy, scientific uncertainty and global warming,* in D Scherer (ed) *Upstream/Downstream: Essays in Environmental Ethics*, Temple University Press, Philadelphia, PA, 1990; D Jamieson, *Method and moral theory*, in P Singer (ed) *A Companion to Ethics*, Basil Blackwell, Oxford, 1991; D Jamieson, 'The epistemology of climate change', *Social and Natural Resources*, Vol 4, 1991, pp 319–329; D Jamieson, 'Ethics, public policy and global warming', *Science, Technology and Human Values*, Vol 17, No 2, 1992, pp 139–153

answers to them and what the social use of these answers might be. These are the real lessons of NAPAP. The greatest mistake of all would be to examine the NAPAP experience carefully, and then draw the wrong conclusion. It appears to us that this may be the course that we are on.