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# Uncomfortable knowledge: the social construction of ignorance in science and environmental policy discourses

Steve Rayner

## Abstract

To make sense of the complexity of the world so that they can act, individuals and institutions need to develop simplified, self-consistent versions of that world. The process of doing so means that much of what is known about the world needs to be excluded from those versions, and in particular that knowledge which is in tension or outright contradiction with those versions must be expunged. This is 'uncomfortable knowledge'. The paper describes four implicit strategies which institutions use to keep uncomfortable knowledge at bay: denial, dismissal, diversion and displacement. It concludes by suggesting that 'clumsy' arrangements may need to be constructed to ensure that uncomfortable knowledge is not excluded from policy debates, especially when dealing with 'wicked problems' where the accepted version excludes knowledge that is crucial for making sense of and addressing the problem.

Keywords: uncomfortable knowledge; social construction of ignorance; organizational strategies.

The message is that there are no 'knowns.' There are things we know that we know. There are known unknowns. That is to say there are things that we now know we don't know. But there are also unknown unknowns. There are

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things we don't know we don't know. So when we do the best we can and we pull all this information together, and we then say well that's basically what we see as the situation, that is really only the known knowns and the known unknowns. And each year, we discover a few more of those unknown unknowns.

(Former US Secretary for Defence, Donald Rumsfeld,  
NATO Headquarters, 6 June 2002)

### Knowing and not knowing

It is perhaps unfair that Donald Rumsfeld was widely lampooned for his famous attempt to parse the relationship between knowing and not knowing. For it is true, as he said at the time, that 'It sounds like a riddle. It isn't a riddle. It is a very serious, important matter.' But in drawing attention to what we know we know, what we know we don't know and what we don't know we don't know, Rumsfeld altogether omitted what is possibly the most intriguing combination: what we don't know we know. This last category covers a variety of possibilities. For example, it might refer to 'tacit knowledge' of the sort described by Michael Polanyi (1966), which is knowledge that is either intuitive or associated with experiential craft skills but is not verbalized. Alternatively, it might refer to knowledge that exists somewhere else in a society or organization, but is not known here, either because the holder deliberately conceals it, as has been the case with the tobacco industry or pharmaceutical companies (McGoey & Jackson, 2009), through neglect (Littlewood, 2009), to elide responsibility (McGoey, 2007) or to avoid individual or organizational embarrassment or revulsion (Cohen, 2001). In this paper I explore unknown knowns of a particular sort: those which societies or institutions actively exclude because they threaten to undermine key organizational arrangements or the ability of institutions to pursue their goals. My interest is therefore in how information is kept out rather than kept in and my approach is to treat ignorance as a necessary social achievement rather than as a simple background failure to acquire, store and retrieve knowledge.

Through her work on institutional memory, Mary Douglas (for example, 1986, 1995) is one of the more influential theorists to consider how the achievement of non-knowledge can serve as a tool of political and social authority. She traced her interest in collective memory and what she termed 'structural amnesia' to the work of her teacher, E. E. Evans-Pritchard, who observed the curious phenomenon that the history of the Nuer appeared to be fixed at about a dozen generations (Evans-Pritchard, 1940). He argued that this was because the complex system of transferring rights to cattle at key stages of the life cycle such as marriage and death depended on detailed knowledge of kinship extending back five generations. However, beyond the minimal lineage, only the names of the founders of the important lineage branches need be placed in a determinate order of ascent, from the founder of the minimal lineage

to the clan founder, because these are the only individuals from so far back in history who are still important reference points for the living. The generations that passed between these various individuals are forgotten and the history of each of their epochs is collapsed into a single ancestral generation.

‘It is evident . . . there has been a telescoping of the agnatic line from the founder of the minimal lineage further up the line of ascent to the founder of the clan’ (Evans-Pritchard, 1940, p. 199). Beyond five generations, lineages may be reckoned to be more closely related to each other if they are co-resident in the same community than lineages to which they may be closely linked by descent, but which are not co-resident. Genealogical fictions are created by which the founders of various ascending branches of a lineage are incorporated into the lineage that has given its name to a local community. In other words, beyond five generations back, consciousness of their face-to-face community becomes more important to the accounting that Nuer give of who they are than their actual descent. Their real ancestors beyond five generations would get in the way and consequently must be forgotten. Actual lineages are effectively tagged on to the mythical ones of the tribal founder and his sons and grandsons who gave their names to the various parts of Nuerland. Evans-Pritchard could detect no deliberate attempt to conceal information, but argued rather that there were strong social pressures to forget the inconvenient truths of ancestry that would confuse and confound the system of cattle entitlements and weaken local community solidarity.

In an essay largely devoted to selective forgetting in biblical texts, Douglas drew on David Backhurst’s (1990) account of theoretical shifts among Soviet psychologists to demonstrate that:

Whenever new knowledge appears, something old will have been rejected. Knowledge does not float in the air; it has practical and social bases. The dissolution of empires entails the collapse of structures of knowledge. When an organization disintegrates, the forms of knowledge that have been called forth by the effort to organise disintegrate too.

(Douglas, 1995, p. 16)

Backhurst tells how Vygotsky’s insistence on the socially mediated character of consciousness and the priority he afforded to the semiotic side of the culture-nature dichotomy led to his fall from favour among the Bolsheviks of Russian psychology in 1931. Under the influence of Marxist ideology, even his own supporters interpreted his carefully balanced arguments as ‘overemphasizing’ the idealist elements over the material conditions. They sought to rescue his reputation by explicitly reversing a perceived emphasis that was actually of their own construction, with the result that even his devoted followers were led to ‘forget’ what he had actually taught. Perhaps ironically, around the same time as Vygotsky was being revised, the Soviet linguist Voloshinov advanced a position very close to Vygotsky’s own, but was able to defend it by drawing an analogy between the relationship between consciousness and the brain as

corresponding to the meaning of a text and the physical form of its inscription, such as the written page. While Vygotsky's ideas about the complexity of the culture and nature relationship constituted uncomfortable knowledge in Stalinist Russia, Voloshinov was able to make almost identical ideas palatable by presenting them in terms that did not present a threat to the Marxist insistence on the primacy of the material over the ideal.<sup>1</sup>

In research carried out in the 1970s, I drew upon the anthropological insights of Evans-Pritchard and Douglas to explore the credibility of millenarian claims by small ultra-leftist organizations, particularly the South-London-based Workers' Institute of Marxism-Leninism Mao Xedong Thought. This tiny group confidently predicted the liberation of the world from capitalism by the Chinese People's Liberation Army by the end of 1977 (Rayner, 1982). The group was the product of a long series of ideological splits, which its members were under strong social pressure to ignore or forget because to recall them would compromise the Institute's claim to be the direct recipients of the revealed truths of the founding fathers of Marxism. Adherents of the Institute averred that the writings of Marx and Lenin were 'literally true for today'. Collapsing the past into the present enabled them also to collapse the future into the present and maintain a faith in the imminence of their deliverance from capitalism by the rapid intervention of distant forces.

This was a radical departure from previous attempts to understand millenarian movements, which were overwhelmingly cast in the language of relative deprivation (for example, Aberle, 1962, 1966; Burrige, 1971; Worsley, 1970). Seeking to explain why millenarian movements occurred as a response to unequal distribution of endowments seemed to me unsatisfactory for many reasons, not least that it did not account for the selection of specifically millenarian, rather than quietist or instrumental revolutionary responses. Instead, I sought to understand not why people resorted to millenarian belief systems, but how it was possible to sustain the distinctively millenarian components of the imminence of radical change brought by a third party over great distance. The answer seemed to lie in the opportunities afforded by their organizational arrangements for members of the Workers' Institute to compress historical time in much the same way as the Nuer did and, at the same time, also to compress geographical distance (such that the Chinese army would occupy Washington DC from Taiwan overnight). To do this, required members (who were overwhelmingly well-educated and of non-UK origin) to suppress their own knowledge of history and experience of distance.

We should note straight away that there is no hint in any of these accounts that social pressure to forget or otherwise exclude information is dysfunctional; far from it. In fact, institutionalized forgetfulness of this sort is essential to maintain the organizational arrangements of societies and organizations. It is part of a broader set of informational and perceptual filters that enable individuals and collectives to make sense of what would otherwise be an overwhelming onslaught of sensory stimuli. Without organizational filters we would not have information at all, only noise. Lacking the capacity for editing

sensory and informational input, humankind would be reduced, as Geertz (1973) has observed, to mental basket cases. Sense-making is possible only through processes of exclusion. Storytelling is possible only because of the mass of detail that we leave out. Knowledge is possible only through the systematic 'social construction of ignorance' (Ravetz, 1986; Rayner, 1986), a phrase which draws on Berger and Luckmann's 1966 classic *The social construction of reality*, and which aims to highlight the ways that ignorance is a socially produced and maintained phenomenon, and the ways that knowing and not knowing are interdependent.

What are we to make of 'information' that lies on the boundary between knowing and not knowing? Again, it is instructive to look to the work of Mary Douglas on ideas of dirt and pollution. Dirt, she reminds us, is matter out of place and her classic work on the dietary laws of Leviticus points out that prohibited foods, described as 'unclean', are taxonomic anomalies that straddle classificatory boundaries (Douglas, 1966). According to Leviticus, fish should have fins and scales, land animals should have legs, marine creatures with legs or land animals lacking them (snakes) are both pronounced 'unclean' or dangerous. Knowledge out of place can be viewed as a form of information pollution, lying on the boundaries of what is organizationally knowable and not knowable; we can understand that it may be dangerous. That is dangerous in at least two ways.

The first is that acknowledging potential information by admitting it to the realm of what is 'known' may undermine the organizational principles of a society or organization. This is the case with the Nuer and their 'disappeared' ancestors, memory of which would cause the system of cattle rights to unravel. The second source of danger is that not admitting such information may also have serious deleterious effects on institutions, either directly or by making them prone to criticism from other parts of society that they 'ought' to have known. The famous case of the vulnerability of the O-rings on the space shuttle launch vehicle (Vaughan, 1996) is just one of a multitude of examples in which subsequent inquiries reveal that information that was not available to decision-makers at the time was indeed known in another part of the organization, but could not be 'heard' until it was too late to avoid catastrophe. Potential information that presents either sort of danger to institutions can be described as 'uncomfortable knowledge'.

I use the term 'uncomfortable knowledge' to bridge two separate but related theoretical concepts: 'wicked problems' and 'clumsy solutions'.

Originally identified by Rittel and Webber (1973) in the context of urban planning, wicked problems are often characterized by multiple competing definitions of what the nature of the problem is. Often the information needed to understand a wicked problem depends upon one's idea for solving it. In other words, wicked problems are often defined by the availability of solutions. With wicked problems, there is no stopping rule: we cannot know whether we have a sufficient understanding to stop searching for more information or clearer definitions. Where there is no end to causal chains, every wicked problem can be

considered as a symptom of another problem. Wicked problems are persistent. Indeed, although phrased in the language of ‘problems’, they may not be soluble in any definitive sense, but are rather chronic conditions that can be managed more or less well. Policy-makers frequently respond to persistent wicked problems by declaring ‘war’ on them. Indeed, almost any ‘declaration of war’ that is metaphorical rather than literally military is a reliable sign of a problem’s wickedness. Hence we have had the war on cancer, the war on terror, the war on poverty and now, courtesy of Richard Branson, the ‘Climate War Room’, which, if there were any lingering doubt, would seem to confirm the ‘wicked’ status of climate change. Other applications of the idea of ‘wicked problems’ include water resource management and large computer software systems as well as health system and social security reform.

The second concept, ‘clumsy solutions’, was originally articulated by the American jurist Michael Shapiro (1988), to describe the coexistence of diverse principles of judicial selection in the US, a clumsy solution allows for the coexistence of common, but differently theorized conclusions. Clumsy solutions may emerge from complex processes of both explicit and implicit negotiation. In other words, solutions are clumsy when those implementing them converge on or accept a common course of action for different reasons or on the basis of unshared epistemological or ethical principles (for a series of case studies, see Thompson & Verweij, 2006). Related ideas include ‘constructive ambiguity’, for example around the idea of ‘sustainable development’, and Sunstein’s (1996) concept of ‘incompletely theorized agreements’. All these ideas describe social arrangements which permit different sub-sections of a society or organization to rub along with each other by not questioning each other’s motivations and worldviews too deeply. They are inherently satisficing (Simon, 1972) rather than optimizing approaches, since each of the competing solutions is optimal from the standpoint of the proposer. Clumsy solutions are inherently pluralistic, although they differ from Lindblom’s (1959) ‘muddling through’ in that they are also inclusive, which is not required in muddling through. According to Verweij *et al.* (2006) clumsy solutions require that ‘all the voices are heard and responded to’. Other writers on the topic emphasize that clumsy settlements may be left implicit and, at least in some cases, may depend on being so (Rayner, 2006). Clumsy solutions, then, may be at least one way to manage wicked problems.

An example of clumsy or incompletely theorized arrangements is the implicit consensus on US nuclear energy policy that emerged in the 1980s and persisted for the best part of three decades. Despite the complete absence of any Act of Congress or Presidential Order, it was implicitly accepted by government, industry and environmental NGOs that the US would continue to support nuclear R&D while operating an informal moratorium on the addition of new nuclear generating capacity. All of the parties agreed to this, but for various reasons, all had a stake in not acknowledging the existence of a settlement.



In the context of wicked problems and clumsy solutions, uncomfortable knowledge is disruptive knowledge. It may be information or understanding that is available to certain parties, but cannot be acknowledged by others. In the case of clumsy or incompletely theorized agreements, the source of the uncomfortable status of the knowledge may derive from its potential to reveal substantive epistemological disagreements about 'facts' or about organizational or ethical principles (values), but, as in the case of the US nuclear power policy, it may also derive from the potential revelation that parties who appear to have reached agreement, or at least accommodation, actually remain divided. In other words, there is always the potential for competing interpretations to be made explicit and cause delicate institutional arrangements to fracture.

### **Organizational strategies to manage uncomfortable knowledge**

In the remainder of this paper I explore four implicit strategies that organizations employ to manage uncomfortable knowledge. I should be clear at the outset that I am using the term 'strategy' very broadly here to include not only deliberate attempts to manage information, but also implicit or even completely unconscious strategies. In the latter case, the use of the term strategy might even be considered to be verging on the metaphorical in the sense that biologists talk of genes or species as adopting strategies. Indeed, it is the second kind of strategies that I find most intriguing.

Drawing on three decades of practical engagement in science for policy processes in Britain, the European Union and the USA, I identify four tacit information management strategies, examples of which I describe in increasing order of sophistication and degree of engagement with uncomfortable knowledge. The strategies are denial, dismissal, diversion (or decoy) and displacement. Denial represents a refusal to acknowledge or engage with information. Dismissal acknowledges the existence of information, and may involve some minimal engagement up to the point of rebutting it as erroneous or irrelevant. Diversion involves the creation of an activity that distracts attention away from an uncomfortable issue. Finally, displacement occurs when an organization engages with an issue, but substitutes management of a representation of a problem (such as a computer model) for management of the represented object or activity.<sup>2</sup>

#### *Denial*

Most of us are familiar with the psychological concept of denial. It is widely recognized as the first phase in the grieving cycle of denial, anger, bargaining, depression and acceptance, but it can also exist as a persistent refusal to act on information or even accept its existence. In his treatise on the psychology of military incompetence Norman Dixon (1976) offers the example of General

Percival who refused to prepare Singapore's northern defences against the approaching Japanese on the grounds that to do so 'might damage morale' by implying that defeat was possible. He remained unmoved by his despairing Chief Engineer Brigadier Simson's observation that being over-run might be more damaging to morale than preparing a defence in time and he completely ignored a direct instruction from Churchill to prepare the northern defences. Had he been able to accept that he was wrong in December 1941, Singapore might have resisted long enough to give time for reinforcements from Australia to arrive. In the event, with minimal resistance, the island surrendered to the Imperial Japanese Army on 15 February 1942. Dixon suggests that Percival's failure was really transferred anxiety about his own morale. To have erected defences would have meant admitting the danger in which he stood by virtue of having grossly misjudged the enemy and having taken the wrong steps to that point. Therefore he was impervious to unpalatable information and persisted doggedly in his chosen course until it was too late.

In a more sociological sense, denial does not refer to the cognitive or affective state of individuals, but to the refusal or inability of organizations at any level to acknowledge information, even when external bodies or even individuals within the organization seek actively to bring it to the collective attention. Again, it is Mary Douglas (1986) who draws our attention to the role of cognition in forming social bonds and conferring identity within institutions. The force of belonging to a 'thought community', one that helps us make sense of the world around us, is one to be reckoned with. It helps to explain why organizations often refuse to learn.

Both aspects of denial seem to play a role in understanding the unresponsiveness to social science research findings on the part of the UK Government Office of Science and Technology (OST), led by the Minister for Science and Innovation who, throughout the relevant period, was Lord Sainsbury. Following the 2000 publication of the landmark House of Lords select committee report that described a crisis of public confidence in UK science (arising in large part out of the government's handling of the BSE crisis) the Department of Trade and Industry pressed the Economic and Social Research Council to commission a 'Science and Society' research programme. The principal questions that the programme was to answer were, first, how to respond to the perceived crisis of confidence in science and technology in the UK and, second, how to address concerns about a future skills deficit by encouraging young people to take up science in secondary education. It was clear that the minister had decided at an early stage that these questions defined the policy problem. The ESRC Science in Society Programme was established in late 2001. The programme was to run for five years with a total budget of about £5.5 million. Over the life of the programme it supported around forty-five projects in various universities around the UK.

Within the first two years, research projects on public perception of science and scientists revealed that, far from there being a crisis of confidence in science and technology in the UK, scientists received very high levels of public

support with 70–80 per cent agreeing with positive statements about science and 85 per cent supporting the idea that '[s]cientists and engineers make a valuable contribution to society'. This was in marked contrast to confidence in civil servants and government ministers who came midway and bottom respectively in the public's ranking of trustworthy professions.

The same research revealed specific concerns that scientists may sometimes allow their enthusiasm for an innovation in science or technology to attenuate their sensitivity to potential risks, but overall, far from being the 'anti-science culture' that the government feared, science emerged as one of the most trusted institutions in the country, even against a backdrop of declining deference to experts and professions in all walks of life. Thus, the ESRC Science in Society Programme suggested that there might not be a crisis of confidence in science so much as a loss of confidence in governance.

Research on scientific labour markets was also revealing in that it suggested that there is a highly developed international market for scientific skills and that scientists will readily migrate to where they see jobs and research funding to be available. Britain already benefited from the in-migration of scientists from Eastern and Central Europe, many of whom were over-qualified for the positions that they held in the UK. Hence, blanket concerns about Britain being under-supplied with the necessary skills to participate in the future knowledge economy were shown to be unfounded.

These results seemed like good news for the government. It was therefore perplexing to discover the programme's apparent inability to communicate them effectively. Of course, the programme's managers realized that academic publication was not going to be an effective way to influence policy, so various means were tried. The results were presented to the communications officers of Research Councils UK (RCUK), the body which coordinates the work of the various UK research funding agencies, where they were welcomed as confirming their own experience. However, several of the communications officers also reported difficulty in getting the upper management of their own research councils to recognize that the government's fears were not borne out by scientific research. They attributed the persistence of ideas of an anti-science culture and concerns about a future skills shortage to the need of research council management to respond to ministerial concerns and the minister evidently remained firmly convinced that the problems of public confidence and scientific skills training were real and persistent.

The programme held workshops with civil servants who had direct access to the minister and collaborated with the same civil servants to help them organize activities. It was clear that, as individuals, the civil servants understood the outcomes of the research and appreciated their significance; however, they were unable to act effectively on the information because it did not reflect the position being taken by the minister who remained wedded to the original framing of the twin problems of anti-science culture and skills deficit. The ministry's institutional resistance to problem reframing was reflected in the 2007 Sainsbury Report entitled *Race to the top*, which was

published just before the Science in Society programme final event, entitled 'Innovation Culture or Anti-Science Britain', held in the Queen Elizabeth II Conference Centre just opposite Parliament and adjacent to the OST office. It was observed by an historian of science policy at the final programme event that the Sainsbury Report could have been written at almost any time in the preceding two decades and that it reflected an enduring policy mythology that had proven exceedingly resistant to contrary evidence. It is, of course, ironic that this state of affairs should have persisted throughout the office of a government that repeatedly made much of its commitment to 'evidence-based' policy.

The ESRC's formal evaluation of the programme appeared to reflect this enduring mythology also. Despite the reviewer's explicit praise of the programme's research dissemination activities, the research council's evaluation staff reached the conclusion that the programme had not met expectations of policy relevance, evidently not because it had failed to address the two central policy questions, but because it had come up with the 'wrong' answers: uncomfortable knowledge that the government was unable to hear because of ministerial pre-commitment to the problem definition and its pre-defined solution.

The knowledge utilization literature tells us that much of the information gathered specifically in response to policy-makers' requests is not considered in the making of decisions for which it was requested. Furthermore, decision-makers will often discount much of the information that is generated, especially when there is a surfeit of contending information (Feldman & March, 1981, p. 174). However, this was not a case in which the minister was exposed to contradictory research results (in which case a sceptical attitude to all of the information would be justified) but one in which research contradicted preconceptions. The situation looks more like a case of what psychologists call confirmation bias where information is accepted if it confirms existing beliefs but filtered out if it is in conflict with them (Nickerson, 1998). Embracing the findings of the Science in Society programme would have been disruptive to established practice in the department as it would have required it to abandon its established trajectory of educational and promotional activity designed to redress scientific knowledge deficits among the public in general and school leavers in particular.

### *Dismissal*

Dismissal is distinguished from denial in that denial offers no recognition that uncomfortable knowledge is available. Dismissal implies at least some level of explicit engagement with uncomfortable knowledge, even if it is only to justify rejecting it. Many reasons can be offered for rejecting uncomfortable knowledge. They include the claims that it is unreliable, not relevant, imprecise, not timely or on the wrong spatial scale.

So-called climate change ‘deniers’ would be considered ‘dismissivists’ under this typology. It seems odd that climate science has been held to a ‘platinum standard’ of precision and reliability that goes well beyond anything that is normally required to make significant decisions in either the public or private sectors. Governments have recently gone to war based on much lower-quality intelligence than that which science offers us about climate change. Similarly firms embark on product launches and mergers on the bases of much lower-quality information. My suspicion is that the origins of this demand dates from early US Congressional Hearings on climate in which scientists acted as issue advocates, essentially offering a single solution (a global agreement on emissions reductions) along with their diagnosis of the problem. This left policy-makers very little room to exercise the discretion they see as their due. Essentially they had to accept the policy presented to them or argue that the policy was unneeded, that is to say to dispute the science. Thus, the policy debate over essentially one set of options (carbon trading) has long been conducted by means of a surrogate dispute over the quality of the science. We have found ourselves in a situation in which scientists acted as poor policy-makers and policy-makers returned the compliment by becoming poor scientists.

My example of dismissal focuses on a very particular aspect of scientific climate information, which is the availability in the late 1990s of seasonal climate forecasts consisting of predictions of deviation from the statistical average of precipitation and temperature over multi-state regions of the USA. The National Oceanic and Atmospheric Administration (NOAA) which provided these forecasts was surprised to find that they were not being taken up as expected by the agricultural and water resource sectors, where NOAA assumed that the opportunities for increased efficiency would be immediately appealing to decision-makers.

NOAA funded a team of social scientists to investigate the apparent lack of interest in seasonal forecasts in the water sector (Rayner et al. 2005). The researchers discovered that water managers are extremely conservative in their operational decision-making. They work in an infrastructure-intensive sector which has evolved precisely to routinize the irregularity of supply due to weather. Their measure of their own success is invisibility. When asked how they knew they were doing a good job they would answer ‘When the (State) Governor is not on the phone to my boss’ or ‘When the customers are not storming the building’ (Rayner et al. 2005, p. 211). Their worst fears are first that customers turn on the tap and nothing comes out, second that customers turn on the tap and something dirty or smelly comes out and third, that customers turn on the tap and something expensive comes out. The first two of these are of course very likely to call immediate and unwelcome public attention to the water manager. Hence, water managers tend to prefer redundancy to efficiency and therefore have little interest in what they see as a high-risk strategy of trying to run supply systems using more efficient, but less forgiving, margins based on seasonal climate predictions. Hence the water

managers explicitly rejected use of seasonal climate predictions on the basis that they were ‘unreliable’ even though they also admitted that they could not say how reliable or unreliable the models actually were.

Another factor here is that the producers and users of the forecasts seem to have criteria for judging the successful use of information that are diametrically opposed. Producers want to demonstrate relevance to funders by showing that their products are being used, while users want to make robust decisions, which actually means reducing their reliance on marginally skilful forecasts.

In some other sectors, particularly construction and agriculture, it appears that there are also strong disincentives to acknowledge seasonal forecast information. In Maryland, farmers are constrained by regulation from spreading nutrients on their fields when rain is forecast in order to minimize run-off into water courses that would pollute the Chesapeake Bay. Construction crews in California are required to implement erosion-prevention measures when rain is forecast. In both cases, additional information about likely rain places constraints on their freedom to organize activities to suit their own schedules and therefore provides an incentive to reject the forecast information, even though they know that it is available to them. It is important to note that, as with denial, dismissal may have a rational basis in how decisions are actually made, which is under conditions of what Simon (1972) referred to as ‘bounded rationality’.

### *Diversion*

Diversion is the organizational strategy of establishing a decoy activity that distracts attention from a subject or problem, thus ensuring that knowledge about it is not created or shared.

An example of such distraction can be found by comparing the published intentions with the outputs of the Science and Society programmes conducted under the Framework 6 Research Programme of the Directorate General for Research of the European Union (DG Research). The importance of understanding existing scientific advisory structures was repeatedly highlighted in successive Science in Society Work Programmes. For example, the 2003 Work Programme stated that ‘The focus will be on assessing the functioning of policy making processes in Europe and major industrialised countries worldwide’ (European Commission 2003, p. 5). The call for proposals in the 2004 Work Programme promised that:

- (i) The focus will be on major organizations playing a significant role in the provision of scientific advice for policymaking, particularly for encouraging new evidence-based approaches, improving how the scientific community reaches consensus on its opinions in the scientific peer-review process. . .

- (ii) Actions for promoting the integration of scientific information and advice in decision-making processes, including communicating it in timely, useful and understandable terms for policy makers, particularly democratically elected representatives (European Commission 2004, p. 6).

The 2005 and 2006 Work Programmes both promised that ‘interrelations between the different actors involved in risk systems, and more specifically those between the scientific community and the policy and decision makers will be identified’ (European Commission 2006, p. 6).

However, throughout this period, the Science in Society programme managed not to fund a single activity designed to scrutinize either how science policy is shaped or how science is used in policy-making at any level within Europe. The activities actually supported by the programme consisted of a small number of localized public participation experiments and the establishment of databases designed to support public engagement exercises, without ever attempting to characterize the systems, cultures and legal frameworks of technical advice with which publics are imagined to engage.

There were various efforts made to draw this disconnect between published intention and delivery to the attention of Science and Society programme managers through the mid-term assessment panel appointed by DG Research for the FP6 Science in Society programme and its programme advisory board for the FP7 programme. Both groups expressed concerns that the programme was not meeting its own stated goals with respect to exploring and facilitating scientific advisory processes within the European Community.

The scope of projects funded under the heading of Scientific Advice and Governance Activities seems to be narrower than the objectives set by the Action Plan and subsequent Work Plans. Indeed, the strong focus on public participation mechanisms in the FP6 Science in Society portfolio may actually have the unintended consequence of distracting attention from a serious and challenging examination of everyday role of scientific advice in the EC and its member states. More widely, the difficulty to engage policy makers in these activities echoes the experience of other strands of the Science and Society Programme, which seems to indicate the possible relevance of systemic constraints on this effort.

(Papon *et al.*, 2007, p. 32)

In other words, a sceptical observer could be forgiven for wondering whether the overwhelming focus on public participation mechanisms in the FP6 Science in Society portfolio was deliberately or opportunistically being used to distract attention from a serious and challenging examination of the status quo with regard to the role of science and scientific advice in governance that DG Research had identified in its own calls for proposals and work plans.

The response of DG Research personnel was that the Science in Society programme was ‘not a research programme, but an action programme’.

The advisory groups, however, had difficulty reconciling this position with the language in the programme outlines and requests for proposals. The issue was never satisfactorily resolved and in the case of the FP7 Advisory Group was one of the factors that led to DG Research standing the advisory board down only halfway into the programme.

It appeared that in planning its Science in Society Programmes, DG Research had quite accurately identified understanding of science and technology advisory processes at various levels of governance and in different member states as an important knowledge lacuna for both academic study and practical policy-making. Given the interest of the Commission in promoting public engagement in European governance through engagement with science and technology (S&T) decision-making, it would, of course, be vital to make those processes visible to potential participants. However, such an exploration would also require scrutiny of DG Research itself and might also create tension between the Commission and member states if examination of their S&T advisory processes raised controversial issues. Since the underlying motive of the Framework Programmes is to promote European integration through science and technology co-operation, it is clear that examination of S&T advisory processes in Europe carries a high potential for the generation of uncomfortable knowledge, hence the diversion of attention into public engagement experiments with very limited scope and the production of relatively uncontroversial databases might seem a more attractive option for DG Research than following through on its published programme, however logical that programme had appeared at the time.

### *Displacement*

Displacement is the term that I use to describe the process by which an object or activity, such as a computer model, designed to inform management of a real-world phenomenon actually becomes the object of management. Displacement is more subtle than diversion in that it does not merely distract attention away from an area that might otherwise generate uncomfortable knowledge by pointing in another direction, which is the mechanism of distraction, but substitutes a more manageable surrogate. The inspiration for recognizing displacement can be traced to A. N. Whitehead's fallacy of misplaced concreteness, 'the accidental error of mistaking the abstract for the concrete' (Whitehead, 1926, p. 51).

The Chesapeake Bay Program (CBP) was established with an initial \$5million annual budget in 1983 to bring together the States of Maryland, Virginia and Pennsylvania with the District of Columbia, the US Environmental Protection Agency and the Chesapeake Bay Commission to address the adverse effects of excessive nutrient (phosphates and nitrates) loading in the bay due to diverse factors including industrial activity, agricultural run-off and increasing encroachment of human settlement close to the water's edge. The President's 2011 budget earmarked \$63 million for the CBP.



In 1992, the CBP initiated a major ecosystem modelling programme in pursuit of its goal of 'cleaning up' the Bay to restore ecosystems functions that have been compromised by nutrient pollution. The programme adopted the specific goal to reduce controllable loads of nitrogen and phosphorous delivered to the Bay by 40 per cent of 1985 levels by the year 2000. The modelling activity consists of a distributed suite of linked models designed to allow scientists to predict positive or negative changes within the ecosystem due to management actions such as improved sewage treatment, controlling urban sprawl and reduced fertilizer or manure application on agricultural lands.

The CBP's modelling activity consists of several components beginning with a watershed model, which divides the 64,000-square-mile Chesapeake Bay watershed into ninety-four model segments, each of which contains information generated by a hydrologic sub-model, a non-point source sub-model and a river sub-model. In turn this is linked to an estuary model, commonly referred to as the water quality model, which examines the effects of the loads generated by the watershed model on bay water quality. The estuary model is built on two sub-models: the hydrodynamic sub-model, which simulates mixing of river and seawater, and the water quality sub-model, which computes the chemical and physical dynamics of the Chesapeake. The final component of the suite of models is an airshed model, which simulates nitrogen emissions from all sources in the airshed from Texas and North Dakota eastwards to Maine and Florida. The airborne nutrient loads are transported by the airshed model and linked to the watershed model through deposition on land surfaces and to the estuary model through deposition on the water surfaces of the tidal bay.

These models make several heroic assumptions. For example, changes in nutrient deposition on land result in instantaneous changes in aquatic loading, when it is known that these nutrients actually take between two and seventy years to leach into the bay. Another is that the hydrology of the Bay can be adequately represented by a seven-year average of the average hydrology, whereas it is widely understood that nutrient loading is heavily dependent on extreme weather events, such as heavy rainfall that scours nutrients that have settled in river sediments and washes them into the Bay.

Using these models to simulate the results of policy interventions, the CBP claims that it has achieved a steady improvement in the water quality of the main stem of the Bay. However, interviews conducted in 1999 with programme employees who conduct actual sampling and testing of the Bay's waters were revealing. First it was clear that there was resentment at the extent to which resources and professional recognition flowed to the modellers while field measurement was under-resourced and received little attention. Second, the personnel taking and analysing field samples reported that there was no discernible trend in the water quality of the main stem of the Bay. It became fairly clear that, for the CBP, the model had displaced the actual Bay as the object of management. Real water quality monitoring data that seemed to contradict the record of improvement constituted uncomfortable knowledge which was displaced by the more gratifying results from the modelling.

There can be no denying that adequate monitoring of such an extensive, diverse and complex ecosystem is, to say the least, challenging. However, the reification of models within the CBP cannot be attributed to scientific difficulty alone. As the budget figures mentioned above indicate, the CBP has expanded into a major regional enterprise and a showcase programme for the Federal Environmental Protection Agency. To ensure continuing support from Congress, the programme needs to tread a fine line between making the case that the threat to the Bay remains serious and at the same time demonstrating the programme's effectiveness in addressing the problem. Congress would be unlikely to vote for continuing, let alone expanding the CBP's budget if the best it can say for its efforts is that there has been no discernible change in water quality. Hence, the monitoring data constituted very uncomfortable knowledge indeed.

The CBP's own website suggests that, despite cautionary language about the models being 'used properly', the programme still continues to conflate the model with the actual bay. In describing the operation of regulatory nutrient caps on each of the Bay's nine major tributaries, the programme says that '[b]ay models are used to track nutrient loads to ensure the cap is not exceeded' (CBP, 2010). In other words, the operation of the cap is governed by the model rather than by direct monitoring of nutrients.

## Conclusion

Notwithstanding the contributions of scholars such as Evans-Pritchard and Douglas, the study of the social construction of ignorance remains in its infancy. Identifying and differentiating various organizational strategies for excluding uncomfortable knowledge is only a simple exercise. I have discussed four strategies that I have encountered personally in a variety of science and policy settings. I am sure that there are others that can be just as easily described. Uncomfortable knowledge and the strategies for managing it are, of course, not confined to the realms of science and social science. But focusing on these fields of contemporary concern ought to dispel any doubt at all that the phenomenon is confined to primitive cultures such as the Nuer or pre-enlightenment societies such as the ancient Israelites or even to modern marginal groups such as the Workers' Institute of Marxism Leninism Mao Xedong Thought.

I have also argued that the social construction of ignorance is not only inevitable, but actually necessary for organizations, even entire societies to function at all. But how do we deal with cases where we encounter dysfunctional cases of uncomfortable knowledge that may endanger institutions to which we are committed (such as social or religious organizations) or upon which we rely for safety or other services such as education (government regulatory agencies or universities for example) or those we rely on to provide us with incomes and wealth (as employees or stock holders)? How can

uncomfortable knowledge be insinuated into the decision processes of organizations and institutions that have evolved on the basis of being able to remain deaf or blind to it?

This question brings me back to my initial claim that the term ‘uncomfortable knowledge’ is a bridge between wicked problems and clumsy solutions. Clumsy solutions come about when multiple, diverse, perhaps incompatible, perspectives are brought to the bear on an issue, resulting in a settlement that is inelegant from any single perspective, but robust because it relies on more than one epistemological and ethical foundation. The way to cope with (I will not say solve) the challenge of uncomfortable knowledge is to ensure that there is a minimum level of diversity in decision-making processes. This recommendation leads inevitably to a discourse about forms of pluralism, power and governance which, as the European Commission’s Science in Society Programme demonstrates, cannot be resolved merely through the turn to public participation.

## Notes

- 1 The better-known story of Lysenkoism in biology is another case in which Soviet political orthodoxy locked out scientific knowledge that was seen as incompatible with its political ideals (Medvedev, 1969).
- 2 Although it is tempting to speculate on how each of these might map onto the four organizational strategies of Douglas’s cultural theory, I will not attempt to do so in this paper, but leave the matter open for further empirical investigation.

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