



A WORKSHOP FOR THE
ESRC SCIENCE IN SOCIETY
PROGRAMME

SCIENTIFIC CONNOISSEURS AND OTHER INTERMEDIARIES: MAVENS, PUNDITS & CRITICS

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HAS SCIENTIFIC CONNOISSEURSHIP BEEN EXCLUDED BY SCIENTIFIC INSTITUTIONS, OR BY THE NATURE OF SCIENCE ITSELF?

EXECUTIVE SUMMARY



Is it only in relation to science that society does not recognise that non-practitioners have a role to play in discriminating between good and poor performance – in acting as connoisseurs? Has scientific connoisseurship been excluded by scientific institutions, or by the nature of science itself? Would we benefit from public intellectuals able to apply the skills and experience of connoisseurs to look critically at scientific uncertainty and public anxiety about new technology, to articulate the issues in public debate, and to advise on policy? Would such intermediaries perform a social service by relieving the citizen from the burden of public engagement with science and technology (S&T)? If so, how could such roles be fostered, and what kind of knowledge and experience would scientific connoisseurs need? What, in particular, can the social sciences contribute?

Steve Rayner posed these questions at the start of this workshop held at Minster Lovell Mill in Oxfordshire in March 2004. The occasion brought together 18 participants with a wide range of training in the natural and social sciences. They worked as scholars of scientific knowledge, science education, history and philosophy of science, and scientific and technological regulation; and as practitioners with an interest in science and society issues from government, Parliament and the private sector.

The workshop reached no final conclusions, but the following key points emerged from discussion.

■ While there is evidence of 'participation fatigue' among stakeholders, who have other contexts in which to pursue their interests, there is no evidence of this among citizens. Scientific connoisseurship needs to find roles which support and articulate wider public engagement.

■ Although public, intellectual, non-practitioner, connoisseurs are important (and there were some examples among the workshop participants), developing popular connoisseurship is a key objective. It is important to establish that preparation for scientific citizenship should have a higher priority in secondary science education. Current UK curricular innovation is a key first step, but it is important to recognise that education for scientific practice and education for scientific citizenship are separate requirements that compete in schools not only for time, but in relation to the values and skills they are trying to instil.

■ Social science offers 'a large portfolio of attractive options' for identifying and implementing change, according to one practitioner at the workshop. Its contributions include paying new attention in the sociology of science to the role of experience in relation to which forms of knowledge qualify people to undertake which roles and make which judgements.

■ Although scientific connoisseurship could be exercised by individuals, it is best thought of as a process rather than a role, and is often best performed by institutions. A number of institutions are already acting as effective intermediaries between science and the public and science and government. But participants thought that more could be done to assess what is going on in scientific governance, so as to ensure that more self-critical and self-aware principles on the construction and use of science in decision-making are carried through consistently in practice. Doing this poses challenges, given the scope of science and technology governance.

■ There are also opportunities for new forms of mediation between science and its wider publics. Building on the workshop discussion, this report proposes four experimental mediation services: ■ Extending the kind of 'constructive deconstructive analysis' that social scientists conduct on regulatory issues (eg in food research) into a range of domains and institutional settings (eg the Food Standards Agency) where it might demonstrate benefits.

■ Introducing 'socio-technical vigilance' involving systematic evidence from the public on intended and unintended consequences of socio-technical change, initially by tapping into and critically assessing evidence from science shops.

■ Exploring the potential across the research councils of 'open door' type research schemes piloted – but not developed – by the ESRC, allowing requests for knowledge from civil society institutions to be pursued and developed into potential research proposals.

■ Reviewing the possibilities of building bottom-up research agendas based on local and community-based needs. This would include evaluating the applicability of 'contestable democratic design criteria' to scenario workshops – a participatory technology assessment methodology – in the definition of community research agendas.

■ Because the social roles which individual and institutional scientific connoisseurs could play are important – framing public debate, adjudicating knowledge claims crucial to regulation, contributing to the education of critical consumers and citizens – it is essential that they themselves are accountable in their methods, approaches and values. In this way scientific connoisseurs would be self-exemplifying – opening up issues for public scrutiny and debate not only in what they do, but in the way they perform their roles.

1.0

INTRODUCTION

SECTION 1

CAN WE CLARIFY OUR THINKING ABOUT THE ROLES PLAYED BY DIFFERENT FORMS OF EXPERTISE AND EXPERIENCE IN QUALIFYING PEOPLE TO TAKE DIFFERENT KINDS OF DECISION, GIVEN THAT THE QUESTION OF WHO IS QUALIFIED TO DECIDE WHAT – SO THAT JUDGEMENTS ARE TECHNICALLY AND SOCIALLY AND POLITICALLY ACCOUNTABLE – IS CENTRAL TO THE ISSUE?

1. The great art auction houses, though primarily commercial organisations, themselves become institutional connoisseurs through their dependence upon and employment of connoisseurs in a range of specialist areas. They occupy a similar role in (e)valuation of artefacts, and indeed more recently also in public education, as the art museums which have traditionally performed these roles.

2. Although Erik Millstone reminded us that artists could also be art dealers, Rembrandt being an example. See also para 2.3.

3. Science and Society, Third Report of the House of Lords Select Committee on Science and Technology, Session 1999–2000, 23 February 2000.



1.1

Connoisseurship fulfils a number of important roles in mediating between various kinds of expertise or expert practice and the wider publics. In relation to a wide range of arts, crafts and artefacts, food and drink, connoisseurs employ judgement in determining what is good and bad in their chosen domain, who produced it and what intrinsic and extrinsic value it should enjoy. Through this activity and through their informed commentary on and involvement in public debate, they contribute to the maintenance of particular artistic or craft skills and standards, to the development of markets for artistic products, and, more broadly, to the social reproduction of cultural tastes, techniques, and the wider values which frame these. Connoisseurs are primary agents in the linkage of arts, crafts and wider society, and increasingly act through the mass media of television, newspapers and magazines. Yet connoisseurs can wield this cultural and market power, and often profit from it through trading¹, without being themselves practitioners of the art or craft whose standards they set².



1.2

The relationship between science and its wider society, as the House of Lords Select Committee pointed out, appears to be facing a paradoxical crisis. Just as the flow of potential benefits from science and technology seems to be increasing at an unprecedented rate, there is a crisis of public trust in scientific governance and science based industry. Who better to turn to at such a time than the recognised intermediary between science and society, the seasoned judge of value, worth and interest, the scientific connoisseur? Scientific connoisseurs, it might be thought, could contribute to the standards of public scientific literacy and professional scientific practice. Their contributions to the public debates of the day could tease out issues in ethics, in methodology, and in interests and approaches. Their judgement could contribute to setting priorities or framing regulation. Yet it seems that this role, despite functioning apparently unchallenged in so many other domains of human activity, cannot be applied to science. Not only does the scientific connoisseur seem not to exist, but something of a reverse social takeover frequently seems to occur when leading scientists are asked to comment on the social impact of science and technology, thus extending their authority beyond their expertise.



1.3

This apparent gap between the need for and provision of intermediaries between science and its publics was the territory which this workshop set out to explore. Its central task was to discuss the roles new forms of mediation might play in improving the quality and focus of:

- The wider debate about the social uses of science and technology.
- The framing and use of advice in scientific governance.

Did the lack of scientific connoisseurs owe something to the nature of science or the functioning of scientific institutions? Can we clarify our thinking about the roles played by different forms of expertise and experience in qualifying people to take different kinds of decision, given that the question of who is qualified to decide what – so that judgements are technically and socially and politically accountable – is central to the issue? If a new form of intermediary is required, what is its nature, and what kinds of environments and policies in science and education might nurture it?



1.4

In approaching these questions we looked not only at connoisseurs, but more briefly at other intermediaries, mavens, pundits and critics. We now turn to the history and usage of these terms.

4. In the Boston Museum of Fine Arts website, On Connoisseurship, now available at www.highlands.com/art/dekoster.htm

5. The recent National Gallery exhibition on the relationship between Bruegel and Bosch offers an interpretation of the drawing from the artist's perspective: that the artist can tolerate the connoisseur's naïve judgements as long as he is paid at the end of the process.

6. Sharon Flescher (2000) executive director of the International Foundation for Art Research, quoted in the Trinity College (US) online newsletter account of its art symposium Connoisseurship and Authenticity.

7. I am grateful to Erik Millstone for this suggestion.

8. Quoted in the Web Gallery of Art, as 1897; possibly from The Central Italian Painters (1897).

9. Polanyi, M, Personal knowledge: towards a post-critical philosophy (London Routledge and Kegan Paul, Chicago IL University of Chicago Press, 1958).

10. Berenson had a famous advisory role and friendship with Lord Duveen, the main influence on the development of the British art collection over a long period.

11. The British Edition is Malcolm Gladwell, The Tipping Point: How Little Things Can Make a Big Difference (London Little, Brown and Company 2000).

12. Jerry Ravetz, in a written contribution to the workshop. This is Western usage only. In its original Indian usage, the 'pundit' or 'pandit' is a wise or knowledgeable man (invariably male) who can use this title as an adornment to a position in national life (eg 'Pandit Nehru') or someone who, at a local level, traditionally accompanied most major events and ceremonies.

13. A cross-cutting element of the European Commission's sixth framework programme.



2.0

FORMS OF MEDIATION EXPERTS AND WIDER PUBLICS

SECTION 2

THE CONNOISSEUR 2.1

In a review of connoisseurship in art in 1994, Richard de Koster, private New York art dealer, tells us: "Over the centuries, the individual who judged the relative merit of works by artists has been known as 'philitakos', 'aestimator', 'cognoscere', 'conoscitore' or 'connoisseur'. Two thousand years ago the King of Bithynia offered to redeem the entire national debt of Cnidia in return for the statue 'Venus of Cnidus' by Praxiteles. The excellence of this connoisseur's judgement is still obvious."⁴ In this reading of connoisseurship the connoisseur is a non-practitioner expert in the evaluation of a work of art.

2.2

The origins of the usage of the term connoisseurship lie the visual arts, as exemplified by the Pieter Bruegel piece, The Painter and the Connoisseur, c1565, the cover picture of this report. Again, the separation of the roles of artist and connoisseur is explicit. Bruegel's connoisseur is fascinated with the art on offer but keeps a tight hold on his purse.⁵

2.3

The first mention of the term connoisseur in English is believed to be in Jonathan Richardson's (1719) Essay on the Whole Art of Criticism as it Relates to Painting and an Argument in Behalf of the Science of the Connoisseur. Richardson himself was an exception to the usual rule of the division of roles, making a commercial success of both art criticism and art practice.

2.4

The extension of the use of the term to scientific connoisseurship is usually attributed to Giovanni Morelli, who worked in the latter part of the nineteenth century. However, this use did not refer to connoisseurship of science, but connoisseurship of art in terms which could be imputed to science. In Morelli's terms, scientific connoisseurship meant systematic categorisation of artistic idiosyncrasies of style and content as additional criteria to be judged alongside 'emotional qualities' which, it is implied, could be subjectively assessed. Morelli illustrated this by his classification of the characteristic way that a variety of painters portrayed hands.

2.5

In contrast, connoisseurship later came to be used predominantly almost as a residual category for the subjective and personal in judgement, to refer to that part of the assessor's evaluative toolkit which did not rely on formal techniques of analysis either scholarly or scientific: "Authentication has three parts – connoisseurship, scholarship and the scientific."⁶

A paradigm example of connoisseurship in this more restricted sense of relying on educated taste and judgement may be Bernard Berenson,⁷ the American expert on Renaissance Italian painting, who almost made a virtue of subjectivity in a variety of eminently quotable statements about his work: "I am only a picture taster, the way others are wine- or tea- tasters."; "Genius is the capacity for productive reaction against one's training."

And most tellingly: "Not what man knows, but what man feels, concerns art. All else is science."⁸

2.6

All the above uses are about the visual arts, and generally assume that the connoisseur is not an art practitioner. But in applying connoisseurship to the arts, Michael Polanyi sees it as the superior and tacit knowledge involved in practice which allows the artist or craftsperson to work successfully beyond the frame of explicit knowledge of their craft, which Polanyi calls 'Rules of Art':

■ "Rules of art can be useful but they do not determine the practice of an art; they are maxims, which can serve as a guide to an art only if they can be integrated into the practical knowledge of that art."⁹

Connoisseurship can be divorced from practice in a specialised critical elite. But for Polanyi it is the most valuable component of practice, the specialised personal knowledge which leads to individual artistic expression through the application of particular (unique?) skills.

2.7

Connoisseurs also employ their knowledge and skill in retrospective evaluation of the past provenance and present value of an artefact. Connoisseurs often give advice on the social and monetary value of artefacts to collectors or individuals who are interested in a particular piece.¹⁰ As we have seen, they can also profit directly as traders in their chosen artefacts. The curatorial role is of particular interest as it combines various aspects of connoisseurship in a role which, because it involves both building and conserving a collection over time, has a future orientation.

2.8

The role of connoisseur is a highly discriminating one and this may have led to the elitist associations of the term, especially in Europe. However, we can recognise many of the characteristics of connoisseurship in a much more popular form, that of the maven.

THE MAVEN 2.9

The term maven is believed to have come into use in American English in the early 1950s. The Webster dictionary definition is 'one who is experienced or knowledgeable' with secondary references to the definitions of 'expert' or 'freak'. Maven, derived from Yiddish and, before that, from Hebrew, is often translated as 'connoisseur' but lacks any of the elitist associations of that word. In typical American usage, the maven is the person in your personal network whom you would consult before you bought a particular kind of product – say your hi-fi separates – and who would have a, perhaps mildly obsessively, detailed knowledge of what to buy and where to go to get it. You would go to a second maven for advice on computers, a third for cars. The maven's knowledge is narrow but deep.

2.10

The idea of the maven has recently been discussed in Malcolm Gladwell's The Tipping Point.¹¹ Gladwell's book, subtitled 'how little things can make a big difference', dissects the point at which social epidemics, as he terms them, take off, and examines the key social roles involved in the process. In his sense, the maven is one who accumulates information on many different aspects of one category of things and likes to

communicate it, actively or in response to requests. The importance of the book for our analysis is that Gladwell demonstrates that quite complex social movements can be built up from the articulation of limited yet specialist roles, like the maven, with others, which he terms connectors and salesmen.

THE CRITIC AND THE PUNDIT 2.11

The term critic can imply a practitioner or non-practitioner based role, but one based in expertise and experience. It is this experience- and expertise-base that distinguishes the critic from the pundit. Limited knowledge may be attributed to the pundit but (s)he is often seen as recycling opinions and doing so with a degree of pedantry.¹²

2.12

The term 'critic' in relation to science practitioners can be extended to include relatively loosely constructed roles which involve scientists themselves adopting a critical perspective on their own practice. One example would be the social responsibility in science movement in Britain, which took root in the 1970s and involved scientists and social scientists in attempts to define issues and possible solutions to science-society issues, often through attempts to extend professional, public and political debate on science and technology in its social and economic context. Similar to the idea of the public as critics are attempts through the current European New and Emerging Science and Technology (NEST) programme¹³ to persuade practising scientists to anticipate problems arising from their own work and bring them into the public sphere.

THE COSY, CONVIVIAL AND CONFIDENTIAL WAYS OF INTERACTION OF SCIENCE WITH POLICY ARE NOW A THING OF THE PAST. WHILE WE STILL MIGHT NOT ENGAGE IN THE BRUISING CONFRONTATIONS IN THE AMERICAN STYLE, WE ARE NOW IN AN ERA OF DEBATE, WITH LITTLE TAKEN FOR GRANTED.

14. Michael Polanyi, 'The Republic of Science: Its Political and Economic Theory' *Minerva*, 1962, 1, 54-74.

15. Polanyi, 'Objectivity in Science – a dangerous illusion', *Scientific Research*, 28 April 1969.

16. Jerry Ravetz, communication for this workshop, March 2004.

17. Ravetz couples science with medicine in making this judgement.

18. Ravetz (n 16).

3.0

THE FUNCTIONS OF SCIENTIFIC CONNOISSEURSHIP

SECTION 3

3.1

As we have seen, connoisseurs and mavens perform a variety of social functions in promoting wider social engagement with the arts, crafts and other domains: attribution/legitimation; (e)valuation/pricing; establishing cultural tastes and techniques, and reproducing these and the wider values which frame them.

What roles might they perform in relation to science and technology?

3.2

For Michael Polanyi there is a direct connection. He sees his analysis of the direct transmission of personal knowledge – of tacit craft skills from artist to artist – as a direct model for the tacit transmission of scientific values and practice:

“...the standards of scientific merit are seen to be transmitted from generation to generation by the affiliation of individuals at a great variety of widely disparate points, in the same way as artistic, moral or legal traditions are transmitted. We may conclude, therefore, that the appreciation of scientific merit too is based on a tradition which succeeding generations accept and develop as their own scientific opinion. This conclusion gains important support from the fact that the methods of scientific inquiry cannot be explicitly formulated and hence can be transmitted only in the same ways as an art, by the affiliation of apprentices to a master. The authority of science is essentially traditional.”¹⁴

3.3

Polanyi himself sees connoisseurship, involving appraisal and judgement, as a counterpoint to “the dangerous illusion of [the] objectivity of science”.¹⁵ The tacit nature of the cultural transmission of scientific skills – and to some extent also of the professional values and priorities which underlie them – of course limits the extent to which formative ideas and approaches can be consciously examined and changed.

3.4

For Polanyi, connoisseurship in science is also part of practice, a fusion of the ‘art of knowing’ with the ‘art of doing’. For the scientists, the emphasis is not only on handing on individual skills from the individual master or school, but on a broader acculturation from a scientific community (and in turn, a contribution to the transmission of values and approaches to the next generation). As Jerry Ravetz pointed out in his written contribution to the workshop, Polanyi once remarked that a scientific style of connoisseurship could only flourish under conditions of privileged, leisured learning. But in fact we can see Polanyi’s view of the role of connoisseurship in science as opening up judgement, not only to the scientists themselves, but to mediators of wider public interest.

3.5

Ravetz takes the American Jewish origins of the term ‘maven’ as a cue for looking at the wider institutional and social context that establishes the need for some forms of systematic mediation between science and its publics:

“Is there something special about American intellectual culture which makes such terms real and relevant there? If so, then we would do well to be aware of it, so as to be able to import and adapt them most effectively... The authority of the elite institutions, while quite real, has a rather narrow base in the whole society. While the public deference shown to a professor at a leading university is genuine, the class of intellectuals as such has not traditionally enjoyed great popular esteem among social strata that are removed from the middle and upper classes of New England. The American intellectuals really do need mavens to elbow their way into the arenas of public debate.”¹⁶

In Britain he sees science, as a meritocratic challenger without roots in an older deferential establishment, as having lost the public’s trust through BSE before it had fully achieved status with an older elite that clung on in public administration: “What these incidents show is that the scientific establishments, like those of the polity and society, can no longer bury their secrets. The deference game is over. Self-regulation of professions,¹⁷ while not eroded in name, is eroded in practice. And the question ‘Why should we start to trust you now?’ is one that cannot be brushed aside.

All this means that when science confronts other spheres of society, it cannot rely on any borrowed prestige or assurance of probity. The cosy, convivial and confidential ways of interaction of science with policy are now a thing of the past. While we still might not engage in the bruising confrontations in the American style, we are now in an era of debate, with little taken for granted.”¹⁸

3.6

In understanding the conduct of that debate, it may not be necessary to distinguish between connoisseurs, mavens, pundits and critics. But it is important to understand the social function being performed by participants in the debate and the skills and claims of experience which form the basis for such contributions.

3.7

Most examples of connoisseurship show the range of uses in 3.1 above. However, the original role and source of legitimacy of connoisseurs varies greatly. For example, we can envisage three kinds of scientific connoisseurship varying according to their relationship with practice:

a) Practitioners who would seek to engage with a wider public in trying to share their assessment of the scope, limits and problems of their science; this could be seen as part of one great tradition of concerned scientists which had peaks in the UK in the 1930s and 1970s.

b) Non-practitioners who would operate more as typical connoisseurs do in the contemporary arts and humanities, as members of a specialised elite whose role would be assessing, evaluating, and advising, sometimes combined with teaching and research, and sometimes participating in debate as a public intellectual; but doing so as a new additional kind of expert whose prime role would be to bring a new meta-expertise to scientific governance, dealing with other experts.

c) Non-practitioners who would use much the same skills, primarily employing them in the articulation of public debate about the social implementation of new technology.

THE GROWING POTENTIAL RELEVANCE OF SOCIAL SCIENCE ANALYSIS TO SCIENTIFIC GOVERNANCE OFTEN SERVES TO HIGHLIGHT THE TENSION BETWEEN THE NATURAL SCIENCE AND SOCIAL SCIENCE COMMUNITIES.

19. Mavens, connectors and salesmen are central roles in the development of social epidemics in Gladwell's *The Tipping Point*.

20. See the useful review of constructive technology assessment by Johan Schot. Interestingly, this begins by seeing CTA as a kind of delayed response to the sociotechnical critiques of the Romantic Movement, but-tressing his argument with a quote from Mumford: "Romanticism as an alternative to the machine is dead... but the forces and ideas once archaically represented by romanticism are a necessary ingredient in the new civilization, and the need today is to translate them into direct social modes of expression, instead of continuing them in the old form of an unconscious or deliberate regression into a past that can be retrieved only in phantasy." Lewis Mumford, *Technics and Civilization*, 1934, quoted in Schot, J 'Constructive Technology Assessment' in *Technology Policy meets the Public*, ed A Jamison, PESTO Papers II, Aalborg University 1998.

21. NEST is one of the cross-cutting research activities of the EU Sixth Framework Programme. See www.cordis.lu/fp6/nest.htm.

4.0

ISSUES OF THE WORKSHOP

SECTION 4

4.1

Arising from this discussion, it was decided that the workshop should focus on four key issues:

- What is the actual and potential contribution of the scientific connoisseur in informed public debate about the role of science and technology in society in general, and to the assessment of the social and economic impact of new technologies in particular? How far is scientific connoisseurship a broad (set of) role(s), of intellectual critic and adviser, based on personal values and experience, and how far a putative social technology? In either case, how does connoisseurship deal with discontinuities in subject or method? Historically, has it led or lagged behind social debate on the issues? What forms of social analysis already embody (at least elements of) scientific connoisseurship in action? What can the experience of the connoisseur in art, and the concept and approach of the scientific connoisseur, add to them?
- What relationship would scientific connoisseurship have to the broader participation of the public in the choice/legitimation of public policy on S&T? Would the scientific connoisseur, as traditionally defined, be able to deal with the potential criticism that judgements that were becoming more transparently based and more widely shared through society were, through connoisseurship, becoming the more opaque

property of a particular elite? Or would it be possible to create a more broadly based social connoisseurship movement, in which the connoisseurs proper might be mavens – discriminating collectors and energetic disseminators of what might be broadly termed 'consumer information' – bolstered by connectors and salesmen?¹⁹

c) What base of knowledge and expertise would scientific connoisseurship draw on/need to build, and what are the contributions of natural scientists and engineers on the one hand, and social scientists on the other? Again, what can we learn from current examples of developed approaches, such as constructive technology assessment,²⁰ or the 'anticipatory research' of the NEST Insight Projects,²¹ or the experimental use of constructivist analysis within the sociology of science to try to characterise risks and opportunities in areas where there is strong scientific uncertainty or competing explanations?

d) What would be the characteristics of a supportive social environment in which experimentation and learning in scientific connoisseurship could take place with a range of social partners, and how could this be developed and financed? Would this take an institutional form, or have more the character of a social network or social epidemic? How would this relate to, or supersede, traditional models of scientific communication?

4.2

In relation to contemporary issues in the governance of science (c) above seemed to have a special value and significance for two principal reasons. First, policy and commercial decision-making in conditions of scientific uncertainty, controversy, or ignorance seem to be increasing in frequency. These newly emphasised features of science seem to present particular difficulties with broader public engagement given that science has often been constructed – not least through the educational curriculum – as being uniquely free of such complexity. They sometimes seem to lead to role uncertainty or conflict amongst scientists involved in advising on science. Any methodology which claims to bear on these issues has a high current policy salience. Secondly, to have such a methodology constitutes a very strong knowledge claim for the social sciences. The growing potential relevance of social science analysis to scientific governance often serves to highlight the tension between the natural science and social science communities. The acknowledged contributions of social science to the articulation of the science and society agenda have not altered basic disciplinary politics. Any strong claims on behalf of the social sciences to mediate between science and its wider publics, or its wider social uses, must expect especially strong scrutiny.

4.3

For these reasons, it was decided that the workshop should discuss in detail claims of a constructivist approach in the sociology of science to contribute to the adjudication of what constitutes 'good' or 'bad' science for particular social purposes. Erik Millstone, who has applied such approaches to issues of food safety, was commissioned to produce a paper on How can scientific connoisseurship be socially useful and intellectually robust?, which can be found in Annex C.

SCIENCE IS AN ABSOLUTELY DOMINANT FORM IN POLITICAL LEGITIMATION; WE NEED TO DISCRIMINATE BETWEEN TECHNICAL QUESTIONS AS SUCH AND THOSE ISSUES THAT ARE WIDER BUT WHICH ARE SHOE-HORNED INTO TECHNICAL QUESTIONS AND ARE THEREFORE REFERRED, INAPPROPRIATELY, TO THE SCIENTIST.

5.0

KEY WORKSHOP CONTRIBUTIONS

SECTION 5

Note: although this summary is organised theme by theme under each theme's designated title in the programme, themes and issues inevitably recur over time under different headings.

In general, and in keeping with the spirit of the workshop, contributions are anonymous. However, in some cases – such as the short presentations which we used to kick off themes – it seemed artificial and even unfair to do this and in these cases the name of the contributor is given.

In order to give a sense of the dynamics of debate, main questions or comments on a topic or presentation are indicated by a 🗣️. Subsequent points responding to or qualifying that question or comment are indented under it, indicated by a 🗣️.

Theme 1: Learning about connoisseurship and other intermediary forms

Steve Rayner: Introduction 5.1

The idea of scientific connoisseurship originated from Jerry Ravetz in the 1980s. Science might be unique in that it appears to be only in relation to science that society does not recognise that non-practitioners are able to between discriminate good and bad performance, to become connoisseurs – why? Have scientific institutions somehow excluded this possibility, or is there something intrinsic about science itself which excludes this? Scientific connoisseurship is a key notion in relation to the communication of science, and the increased involvement of science and technology in all forms of decision-taking.

In the UK the term connoisseur appears to carry connotations of elitism. In the US the term maven, originating from Yiddish, for a source of non-practitioner expert knowledge, manages to escape this. Both are examples of a broader category of knowledge intermediaries. Some of the issues are listed below.

🗣️ Is it true that we don't have scientific connoisseurship, or is it in disguise? For example, a social scientist coming to work on the environment may acquire connoisseur status in relation to work in climate change, even in relation to aspects of climate change modelling which are beyond the social scientist's original mathematical training.

🗣️ How is connoisseurship created, how would you go about it?

🗣️ What societal benefits would emerge from a respectable category of knowledge intermediaries?

Harry Collins: Expertise, experience and participation in decision-making 5.2

Though science studies have resolved the problem of legitimacy by showing that the basis of technical decision-making can and should be widened beyond the core of certified experts, they have failed to solve the problem of extension: 'How far should participation in technical decision-making extend?' In other words, science studies have shown that there is more to scientific and technical expertise than is encompassed in the work of formally accredited scientists and technologists, but have not told us how much more. Brian Wynne coined the term 'lay expertise', although not everybody has it – witness the controversy over the combined measles, mumps, rubella vaccine (MMR).

One way in which the group of analysts who practise the sociology of scientific knowledge (SSK) confront the concept of expertise is in the problem they themselves face in trying to gain a cultural foothold in the areas of those sciences they want to analyse. Typically, SSK fieldworkers enter scientific fields which they do not know, and try to learn enough about them to do sociological analyses. Rarely, however, do they reach the level of expertise of a full-blown participant. In the case of the esoteric sciences, the fieldworker hardly ever participates in the science itself. Thus, to begin with, by reflecting on certain sociologists' fieldwork experiences, we can distinguish three levels of expertise.

1. No expertise – the degree of expertise with which the fieldworker sets out. It is insufficient to conduct a sociological analysis or to do quasi-participatory fieldwork.

2. Interactional expertise – enough expertise to interact interestingly with participants and carry out a sociological analysis.

3. Contributory expertise – enough expertise to contribute to the science of the field being analysed.

There is also ubiquitous expertise, expertise which can be hugely skilful – as in the case of natural language skills – but remains unrecognised because ubiquitous.

Interactional expertise involves the ability to sum up the ability of experts. The statement "nuclear is safe", made by Lord Marshall after a train crash involving a container of nuclear isotopes, invited people to accept the safety of every nuclear industry process or none of them; they chose the latter.

Sometimes expertise in one field can be applied to another. A third category of expertise which seems useful is 'referred expertise'; which is, as it were, expertise 'at one remove'. An example could be the managers and leaders of large scientific projects. In general they will not possess contributory expertise in respect of the many fields of science they must co-ordinate. Referred expertise will work in relation to a close field, but becomes punditry (baseless judgement) if removed too far from the centre of one's own expertise.

How can we use scientific knowledge in these unresolved periods where the speed of politics is greater than the speed of science? What credentials can we use, other than weighing institutional and other forms of experience which have a bearing on the question? Defining experience is difficult, it's attributed, it's socially constructed, but there is no alternative. Brian Wynne's

Cumbrian sheep farmers are a classic case of the possession of appropriate experience that qualifies them as experts. Mothers resisting MMR are the opposite, they have inappropriate experience, or are just wrong.

Discussion 5.3

🗣️ The role of experience in this approach may be thought to cut off the role of the naïve enquirer and more generally undercut the whole democratic nature of public engagement.

🗣️ Mothers resisting MMR can be thought of as having inappropriate experience or invalid judgement only if 'science' in the MMR debate is restricted to epidemiology. Mothers may be experts on their kids and their observations may be consistent with certain clinical perspectives.

🗣️ The issue is not about exclusion from democracy but the need to avoid confusing democratic participation with the resolution of technical issues.

🗣️ Science is an absolutely dominant form in political legitimation; we need to discriminate between technical questions as such and those issues that are wider but which are shoe-horned into technical questions and are therefore referred, inappropriately, to the scientist.

🗣️ Democratic legitimacy doesn't answer every question; academics ought to be able to recognise when something else is going on which requires appropriate experience to be assessed as part of the picture.

🗣️ There is a tendency to think of the lay person as ignorant. What is the relevant qualification for engagement? Doesn't experience of patient interaction with medical practitioners suggest that

DOES ECONOMIC GROWTH REST ON SCIENCE PRODUCTION OR DO BOTH REST ON HEALTHY CIVIL SOCIETY STRUCTURES? IN EITHER CASE, INSTITUTIONAL CHANGES ARE NOT EASILY BOLTED ON. INCREASED EMPHASIS ON THE ROLE OF THE CONNOISSEUR MIGHT ESTABLISH THE KIND OF SOCIAL RELATIONS THAT UNDERPIN EFFECTIVE KNOWLEDGE TRANSFER.

22. Steve Fuller, Thomas Kuhn: A Philosophical History for Our Times (Chicago and London, University of Chicago Press 2000).

23. The source for these is a lecture from the UK Chancellor of the Exchequer, Gordon Brown, in March 2004.



people can surf the internet to gain the requisite knowledge and will do so if personally involved?

- Large amount of research under an earlier phase of science studies suggested that scientific expertise was complex, sometimes tacit, often difficult to replicate; ironic that now it has become seen as something relatively easy to acquire.

- The model plays to the gallery of justification, acknowledging expertise and experience, but only on this one domain. Wider pertinent questions are crucial, and often those citizens and consumers have an important role in judging what these are.

- Scientists operating as public intellectuals are quite often functioning outside their own area of expertise – for example, in commenting on why physics is not more influential in schools. Interactional expertise is in play on these occasions.

- The entire scientific advice system relies on the broadening of expertise in this way. The system will find someone within the immediate reference group – “good old Fred, he knows some of this” – rather than looking further.

- Indeed, peer review itself is entirely based on interaction rather than contributory expertise, in that the peer reviewers are selected by those with interactional expertise.

- Where is the intermediary in all this? What are the arrangements that can generate trust? What are the means of decision-taking which can take account of non-expert narratives, like that of the MMR mother who has tried to synthesise what is going on and transmit it to her peers? If we can't reconcile the naïve and expert what social mechanisms can we have?

- The Collins model of expertise and experience itself doesn't exclude intermediaries or popular connoisseurship. It does try to indicate the kinds

of expertise and experience appropriate to different roles in producing, mediating and using knowledge; against this background it is able to accept that popular connoisseurs, for example MMR mothers, may be wrong.

- There is a wider social context for the development of the model in the relativism of all kinds of expertise under postmodernism and in freedom of information which has allowed issues to be opened up for debate: it is necessary to contest some of the more simplistic relativistic readings coming from post-modernism.

- Politicians, in confronting popular causes related to science and technology, are motivated not by a wish to mediate between science and society as such, still less to adjudicate between different forms of expertise, but to facilitate democratic choice.

- However, there is one view of democracy which excludes the need for expert mediators. This seemed to inform the decision to wind up the Office of Technology Assessment (OTA) (an institutional connoisseur?) in the USA by the incoming Republican majority in the mid-90s. In this case politicians saw it as their job to translate constituents' views into legislation and budgets. More generally, politicians often have a suspicion of intermediaries limiting their agency.

Theme 2: Defining opportunities for scientific connoisseurship

Peter Healey: An American experiment 5.4

Steve Fuller, in one of two books on Thomas Kuhn,²² looked at an experiment in science education which was in some sense an attempt at

building scientific connoisseurship in the post-World War 2 USA.

An issue of public policy at the time was how to cope with returning servicemen. There had been advice to Franklin D Roosevelt that there would be a threat to civil order if GIs did not share in the anticipated post-war economic boom.

The corresponding science policy issue was laid out by James Bryant Conant (then President of Harvard), who saw some of the dilemmas that still confront us today, but which were novel at the time:

- Increased science expenditure on the public purse.

- Disagreement/competition between experts – the competitors for those public resources – on the path science should take.

- The consequent need for the typical Harvard trained corporate executive or public servant to become 'expert in judging experts'.

The conjunction of these two policy concerns led to a new science education course under the GI Bill at Harvard, designed to produce such 'experts in judging experts'. It stressed that students should reach these judgements through the ability to assess methodology from without – as what we would now see as non-practitioner connoisseurs. Later the approach was diffused through liberal arts programmes (though non-systematically). The irony that the novel Harvard science education approach was defeated by Sputnik in 1957 when a Harvard review by Jerry Bruner, the famous psychologist, concluded that, consistent with the national mood for more effective technological competition with the Soviet Union, the absolute priority was training based directly in science practice.

You can see echoes of this early approach from a slightly different angle in the UK in the broader education of scientists and engineers in the wider social context of their work under the then SSRC/SRC Joint Committee. This operated between the late 60s and mid-70s, and sponsored much experimental training, for example Joan Woodward's courses in Imperial College.

Tony Whitehead: A government perspective 5.5

This was a view from the demand side. Are there questions from the government perspective to which scientific connoisseurship would be an answer?

The view of the UK Government is that science underpins national prosperity. As yet nothing in science and technology has been excluded as a result of public response but there have been delays. The downsides of negative public response are that the area of science concerned can be de-prioritised with a consequent loss of flows of benefits in technology, education and jobs.

There is a government ten-year strategy for science and technology, the objectives of which are:²³

- World class research driven by competition between excellence.
- A science base meeting the needs of public and private funders.
- More collaboration between academia and industry.
- Better translation of science into commercial products.
- More scientists, underpinned by excellent teaching and an excellent curriculum.

From the perspective of his directorate, in five years' time Tony Whitehead would look for the following.

- People who are more comfortable about dealing with risk and uncertainty – eg mobile phones, where complicated risk assessments are being made.

- A consistent application of ethics – is it possible to produce a universal ethical code for science? Whitehead would like a free and open scientific agenda (subject to resources) provided and pursued in an ethically responsible way – could there be an equivalent of the Hippocratic oath?

- A holistic cradle-to-grave approach to the whole relationship between science and the community, and a reduction and simplification of interfaces.

- Public engagement, characterised by transparency and honesty – improving the relationship with the public.

- Indicators of:

- Improved public perception of scientists, engineers and technologists.

- Improved media coverage, with a more mature approach.

- Improved role of women and ethnic minorities in science.

- Improvement in trends in education, and improvement in the numbers of those pursuing careers in research, particularly in the hard sciences.

Focussing on the role of the connoisseur, are we in danger of replacing one set of experts with another set of experts? Where are the new interpreters going to sit in the spectrum of being believed? Who were they going to be? Even academics have funding sources, and they might be sucked into the suspicion of bias already associated with government and industry science.

Alternatively this kind of cynicism may be countered by a recognition that everyone needs sources of funding.

Discussion 5.6

- There is a tension between science for industry and science for society. The training needed for science and the training needed for scientific citizenship are fundamentally different.

- Does economic growth rest on science production or do both rest on healthy civil society structures? In either case institutional changes are not easily bolted on. Increased emphasis on the role of the connoisseur might establish the kind of social relations that underpin effective knowledge transfer.

- A recent review by Nick Von Tunzelmann suggests that the relationship between aggregate R&D expenditure and economic growth doesn't hold.

- This does not however rule out a relationship between industrial R&D and economic growth.

- In any case there are opportunity costs in the choice of scientific priorities, between GM and low input agriculture, for example.

- The issue is not one of trust, of education or of democracy, but of what is required to inform decisions. Is the public prepared for uncertainty?
- Consensus mechanisms are proxies for the public to trust, and an alternative to direct participation.

- Education is a base for popular scientific connoisseurship – we don't have a dialogue in society as to what constitutes good science and bad science. We do have such a debate on literature and in literary education.

WHILE WE MIGHT WANT TO SEE ALL THE PARLIAMENTARY INSIDERS AS CONNOISSEURS, WE CANNOT BE AS SURE ABOUT THE DEPARTMENTAL INSIDERS. EXPERT ADVISORY COMMITTEES WHICH PROVIDE MONOLITHIC PRESCRIPTIVE POLICY ADVICE (PRETENDING THAT IT IS PURELY SCIENTIFIC) RATHER THAN PLURAL AND CONDITIONAL ADVICE ABOUT A RANGE OF POLICY OPTIONS, DISQUALIFY THEMSELVES, PARTLY THROUGH MISREPRESENTING POLICY OPTIONS AS IF THEY WERE SCIENTIFIC.



Gary Kass: Connoisseurship in governance – what's there, and how it could be improved
5.7

Institutions with capacity for connoisseurship include the Parliamentary Office of Science and Technology (POST); the House of Commons Library, answering individual enquiries of members; Select Committees and their advisers; all-party groups, members and industrial interests (although these include a good deal of lobbying); the government's Chief Scientific Adviser – who arguably functions directly as a scientific connoisseur; scientific committees with a responsibility for risk assessment, such as the Advisory Committee on Releases [of genetically modified organisms] into the Environment; broader agencies and advisory bodies, such as the Agricultural and Environmental Biotechnology Commission or the Royal Commission on Environmental Pollution (RCEP);²⁴ the Royal Society and other scientific bodies in some of their roles.

The Research Councils are occasionally asked to act as discriminatory scientific connoisseurs, such as when the government asked the Natural Environment Research Council (NERC) to rule on whether the dumping of Brent Spar would be safe.

To characterise these:

1. They would claim legitimacy and independence, although independence doesn't translate directly into balance in all cases.
2. The role actually played varies: they either lead debate or inform it (the latter in the case of POST), and they undertake either reportage or analysis.
3. The form of control exerted on these bodies also varies widely – professional ethics; club rules; peer review; codes of practice.
4. Sanctions also vary widely – voluntary,

imposed, market-based incentives, etc.

The phenomenon of public participation is increasing across all policy domains: health care, planning, and so on, with three principal drivers:

1. General policy – reaching back to the UK 'modernising government' agenda (introduced by the Labour Government in 1997).
2. International and national legal requirements (Aarhus convention on environment management internationally, the nuclear decommissioning machinery proposed in government legislation, specifying varying degrees of meaningful public consultation as part of the process).
3. 'The deliberative fix' – we've tried other means, and now come to this.

The aims of these deliberative mechanisms are often understood by those commissioning and taking part in them, but there is less understanding of rationales and processes. There is a need for capacity building. In particular there is a deficit in capacity in forecasting or measuring outcomes.

Government in the UK has limited experience of connoisseurship or mediation. The connoisseur's contribution can be constructive/positive or destructive/negative. For it to be positive requires that:

1. Practitioner connoisseurs (eg in advisory committees) should understand their roles and not stray on to political decision ground (eg by setting limits for toxicity).
2. Scientific connoisseurs should have the capacity and willingness to respond/act in response to the needs of the exercise.
3. They should be willing to stand up and be counted – have their views attributed and be willing to be subject to scrutiny and criticism: important if connoisseurs are to gain respect and legitimacy.

Discussion
5.8

In this discussion we are assuming practitioner connoisseurs are scientists themselves. In the terms of Harry Collins' analysis the referred expertise is sometimes stretched to or beyond its useful limits: because a scientist is senior in one domain, authority in another is assumed. Senior scientific status – for example as an FRS – is sometimes seen as a qualification to talk about all aspects of science, or even more controversially, science in society. By contrast, the RCEP sees itself as a 'commission of experts' not an all-purpose 'expert committee'.

There is also concern with the scientific framing of issues and with the compartmentalisation of issues as a way of separating out things which bother people.

There are examples of experts being mobilised to contribute to public engagement exercises at the local level of politics – for example in relation to waste disposal in Hampshire – or in experimental exercises – as with Andy Stirling's work on deliberative mapping – although there are no great intrinsic rewards for them to do so.

Engagement can be considered a kind of tyranny that prevents people spending more time with their families. But while stakeholders, who have other options for exercising their agency, sometimes express frustration with engagement, there is little evidence of this being true of citizens. On occasion, the problem becomes that they won't disengage, with their involvement threatening their marriages, etc.

There is a danger in eliding several things together, such as parliamentary insiders and departmental insiders. While we might want to

see all the parliamentary insiders as connoisseurs, we cannot be as sure about the departmental. Expert advisory committees which provide monolithic prescriptive policy advice (pretending it is purely scientific), rather than plural and conditional advice about a range of policy options, disqualify themselves, partly through misrepresenting policy options as if they were scientific. The role of the civil servants in expert committees has traditionally been very strong: until 1994, draft minutes of a meeting were attached to expert advice presented to committees. In this way the committee was often effectively squeezed into the advice wanted, in a way that would now violate the code of practice about how advisory committees should be run. Typically, what advisory committees fail to do is offer plural and conditional advice; the process of delivering robust technical advice is about specifying the conditions around it.

Most politicians do not want pre-packaged decisions – they rather regret the loss of their agency in making decisions.

Non-governmental organisations can be connoisseurs but typically have a policy/interest agenda. While disinterested advisors are not a realistic option, declarations of interest, clarity as to sources, accountability and openness to scrutiny are, and need to be, widely applied in engagement exercises.

Political framing can inhibit open discussion in a downwards direction too. It is difficult for US policy advisors to advise on climate change in the current political climate; however, in the UK the role of chief scientific advisor is one which includes the duty of conveying unpopular advice.

Theme 3 – Supporting and assessing change

Charles Polk: The role of markets
5.9

A different perspective is offered – on the role of markets in adjudicating expertise. Markets exist on the basis that the truth of claims of what is brought to them is irrelevant. How do you adjudicate between different forms or qualities of expertise? Markets accommodate this also, to the extent that the ignorant contribute to the operation of markets. Some examples of markets in relation to science – markets as monitors of patronage – follow.

1. The 1986 Challenger disaster – the day after this happened there was a drop in the price of Morton Thiokol stock. The cause of the disaster was known by mid-afternoon on the same day; a panel of luminaries with skills of varying relevance convened; one reason that panel existed was to delay resolution until the public had forgotten about it. Given the causes and companies in the frame one of three stocks was going to fall – the market gave an indication, an air of legitimacy, as to where to look, while the panel moved slowly. Eventually Richard Feynman, the panel chair, was given the information that had informed the price shift, so put the seals into liquid nitrogen and demonstrated seal failure.

2. A second example was NASA's Cassini mission, where there was an attempt to avoid cost overruns dogging payloads on missions in the past. The challenge was to try to change the incentive structure to get over this. Decentralised management, distributed budgets, and permission to trade between them were introduced in an attempt to smooth out problems: it was the only

payload development to come in on time and on budget.

3. An example of non-experts making the business/practice of science more efficient – Craig Venter and the Celera human genome mapping effort which did a 'very good job' – market forces providing patronage to a scientific act.

4. Eco-tourism and some forms of purchase in post-industrial society are forms of consumption that support certain forms of scientific goal. Buying up land for conservancies or eco-tourism is an example of a broader interest being expressed through markets. This kind of mechanism can operate on a small scale.

Inside science, there are huge information problems about who is doing what and what has merit – an example is the difficulty of picking winners/risk management in pharmaceutical development. Net Exchange is currently working for a pharmaceutical company on this. It involves monetarising information and providing managers with a stake which they can trade. This provides a way of getting past the problem of protection of information by mid-level fiefdoms looking after their pet projects.

It would be interesting to look at ways of making secure issues of what has commercial or public policy relevance in publicly funded science; these kind of surveys could be used instead of focus groups etc to get high quality information.

Discussion
5.10

Are these examples convincing? Did the market misallocate blame, which lay not with Thiokol but with NASA? How far did the private genome mapping effort free-ride on information

PRECAUTION DOESN'T MEAN BANNING THINGS, IT MEANS THAT IN A SITUATION WHERE SCIENCE LESS THAN FULLY ACCOUNTS FOR DECISIONS, YOU INSTITUTE A FURTHER SET OF PROCESSES, YOU COMPARE DIFFERENT OPTIONS, YOU LOOK CAREFULLY AT THE BURDEN OF PERSUASION, THE LEVELS OF PROOF, YOU LOOK AT THE PROS AND CONS, YOU UNDERTAKE A MORE ELABORATE PROCESS OF APPRAISAL. THIS DOES NOT NEED TO END IN BANNING, BUT LABELLING PROVISIONS, ETC – THE POINT ABOUT IT IS THAT IT IS A MORE ONEROUS AND EXHAUSTIVE PROCESS.

25. SET for success. The supply of people with science, technology, engineering and mathematics skills, report to the UK Funding Councils, April 2002.

26. Enquiry into the Flow of Candidates in Science and Technology into Higher Education, report of the Council for Scientific Policy (HMSO 1968).



being provided from the public effort? Participation of ignoramus in markets can actually ruin them – by hopelessly overvaluing stocks which then crash, causing widespread human misery.

⚡ Market information should be seen not as a signal of blame, but of cause. Markets pool information and amplify it. Clearly with market bubbles this amplification process causes tragedies. Price changes typically occur because of something that happens at the margin, in the Thiokol case by the selling of stock by two large holders who were in the position to know. It cannot be said that stockholders in general act as connoisseurs.

⚡ The Thiokol case is analogous to Greenpeace's claims over Brent Spar – it sent an effective but wrong signal.

⚡ Concerns over GM food were amplified through the market. At one point, Unilever would say that all the enquiries they were getting were about GM.

⚡ The development of organic foods in the US is a similar case; organic is becoming more affordable, an example of market driven change.

⚡ These cases exemplify the role of power in closing down discussions. The Thiokol case is in favour of the market-driven case in that the market, with its more widely distributed power, showed itself less susceptible to special interest than the panel (the connoisseurship analogues in Thiokol). In the case of Greenpeace and Brent Spar, Greenpeace made an error of a couple of orders of magnitude but the original inventory, as prepared by Shell, was out by a similar order of magnitude, so why did one error prove crucial but not the other? There is an issue of power and closure and how that works.

⚡ Markets offer a powerful means to allocate resources and interpret information, where you have a given set of institutional arrangements, a given set of property rights, and an agreed framework for constituting efficiency. But the problem is that you are in a situation where there is often a lot of disagreement about what constitutes efficiency and fairness; how can markets adjudicate on that?

⚡ Creating a market involves creating property rights. If there is no way of determining what has value, if there is a tremendous issue about what is at stake, rather than an issue as to what kind of development can go forward or be pursued, then that may not be the kind of problem that can be resolved by a market. If a problem which can be objectively resolved can be defined, those various outcomes can be made secure, and there can be information sharing around that. If the problem is never going to have an objective solution, it can't be resolved through a market – the market works for problems that are part of a finite set.

Erik Millstone (commissioned paper in Annex C) 5.11

This paper is about the use of social science analysis to open up the construction of science used as the basis of regulation, so as to assess its quality. The first two cases are of regulatory testing and approval of two sweeteners – saccharin and aspartame. Official regulatory organisations, in the case of saccharin, adopted the most optimistic end of the range of possible outcomes in giving dosage limits; analysis under the sociology of scientific knowledge shows these judgements are very poorly constructed.

Aspartame “started with incompetence, went

on to dishonesty”. Experimental rats were dying, and then appearing in the experiment again – apparently having been resurrected – the next week. The company was found out, then lied to cover it up, then bribed a series of public officials. The deception came out because a discrepancy between the executive summary of a trials report and data was resolved by the company changing the data.

The basic claim for the method is that, in a field where science is entirely unreliable, understanding how science is constructed tells us a great deal about its quality.

It is important that the third case – that of lead – constitutes a counter example, where knowing more about the construction of the science doesn't lead you to conclude greater scientific uncertainty. It is important that the method is understood as discriminating, and not just as a tool for the deconstruction of scientific certainty as such. Lead has been known to be a neurotoxin since the 1930s – regulation assumed that there is a threshold below which no significant damage occurs. UK policy has been consistent – never to set a target until it has already been met. The case of lead shows that experimental knowledge in fact can become more reliable over time.

Social science analysis of this kind can provide discriminating curatorial judgement, input into public policy, also input into science policy in recognising the incentives in institutions not to acknowledge scientific uncertainty; its main benefit is to put aside this nonsense struggled with since the end of WW2 – that decisions rest on sound science. In giving the GM decision, Margaret Beckett went even further, calling it “a scientific judgement”. The apocryphal statement

of a Minister to the Chairman of a Government Agency on appointment – “Professor, I shall never hesitate to use you as a shield” – illustrates an approach which constitutes both a fundamental political abuse of science, and an evasion of political accountability.

Discussion 5.12

⚡ Not clear why so much uncertainty about the toxicity of saccharin still exists; aspartame illustrates corruption, not bad science, indeed not science at all. The case illustrates that when interest and money collide, and checks and balances break down, money wins. Decisions do get made. We can see that the consultation process sometimes gives politicians room to make decisions because they believe in them; on other occasions, it results in better decision-making. The whole notion of ‘sound science’ is problematic – politicians can hide behind it.

⚡ Wanting to see how the science was done and how the results were produced is a laudable social science activity, but while in this case it served an interest in the toxicity of certain substances, SSK practitioners would look symmetrically. Cheating is not interesting to SSK, it is of interest to the law; honesty is much more interesting because all the forces are against it.

⚡ Millstone's interests, commitment to food safety and quality, frame what he does. However, his conclusions are open. In relation to this specific case, Millstone was invited by the US Department of the Interior to look at the lead issue as an outsider, went with an open mind and came out feeling that their procedures were pretty good, but it was more the UK that had the problem.

⚡ We can be happy with the paper because we can recognise Millstone as an expert according to some definition. An important question for the workshop is how he got this expertise.

⚡ He learned it as an individual. Millstone has a first degree in physics, a second in philosophy and no formal training in the social sciences at all. He did a PhD on the epistemology of heavy metal, and stumbled into toxicology policy.

⚡ Doesn't agree that to do SSK involves going into the laboratory: in the aspartame case there were 45 kilos of documents about what went on in the laboratory, it just didn't go on in the researcher's presence.

⚡ Would challenge the notion that scientific fraud is not interesting to a sociologist of science; it is interesting because fraud and incompetence are probably not infrequent, and because the institutional mechanisms for detecting and dealing with them are often absent.

⚡ It is axiomatic that in line with the symmetry principle in SSK, certainties are always someone's uncertainties, and that certainties always become uncertainties. From this perspective a lot of the story is being lost through ‘revealing’ uncertainties; it opens up questions, but doesn't address another level at all, and on that level doesn't actually settle much. The decision-maker is not put on the intellectual hook by this analysis: (s)he can still talk about ‘sound science’ on the one hand and politics on the other as if they can be separated and doesn't acknowledge that it is a struggle to take them apart and put them together. This is not some radical solipsism that we can't decide – we need to understand what arguments lead you to be persuaded that any specific case represents certainty or uncertainty.

⚡ The SSK/sociology of science distinction is not helpful because an analyst in this situation is grappling with great asymmetries of power, the issue is not what decisions are made but how to justify decisions, who is accountable and by what means – centre stage is how scientific connoisseurship deals with issues of justification and accountability. Precaution doesn't mean banning things, it means that in a situation where science less than fully accounts for decisions, you institute a further set of processes, you compare different options, you look carefully at the burden of persuasion, the levels of proof, you look at the pros and the cons, you undertake a more elaborate process of appraisal. This does not need to end in banning but labelling provisions, information provisions, etc – the point about it is that it is a more onerous and exhaustive process.

Jonathan Osborne: issues of science education 5.13

Science education is on the horns of a dilemma – both trying to provide pre-professional training, and increasingly also preparing students for scientific citizenship. The problems of science education are largely driven by the supply line of scientists. The Roberts Report²⁵ for example is primarily concerned with the supply line although it includes some acknowledgement of the issues of citizenship in a scientific society. Dainton²⁶ had the same supply concerns 30 years earlier. These seemed to have proved unfounded – we need to ask whether the supply of scientists is really a problem.

Science education in the UK in the 1960s–1980s was very gender stereotyped – boys did physics, girls did biology; counter to this

FORMAL SCIENCE RESTS ON ITS ONTOLOGY – ITS EXPLANATIONS ABOUT THE WORLD – AND VERY LITTLE ON ITS EPISTEMOLOGY, AND THE KIND OF SOCIAL EPISTEMOLOGY BEING DEALT WITH – ABOUT POWER AND INFLUENCE – IS KEPT OUT BY A RIGID HERMETIC SEAL AT THE LABORATORY DOOR.



was the proviso brought in at the end of the 80s that both boys and girls did all the sciences until the age of 16. It was hoped that this would address the gender stereotyping problem – it has only done so to some extent in chemistry. This is not very positively engaging; as Jerry Ravetz said in evidence to the Select Committee,²⁷ science is a very authoritarian knowledge system, very unreflective, offering little room for discussion, you just have to get on and learn it. The science knowledge system contrasts with the view promulgated by some contemporary sociologists that society is now much more reflective, that people want to discuss.

A second issue is the use of scientific knowledge: this is always very context specific, and has to be reworked. In use it is not very much related to anything that people may have learned in school; therefore they can easily come to the conclusion that school science is not needed and because it is not needed, can be lost. That said, one can be sceptical of public understanding of science surveys that show the public as extremely ignorant – you would get the same findings with Shakespeare. Formal science rests on its ontology – its explanations about the world – and very little on its epistemology, and the kind of social epistemology being dealt with – about power and influence – is kept out by a rigid hermetic seal at the laboratory door.

A third problem is that school science is limited in range – physics, chemistry, biology – and these deal with certainty, precision and well-established consensual knowledge. This is unsatisfactory – in content, there is a huge explosion of new knowledge, about genetics and so on, which is hardly reflected in a curriculum which

consequently appears remote from pupils' lives. The need is for science education which is based instead on something of universal value – what should that be? In the context of scientific connoisseurship, how can formal science education make a contribution to a critical scientific literacy? A 1998 report²⁸ set out a new approach to this – to education for science citizenship – and a new syllabus which is being introduced this year embodies many of its ideas. There is a lot riding on it; its success will be partly judged in terms of the traditional concerns about recruiting new kids into science and solving the supply line problem, although that is not its intention.

These are the ideas about science which we feel the course for 14–16 year olds should cover, plus science for public understanding (optional):

- Concepts of evidence, data.
- Relationship between correlation and cause.
- Theories and models.
- Scientific community and its practices (eg peer review).
- Politics, ethics, implications.
- Exploration of risk.

The aim is to help people to become critical consumers of science. The main work is being done at the University of York; Jonathan Osborne is involved in the pilot scheme as an evaluator. There are problems in enacting this new kind of model of education for critical consumption; science teachers (although mostly not themselves scientists by experience) are quite good at socialising new generations of scientists. They may only be supportive because there is seen to be a supply crisis.

The new curriculum offers a different kind of science at 14; if that is successful, to what extent

should people be required to study these kinds of issues and in how much depth? A contribution on two main issues would be welcome from social studies of science:

- what are the requirements for scientific connoisseurs?
- what is the role of informal science education – should we worry about this – how far should this kind of community be concerned with this?

Discussion 5.14

- Science fiction puts science into a social and political context: can science education link up with this new curriculum and generate fervour?
- But science fiction is in a narrative form – not the discourse of science – and it encounters accusations of lack of rigour, etc. So it is hard to see how its perspectives would apply.
- The approach again seems to be predicated on the essential separateness of science and society, and its analytic force is reduced by doing that, by failing to acknowledge that all the constructs and issues in science are socially constructed.
- It may have been structured in this way because if you give the scientists things they recognise, they are likely to accept the rest.
- Does this approach also encourage scientists to be critical producers?
- This new curriculum is only a base, and is not compulsory. Further, it is followed by more traditional forms of science education, or applied courses, etc (or by no more science). Students don't become better at criticising science under traditional science education until they are scientific practitioners; whether they will under this scheme remains to be seen.

• Could this be the basis for cross-curricular effort to provide education which would provide a more critical approach to evidence, and critical analysis methods – for a more general set of skills that will support popular connoisseurship across a range of domains?

• We have already seen a revolution in the teaching of history – from learning facts to the assessment of evidence in context. However, we have to recognise that history and science are different. History will always accept multiple interpretations of past events. Science is about closure. Where geography sits would be different again. So constructing such a cross-curricular programme would be challenging. There is also the factor of attention. Cross-curricular initiatives, citizenship being the latest, are all politely ignored by the education profession.

• Since this analysis of science education seems closer to a social science than a natural science perspective on the sciences, realising it may involve utilising social scientists more in decision-making and implementation; so it can be seen as part of the wider issue of having social scientists recognised as public intellectuals on the social aspects of science.

• This is because the conventional model doesn't work – the public wants to consume more science but be less involved with the nuts and bolts of it – we need another way and social science seems to offer a large portfolio of attractive options. The sort of contributions we're hearing here are part of the lens that will help policy-makers determine what we should be doing and will also help with the execution of difficult tasks such as assessing the effectiveness of science centres and science ambassadors in schools.

• The perennial issue of the role of social scientists applies of course in relation to their contribution to the development of science education and also scientific connoisseurship: social science analysis should always open up analysis and discussion, and not be a servant of closure.

6.0

CONCLUSIONS AND RECOMMENDATIONS

SECTION 6

6.1

This workshop was regarded by the participants as a stimulating beginning to an important discussion with wide implications for the education and governance of science and technology. The workshop will inform its participants' individual work and some of its presentations may also be developed into a special issue of a journal. There will thus be a number of places where the workshop will contribute to ongoing debate.

6.2

Given this perspective, it would be premature to expect final conclusions at this stage. Instead, this section offers a number of key points distilled from discussion, and a number of proposals for experimental action extrapolated from points discussed in the final session.

6.3

The key points which emerged from discussion:

- While there is evidence of 'participation fatigue' among stakeholders, who have other contexts in which to pursue their interests, there is no evidence of this among citizens. Scientific connoisseurship needs to find roles which support and articulate wider public engagement.
- Although public intellectual, non-practitioner, connoisseurs are important, (and some examples were detected among the workshop participants), it follows that developing popular connoisseurship is a key objective. In this context it is important to establish preparation for scientific citizenship as a higher priority in secondary science education. The current UK curricular innovation is an important first step, but it must be recognised that education for scientific practice and

education for scientific citizenship are in competition, not only for time in schools, but in relation to the values and skills they are trying to instil.

- Social science offers "a large portfolio of attractive options" for identifying and implementing change, according to one practitioner at the workshop. The contributions include new attention in the sociology of science to the role of experience in relation to which forms of knowledge qualify people to undertake which roles and make which judgements.

■ Although scientific connoisseurship could be exercised by individuals, it is best thought of as a process rather than a role, often best performed by institutions. A number of institutions are already acting as effective intermediaries between science and its publics and science and government. However, it was thought that more could be done to assess what is going on in scientific governance, to ensure that more self-critical and self-aware principles on the construction and use of science in decision-making are consistently being carried through in practice on the ground. There are challenges in doing this, given the scope of science and technology governance.

- The social roles which could be played by individual and institutional scientific connoisseurs – framing public debate, adjudicating knowledge claims crucial to regulation, contributing to the education of critical consumers and citizens – are important. It is essential that they themselves are accountable in their methods, approaches and values. In this way scientific connoisseurs would be self-exemplifying – opening up issues for public scrutiny and debate not only in what they do but also in the open and transparent way in which they perform their role.

THE SOCIAL ROLES WHICH COULD BE PLAYED BY INDIVIDUAL AND INSTITUTIONAL SCIENTIFIC CONNOISSEURS – FRAMING PUBLIC DEBATE, ADJUDICATING KNOWLEDGE CLAIMS CRUCIAL TO REGULATION, CONTRIBUTING TO THE EDUCATION OF CRITICAL CONSUMERS AND CITIZENS – ARE IMPORTANT. IT IS ESSENTIAL THAT THEY THEMSELVES ARE ACCOUNTABLE IN THEIR METHODS, APPROACHES AND VALUES. IN THIS WAY, SCIENTIFIC CONNOISSEURS WOULD BE SELF-EXEMPLIFYING – OPENING UP ISSUES FOR PUBLIC SCRUTINY AND DEBATE NOT ONLY IN WHAT THEY DO BUT IN THE OPEN AND TRANSPARENT WAY IN WHICH THEY PERFORM THEIR ROLE.

6.4

In parallel to developing these ideas in a variety of contexts we propose a modest programme of experimental research – action research – which will help define the scope and limits of scientific connoisseurship, and related forms of mediation between science and its publics. The programme is summarised below and outlined in more detail in Annex D. All four suggestions put forward are responses to perceived gaps in the current system of scientific governance; equally, all four are derived more or less directly from ideas and suggestions put forward at the workshop. Although some potential funders are indicated, the first stage in some cases may be a further workshop to recruit a wider range of stakeholders to the ideas and approaches suggested. The approach is intended to be small scale. Each activity is intended to allow practitioners/policy-makers and social scientists to assess the extent to which each particular initiative meets the claims made for it and can be seen as making a recognised contribution to the development of new forms of scientific connoisseurship.

6.5

The proposals for experimental action are:

- Extending the kind of 'constructive deconstructive analysis' that social scientists conduct on regulatory issues (eg in food research) into a range of domains and institutional settings (eg the Food Standards Agency) where it might demonstrate benefits.
- Introducing 'socio-technical vigilance', involving systematic evidence from the public on the intended and unintended consequences of socio-technical change, initially by tapping into and

critically assessing evidence from science shops

- Exploring the potential across the research councils of 'open door' type research schemes piloted, but not developed, by the ESRC, allowing requests for knowledge from civil society institutions to be pursued and developed into potential research proposals.
- Reviewing the possibilities of building bottom-up research agendas based on local and community-based needs. This would include evaluating the applicability of 'contestable democratic design criteria' to scenario workshops – a participatory technology assessment methodology – in the definition of community research agendas.

ANNEX A

WORKSHOP PROGRAMME

Thursday 11 March



10:30
Bus pick-up from Oxford Station

11:00–11:30
Registration and coffee (Malthouse)

11:30–11:50
Introduction to the Workshop: Professor Steve Rayner. Introductory Tour-de-Table.

11:50–13:00
Theme 1:
Learning about Connoisseurship and other Intermediary Forms (Malthouse)
What can we learn from other domains, or from institutional intermediaries in S&T, about: Purposes of connoisseurship; roles and styles of mediation; sources of legitimacy and authority, construction of evidence and argument; the identification and management of conflicts of interest. How does our more general understanding of different forms of expertise help us to interrogate and position connoisseurship?

13:00–14:00
Lunch (The Old Swan)

14:00–15:30
Theme 2 session 1:
Defining Opportunities for Scientific Connoisseurship
What can we learn about the history of scientific connoisseurship? What are the current problems of science and society to which scientific connoisseurship is seen as a contributory solution? How are these distinctive of our scientific culture and our times? Does the agenda for scientific

connoisseurship look different in a globalised world with a division of labour in knowledge production? How can we recognise scientific connoisseurship in action, and how do we deal with potentially conflicting roles that we might demand of it? What might be the unintended social consequences?

15:30–16:30
Tea and coffee – pick up room keys

16:30–18:00
Theme 2 session 2:
Defining Opportunities for Scientific Connoisseurship
What are the principal forms of scientific connoisseurship and what do we understand about popular or professional scientific connoisseurship? How is scientific connoisseurship socially constructed – or limited? What sort of contributions can we expect from it – does it lead or lag behind social debate on the issues or redefined issues? Does it have an anticipatory role in relation discontinuities of subject and method? In these respects how does it relate to other forms of mediation between science and its publics/ different stakeholders in science and technology? In particular, what relation does it have to the broader participation of the public with S&T, and to the development and delivery of public policy?

18:45–19:30
Pre-dinner drinks (Bar area, The Old Swan)

19:30–21:00
Dinner (The Old Swan)

Friday 12 March



08:00–09:00
Breakfast (The Old Swan)

09:30–11:00
Theme 3 session 1:
Supporting and Assessing Change
What base of knowledge and experience would scientific connoisseurship draw on/need to build? Is there a particular role for the sociology of scientific knowledge? How far is scientific connoisseurship a broad set of roles, and how far a putative social technology, supporting the 'co-evolution' of science and society? What can we learn from existing forms and approaches, such as constructive technology assessment, or the 'anticipatory research' of the NEST insight projects? *This session will include a Panel-led discussion of Erik Millstone's pre-circulated paper: How can scientific connoisseurship be socially useful and intellectually robust?*

11:00–11:30
Coffee and tea

11:30–13:00
Theme 3 session 2:
Supporting and Assessing Change
What do the different forms of scientific connoisseurship suggest for educational curricula? How far do educational changes already under development – such as those in the new science and citizenship curricula – contribute to scientific connoisseurship or other forms of public engagement with science and technology? What would be the characteristics of a supportive social environment in which experimentation and learning in scientific connoisseurship could take place with a range of social partners, and how could this be developed and financed? Would this take an institutional form, or have more the character of a social network? How would this relate to, or supersede, traditional models of scientific communication?

13:00–14:00
Lunch (The Old Swan)

14:00–15:00
Next steps: Developing, publishing and experimenting with the workshop's ideas

15:00–15:45
Coffee and tea

16:00
Bus departs for Oxford station



ANNEX B

CONNOISSEURSHIP WORKSHOP PARTICIPANTS

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ANNEX C

ERIK MILLSTONE'S COMMISSIONED PAPER

How can scientific connoisseurship be socially useful and intellectually robust?
by Erik Millstone, SPRU, University of Sussex
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February 2004

What might connoisseurship be?

If the concept of scientific connoisseurship is to have any application, its ascription would imply that people who were not professionally employed as scientists may be able to make well-informed discriminating judgements about the quality of some, any or all of the following: scientific ideas, theories, beliefs, institutions, research programmes, expert judgements and science-based public policies.

If the range of contributors to scientific connoisseurship is to extend beyond those actively and professionally employed as scientists, then we might expect them to provide well-informed insights over and above the judgements that professional scientists themselves normally make. Those contributing to such connoisseurship could, for example, be able to provide insights that the members of the scientific community might be unable or unwilling to provide, or which they might not be expected to volunteer or acknowledge. Such contributions to connoisseurship might come from a variety of different sources. Those sources might include groups with lay knowledge that professional scientists might not possess, while a rather different source might include scholarship in the history, philosophy and sociology of science and among science policy analysts.

I will show how an exciting and illuminating form of scientific connoisseurship can be, and has been, provided by scholars in the sociology of scientific knowledge, and that connoisseurship of this type can make a constructive contribution to policy debates. I will argue that sociological analyses of the construction of science can not only enrich intellectual scholarship and cultural connoisseurship, they can also help to improve the scientific and democratic legitimacy of science-based policy-making by making protago-

nists in science-based policy debates, including expert advisory bodies and policy-making institutions, more honest about the construction and the epistemological status of their conclusions.

Illuminating analyses can be, have been, and are being provided by some sociologists of scientific knowledge, and many of the most interesting contributions have come from those who have adopted what can be characterised as a realist-constructivist perspective. There is a range of competing approaches in the sociology of scientific knowledge. One of my key contentions is that realist-constructivist approaches can make a type of contribution to both scientific connoisseurship and public policy analysis that cannot be provided by approaches that are either anti-constructivist or anti-realist.

Anti-constructivist approaches cannot even start to recognise, let alone understand, the ways in which science-based contributions to policy debates can be exquisitely sensitive to the institutional and policy contexts in which they are developed. Consequently they cannot understand why debates take the forms they do; they can try to retreat to an assertion that one group of protagonists is unproblematically in possession of reliable knowledge, and that all the others are just wrong and/or unscientific, but that tactic has lost what little credibility it might once have had. Constructivist approaches make an important contribution by highlighting the importance of framing assumptions that underpin particular representations of aspects of our world, and consequently anti-constructivist approaches are poorly resourced for explaining the differences between competing representations.

Anti-realist versions of constructivism in the sociology of scientific knowledge may provide some understanding of the different patterns of beliefs in, and of epistemological assumptions

SOCIOLOGICAL ANALYSES OF THE CONSTRUCTION OF SCIENCE CAN NOT ONLY ENRICH INTELLECTUAL SCHOLARSHIP AND CULTURAL CONNOISSEURSHIP, THEY CAN ALSO HELP TO IMPROVE THE SCIENTIFIC AND DEMOCRATIC LEGITIMACY OF SCIENCE-BASED POLICY-MAKING BY MAKING PROTAGONISTS IN SCIENCE-BASED POLICY DEBATES, INCLUDING EXPERT ADVISORY BODIES AND POLICY-MAKING INSTITUTIONS, MORE HONEST ABOUT THE CONSTRUCTION AND THE EPISTEMOLOGICAL STATUS OF THEIR CONCLUSIONS.

among, various groups of protagonists, but they provide no tools with which to evaluate, or discriminate between, the competing claims; in other words, they can contribute only an attenuated range of connoisseurial judgements.

A key advantage of realist constructivist approaches therefore is that they provide resources that can enable well-informed judgements of scientific quality, that are far harder to provide from either anti-realist or anti-constructivist alternatives, to be articulated.

The account provided here is not intended as a general account of scientific connoisseurship tout court. It explores more narrowly the ways in which connoisseurial judgements can be made about one particular but important type of science, namely what Ravetz and Funtowicz amongst others have called 'regulatory science'.²⁹ Regulatory science is that type of science which is developed in a policy context for policy decision-making. Science-based policy decision-making is frequently difficult because, while decisions need to be taken about which technological practices and innovations to accept or even encourage and which to discourage, restrict or prevent, decisions typically need to be made even though facts are uncertain, values are in dispute, levels of trust are low, stakes are high, and decisions are urgent. Regulatory science is a type of science that is formed inevitably with an eye to the use to which it will be put by policy-makers.

In this context, the proper objects of connoisseurial interest include not just:

- How well- or poorly-developed the relevant sciences may be;
- How specific, precise and reliable are their claims, but also;
- How they have interacted with policy context;
- How they have been interpreted and represented;

29. Funtowicz S and Ravetz J, 'Science for the Post-Normal Age', *Futures* 25, 1993, S 739-55.

30. Chubin DE and Restivo S, 'The 'Mooting' of Science Studies: Research Programmes and Science Policy', in *Science Observed*, ed KD Knorr-Cetina and M Mulkay (London Sage 1983).

■ How the relationship between scientific and non-scientific considerations (such as political and economic factors) has been represented.

While the agenda of expert advisors to policy-makers could properly include questions such as: "What is known, and uncertain, about the consequences of following (or failing to follow) any of several alternative possible courses of policy action?", the proper objects of inquiry of the type of scientific connoisseurship on which I am focussing include the assertions and judgements of those expert advisory groups. The question is how have those assertions been constructed and how have those judgements been reached?

A conceptual and historical preliminary disclosure

One of my claims is that, under certain conditions, the results of empirical sociological research into the social construction of judgements in, and about, regulatory science can equip sociologists to participate in and contribute both to scientific and to policy debates, and to contribute (sociologically-derived) scientific insights that the members of the scientific community may be unable or unwilling to make themselves. A second bold claim is that making those contributions can help enhance the scientific and democratic legitimacy of science-based policy-making processes.

The approach I am advocating and pursuing is not entirely new. One of the earliest articulations of this approach was outlined by Chubin and Restivo in 1983, and described by them (with self-conscious irony) as a 'weak programme' in the sociology of science.³⁰ Numerous scholars have subsequently initiated work of this type and contributed to the tradition (see Appendix I). There is an honourable intellectual tradition of scientific connoisseurship which has engaged with important issues of public policy, and which

WHETHER THE ADVICE PROVIDED BY THE EXPERTS TO POLICY-MAKERS WAS QUANTITATIVE OR QUALITATIVE, POLICY-MAKERS AND THEIR ADVISORS AND ATTENDANT OFFICIALS ROUTINELY CHARACTERISED THE ADVICE AS 'SOUND SCIENCE', IMPLYING THAT IT WAS THE SCIENTIFIC RELIABILITY AND OBJECTIVITY OF THE ADVICE, AND OF THE ADVISORS, THAT LEGITIMATED THEIR POLICY JUDGEMENTS. A CRUCIAL STAGE IN OUR POLITICAL AND CULTURAL HISTORY WAS REACHED WHEN THE SOUNDNESS OF THE ADVICE AND THE SCIENCE ON WHICH IT PURPORTED TO BE BASED CAME TO BE SEEN AS LEGITIMATELY QUESTIONABLE.

31. Bloor D, 'Wittgenstein, Mannheim and the Sociology of Mathematics', *Studies in the History and Philosophy of Science*, 1973, vol 4, p 173-74; compare Bloor D, *Knowledge and Social Imagery*, p 4-5.

32. US National Research Council, *Science and Judgment in Risk Assessment*, Commission of the Life Sciences, 1994, Washington DC, p 31.

33. van Zwanenberg and Millstone, BSE: risk, science and governance (Oxford University Press, forthcoming), ch 4 'A new cattle disease'.

34. For example, US NRC, *Risk Assessment in the Federal Government: Managing the Process* (US National Academies Press 1983); Jasanoff S, *The Fifth Branch: Science Advisors as*

Policy-Makers (Harvard University Press 1990); Weingart P, 'Scientific expertise and political accountability: paradoxes of science in politics', *Science and Public Policy*, June 1999, p 151-61.



has utilised some tools of scholarly research to develop realist constructivist analyses of regulatory science.

The approach I am advocating contrasts markedly with what Bloor has taken to be a defining characteristic of his so-called 'strong programme' in the sociology of scientific knowledge. David Bloor argued that: "The aim of the sociology of knowledge is to explain how people's beliefs are brought about by the influences at work on them. This programme can be broken down into four requirements... of causality, impartiality, reflexivity and symmetry will be referred to as the strong programme of the sociology of knowledge."³¹ My analysis implies that while it may be methodologically appropriate to be impartial to the true value of regulatory scientific judgements in advance of an exploration of the processes by which they were socially constructed, it is often unnecessary and inappropriate, and often intellectually perverse, once the judgements have been deconstructed.

Institutional evolution of regulatory science

Regulatory science emerged as a distinct and novel phenomenon in response to the changing needs of policy-makers in industrialised countries in the period after the Second World War. In the post-1945 period, in the context of an expanding regulatory state, policy-makers found themselves increasingly under pressure to make decisions about the acceptability of newly emerging technologies, or in response to newly emerging evidence of risks from technologies that were already in use, especially in relation to matters of health, safety and the environment.

Policy-makers were poorly prepared and equipped to respond to those challenges. They could accept the general argument that some kinds of restrictions needed to be placed on the

products and processes of, for example, the food, chemical and pharmaceutical industries, but they had no basis for judging which products and processes needed to be restricted, nor the extent of those restrictions. To make those judgements, they turned for advice to selected scientific experts.

The scientific community was also ill-prepared for that role. Very few scientists understood how to make themselves and their knowledge useful to policy-makers, and scientific knowledge of what was, and was not, safe was very limited. By the mid-1950s, scientists were only able to identify materials that caused severe harm that occurred either rapidly or frequently, or both. The disciplines of epidemiology, toxicology and ecology were rudimentary and the available evidence was fragmentary, incomplete and equivocal. If asked, many scientists then would have said either "we do not know what is and is not safe" or "there are risks everywhere", but such advice was not much help to policy-makers.

A set of alliances then developed between a fraction of the scientific community that found that it could gain an increasingly important role in, and influence over, policy-making processes, and the officials and elected representatives responsible for administering those aspects of policy-making. One distinctive feature of these alliances was that they often coalesced, and continue to try to coalesce, around the assumption that there are thresholds of exposure to potentially hazardous materials which can be readily identified and below which adverse effects would not occur (eg latest WHO on Acrylamide). Scientists could help policy-makers by indicating quantitatively where those threshold levels might be.

Concepts such as 'threshold limit values', 'critical loads', 'acceptable daily intakes' and 'maximum residue levels' provide examples of

such forms of engagement. Once the benchmarks had been indicated, policy-makers could then take responsibility for deciding the most appropriate ways of ensuring that these designated levels were not often exceeded.

There were always problems with the assumption that the presumed thresholds were real and that they could accurately be identified. But the scientists who claimed to be able to identify them were providing policy-makers with what they wanted, and policy-makers chose for their expert advisors only those from the minority of scientists who then believed in thresholds, and expressed confidence in being able to identify them. As the US National Research Council indicated in the early 1990s: "The threshold hypothesis has been criticised as inadequate to account for some toxic effects, and it has not been accepted by [US] regulators as applicable to carcinogens, but it remains a cornerstone of other regulatory and public health assessments."³²

In the UK and in some continental European countries, prior to the mid-1990s, there was less focus on quantitative targets, but the adoption rather of the vocabulary of 'safe' or 'acceptably safe' using non-quantitative benchmarks such as 'as low as reasonably achievable' (ALARA), 'best available technology' (BAT), and even 'best available technology not entailing excessive cost' (BATNEEC). UK and European policies for chemical risks, especially in foodstuffs, were more often qualitative than quantitative but they were nonetheless predicated on the assumption that risks were negligible as long as exposures were acceptably low. Under these circumstances, the role of scientific advisory committees grew in importance, and they understandably became an object of inquiry for some sociologists of scientific knowledge and science policy scholars.

Whether the advice provided by the experts to

policy-makers was quantitative or qualitative, policy-makers and their advisors and attendant officials routinely characterised the advice as 'sound science', implying that it was the scientific reliability and objectivity of the advice, and of the advisors, that legitimated their policy judgements. A crucial stage in our political and cultural history was reached when the soundness of the advice and the science upon which it purported to be based came to be seen as legitimately questionable.

The general form of a research problem

Given that expert policy advisory committees were frequently provided with bodies of incomplete and equivocal evidence, and were frequently confronted by ambiguities and uncertainties, it was often puzzling that they nonetheless reached specific prescriptive policy conclusions. A puzzle confronting researchers was how the expert advisory committees had reached their eventual conclusions, given what was known about the limitations of the available evidence and knowledge? The form that many inquiries took was to search for evidence of implicit or unacknowledged assumptions that might account for the transition across the gulf between available evidence and eventual conclusions. These assumptions might have influenced the creation or gathering of evidence, the selection of evidence and its interpretation. In such circumstances, the methodologies of the sociology of scientific knowledge often seemed to provide promising ways to identify these assumptions.

Many researchers in the sociology of scientific knowledge and science policy studies forensically examined the social construction of regulatory science, and found that much of what was officially championed or designated as 'sound science' was in practice desperately shoddy sci-

ence, constructed on assumptions that embodied unacknowledged values and interests of special interest groups. Regulatory science was frequently found to be 'unsound'. What scholars found was that it was often, but not always, very uncertain and poorly constructed.

The contemporary policy context

Since March 1996 (3/96) when the BSE (bovine spongiform encephalopathy)-vCJD (variant-Creutzfeldt Jakob disease) crisis erupted into the public domain, British and European policy cultures have been confronted by a crisis of science and governance more severe and intense than the long-running challenges that faced it before. Prior to 3/96, science-based risk policy-making in the UK was routinely represented as if it were fully legitimated by 'sound science'. For example, in the ten years before 3/96 MAFF repeatedly asserted that British beef was securely and certainly known to be perfectly safe.³³ Once the UK government, at the prompting of the Spongiform Encephalopathy Advisory Committee (SEAC), acknowledged that cases of new-vCJD had emerged, that were probably attributable to the consumption of BSE-contaminated materials, assertions of fully scientific certainty lost what little credibility they might previously have had. The fact that science is less than fully sound substantially complicates the challenges facing policy-makers and their scientific advisors.

The USA had been compelled to acknowledge at least some of the uncertainties in science for policy since the mid-1970s after the political crises of the Vietnam War and the Watergate Scandal provoked Congress into legislating on freedom of information. Once the US Freedom of Information Acts (FoIA) of 1966, 1974 and 1976 compelled regulatory policy-makers to disclose a large fraction of the evidence upon

which their decisions were purportedly based, many of the previously undisclosed types of uncertainties became unavoidably conspicuous. The response of the US authorities, and of the community of advisory scientists, has been described and analysed in several competing ways.³⁴ The US authorities have endeavoured, with varying degrees of success, to manage policy-making processes by replacing the technocratic model with the so-called 'Red Book' binary model differentiated into 'risk assessment' and 'risk management', but those arrangements are being destabilised by clashes with the Europeans, and internal critiques and contradictions.

The US sociology of science community took far less advantage of the opportunities provided to it by the US Freedom of Information Acts than were taken by US corporate lawyers, but it remains the case that constructivist analyses of regulatory science in the US were far easier to deliver after the FoIA than before. Often it has been critical scientists in the USA rather than sociologists of science who have taken upon themselves the role of scientific connoisseur and public maven on controversial issues.

Whatever may have transpired in North America, the policy context for this discussion is focused primarily on developments in the UK and EU. The contemporary predicament of public policy-makers in the UK and in other EU Member States and at the European Commission can be illustrated by reference to the, probably apocryphal, story of a minister in the UK government during a private meeting with a professor who chaired an expert advisory committee. The minister asked the professor "Is it safe?" The professor responded by outlining that while, on the one hand, evidence from some studies suggested that there might be significant risks, on the other hand different studies indicated no risks whatever. The

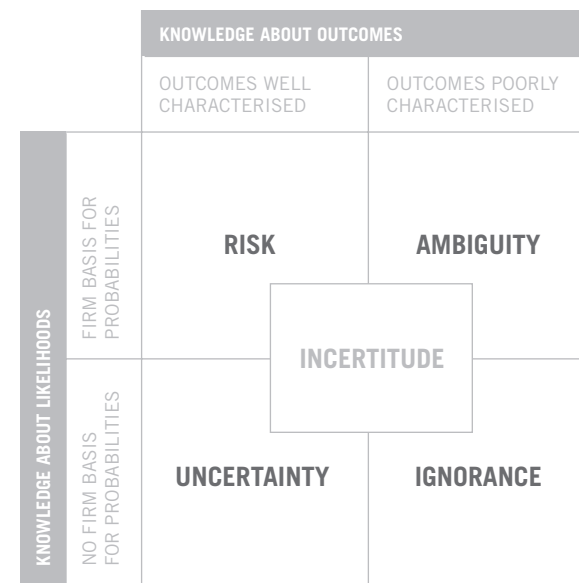


Figure 1. Stirling's matrix: distinguishing risk, ambiguity, uncertainty and ignorance

35. Rorty R, *Philosophy and the Mirror of Nature* (Princeton University Press 1979), p 176.

36. Robert Brandom, 'Truth and Assertibility', *The Journal of Philosophy*, Vol 73, No 6, 25 March 1976, p 137-49.

37. Rorty (n 36).

38. Millstone E, *Food Additives* (Penguin Books 1986).

39. Arnold D L et al, 'Saccharin: A Toxicological and Historical Perspective', *Toxicology*, Vol 27, Part 3-4, July-August 1983, p 179-256, especially p 181.

40. Arnold (n 40).



minister is alleged to have appealed to his officials: "Can no-one find me a one-handed scientist?"

In practice ministers, and policy-communities more generally, have to learn to manage risk appraisal and to take policy decisions in ways that are both scientifically and democratically legitimate while acknowledging the scientific uncertainties. Connoisseurs of regulatory science, using the analytical and investigative tools of the sociology of scientific knowledge, may not be able to provide ministers with one-handed scientists, but they can provide what might count as the next best alternative, namely empirically well-informed discriminating judgements about the relative reliability of the competing claims of different groups of scientists and of protagonists more generally. If sociologists of science can provide empirically well-grounded critical and discriminating judgements on science (and thereby in science too) and about the relationship of science to policy, they may be able to suggest how the legitimacy of policy-making could be enhanced, and illuminate particular policy debates.

One of very useful ways in which the tradition of work I am applauding has enriched our understanding of regulatory science has been by unpacking the concept of uncertainty. The realist anti-constructivist account assumed that there was just one kind of uncertainty, namely that type which can be diminished or even eradicated by conducting further scientific research. Scholars including Funtowicz, Ravetz, Wynne and Collingridge explored the limitations of that scientific account on uncertainty. Building on that tradition, Stirling has provided a graphic differentiation of four types of epistemological predicaments within which policy-makers and their expert advisors operate (see Figure 1). He contrasts the relatively straightforward circum-

stances in which we have a firm basis for characterising significant outcomes, and can reliably assign probabilities to them, with their contrary predicaments. 'Risk assessment' in its orthodox portrayal, in terms of 1. hazard identification, 2. hazard characterisation, 3. exposure assessment and 4. risk estimation is an activity that can be conducted in the upper left quadrant of Stirling's 2x2 matrix, but if either of those two conditions are not met then advisors are in the domains of uncertainty, ambiguity or ignorance, and 'risk assessment' cannot be pursued.

As Stirling has argued, expert advisors are often under considerable pressure to portray their predicament as unproblematically located in the top left-hand quadrant, when in reality they lack a firm basis for assigning probabilities and/or the outcomes they are considering are poorly characterised.

The condition of ambiguity was dramatically illustrated, for example, at the occasion when the results of the Farm Scale Trials of three varieties of GM crops were published at a meeting in November 2003 at the Royal Institution in London. The ecologists reporting their results explained that in trials with two of the three GM crops, significant reductions in weed and invertebrate populations were found, and to that extent adverse effects on the local ecology had been found. A farmer responded by insisting that the scientists had provided a perverse account of their data. To the extent that the fields in which GM crops were cultivated had fewer weeds and invertebrates they were less polluted and cleaner than the fields in which non-GM varieties had been cultivated.

As far as ignorance is concerned, it will always be difficult to circumscribe the extent of our ignorance, but in retrospect we can report on matters about which we were previously ignorant but now

have some understanding, eg vCJD and endocrine disruptive effects.

The tradition of work that this paper applauds has used the analytical and investigative methods of the sociology of scientific knowledge more accurately to characterise the epistemological predicaments in which regulatory science has developed, but having done so it has the advantage of not leaving policy-makers high and dry. By unpacking the ways in which regulatory scientific representations of risk have been constructed, and by highlighting the contrasting framing assumptions underlying those competing representations, scholars have provided insights into the quality of those competing representations, and those insights have been relevant to the policy and scientific debates, and more generally to scholarship and connoisseurship.

Methodology for connoisseurship of regulatory science

The methodology that I recognise in scholarly practice, and that I envisage being deployed more frequently, can be represented as a two-stage process; the first focusing on an examination of how regulatory scientific judgements have been constructed and the second on an exploration of the implications of the findings from the first stage for the epistemological status and reliability of those judgements.

The central objects of inquiry for this type of connoisseurship are the judgements of expert advisory bodies, and the first methodological stages concern the nature of the social processes by which those judgements were reached. Sociologists can, and often do, ask specifically about:

- Which experts were invited, involved and excluded?
- Which questions were asked and which avoided or discounted?

- Which bodies of evidence were included and which excluded?
- Which research topics and questions were commissioned and which avoided?
- Which data were collected and which omitted or discounted?
- Which data were revealed and which concealed?
- Which interpretations of the data were considered and which ignored or discounted?
- Which interpretations of the evidence were accepted and which rejected?
- Which uncertainties and data gaps were acknowledged and which ignored or discounted?
- Which policies were endorsed and which questioned or undermined?

Having asked and answered these questions, sociologists can, and I am arguing often should, go further to the second stage and ask what this information implies about the epistemological status of the judgements and advice of the experts. A methodology for doing so has often been employed but rarely described. An effective approach is firstly to establish what the evidential, conceptual and institutional conditions are for the 'warranted assertibility' of the conclusions and judgements of the expert groups, and then to examine the extent to which those conditions have been met. The concept of 'warranted assertibility' was introduced by the American philosopher John Dewey, but for a rather different purpose.³⁵ Once these questions have been answered it is often appropriate to ask whether the assertibility of other judgements would have been more warranted. To the extent that deficiencies in satisfying the conditions of warranted assertibility emerge, it is often appropriate to inquire why those assertions were made despite those deficits.

Ironically, the concept of 'warranted assertibility' was invoked by Richard Rorty and Robert Brandom to try to provide a philosophical account of how language could have both sense and reference without having to rely on the concept of 'truth conditions'.³⁶ Despite the fact that they were trying to use the concept of 'warranted assertibility' to try to sustain a radical relativistic form of epistemological scepticism (along the lines of 'truth is what my peers let me get away with saying'³⁷) it can be used (from a realist perspective) to provide a discriminating connoisseurial tool sceptically to critique a particular category of judgements.

In practice, we can explore the reliability of the scientific claims themselves, and also assertions or implicit signals concerning how the reliability of the scientific claims was represented, and of the representations of the relationship between the scientific judgements and non-scientific considerations. We can ask: what are the epistemological and institutional conditions for the warranted assertibility of those claims and, to the extent that deficiencies are found, which other claims would have been more warranted?

The next two sections of this paper summarise the outcome of inquiries of this type. They retrospectively report the findings of such research, but they do not report the chronological process by which those findings emerged. They have been chosen to highlight two kinds of shortcomings, that is to say, severe deficiencies in the conditions sufficient for warranted assertibility, namely quantitative and qualitative shoddiness. These two types of shoddiness will be discussed in turn, and the discussion illustrated with examples drawn from the regulatory field I know best, namely food and chemical toxicology.

Quantitative shoddiness

The case of saccharin

The USA Food and Drug Administration (FDA), the World Health Organisation and UN Food and Agriculture Organisation's Joint Expert Committee on Food Additives (JECFA) and the European Commission's Scientific Committee for Food have, from time to time, set acceptable daily intakes (ADIs) for saccharin that varied from 2.5 milligrammes per kilogramme of body weight (mg/kg bw) to 5 mg/kg bw in a manner that suggests that the ADI could reliably be identified with precision to at least one decimal place.³⁸ Despite the implication, the underlying science is in point of fact far too uncertain to justify judgements of that precision. These judgements have been very bizarrely constructed in ways that imply that they are not reliable.

Saccharin has been in use since the 1880s, although there have been doubts about its safety since 1890.³⁹ The central focus of the contemporary debate about toxic hazards from saccharin dates from the early 1970s and has concentrated on the issue of its putative carcinogenicity. To cut an extremely long story short, by the mid-1970s there was clear and consistent evidence from at least four rat feeding studies that, when saccharin is incorporated into the animals' diet there is a significant dose-related increase in bladder cancer rates among the males. The experimental evidence from laboratory animals is, however, equivocal, because not merely do male and female rats exhibit different patterns of response, such patterns also vary between different varieties of the rat species. Moreover when different species are compared, the varying patterns show even less consistency.⁴⁰

This lack of consistency in the patterns of toxicological action between species, varieties and

THE HISTORICAL EVOLUTION OF TOXICOLOGICAL DEBATES ABOUT SACCHARIN SUGGESTS THAT SCIENTIFIC BELIEFS ABOUT THE HAZARDS AND RISKS THAT CHEMICALS MIGHT POSE CAN SOMETIMES BE RUDIMENTARY AND CHRONICALLY IMPRECISE, AND CONSEQUENTLY THAT THEY HAVE OFTEN PROVIDED A POOR BASIS FOR POLICY DECISION-MAKING. THE HISTORICAL EVOLUTION OF SACCHARIN'S REGULATORY HISTORY SUGGESTS THAT DESPITE THE FACT THAT SCIENCE PROVIDED A VERY POOR BASIS FOR POLICY DECISION-MAKING, THAT POVERTY WAS FIRST CONCEALED AND THEN IGNORED OR DISCOUNTED.

41. For example Salsberg D, 'The Lifetime Feeding Study in Mice and Rats – An Examination of Its Validity as a Bioassay for Human Carcinogens', *Fundamental and Applied Toxicology*, 1983, Vol 3, p 63–67; Hoel DG, 'Statistical extrapolation methods for estimating risks from animal data', *Annals of New York Academy Sciences* 271: 418–20, 1976; Hoel DG, 'Extrapolation of laboratory data to human health effects', *Environmental Science Research*, Vol 25, 1982, p 521–26.

42. Ames BN et al. 'Carcinogens are Mutagens: A Simple Test System Combining Liver Homogenates for Activation and Bacteria for Detection', *Proceedings of the National Academy of Sciences*, 1973, Vol 70, p 2281f.

43. Arnold (n 40).

44. US National Academy of Sciences, *Saccharin: a technical assessment of risks and benefits: Part 1, Committee for a Study of Saccharin and Food Safety Policy*, 1978, US NAS, Washington DC.

45. WHO/FAO, *Summary of Evaluations Performed by the Joint FAO/WHO Expert Committee on Food Additives (JECFA)* (ILSI Press 1994), p S-1. JECFA also set a separate ADI for diabetics, at three times the level for all other consumers, despite having no more information about the toxicity of saccharin for diabetics than for other groups. JECFA took that step because available evidence indicated that diabetics were already exceeding the ADI set for the general public; a fact that was administratively untidy and politically problematic.

46. US Environmental Protection Agency, *Health Assessment for 2, 3, 7, 8-Tetrachlorodibenzo-p-Dioxin (TCDD) and Related Compounds*, Vols I to III, 1992.



genders of laboratory animals raises several critical issues that extend far beyond the particularities of the saccharin debate. One pivotal scientific question is which type(s) of animals, if any, provide suitable models to represent accurately the effects that chemicals and microbiological pathogens can have on people? My researches have indicated that that is a question which only a very few scientists and scholars have had the courage to address.⁴¹

Progress so far has been confined to debates about the scale of the poverty of extrapolative relevance of the animal species used in experiments to humans. A few studies have looked at the extent of the concordance between data from different animal species, and between the results of animal studies and data from *in vitro* studies using bacteria as the 'target' organism. Different questions, that a scientific connoisseur might pursue, include what does this imply for the epistemological status of toxicology as a discipline and for the reliability of toxicological advice to policy-makers?

Two other lines of potentially relevant inquiry into the putative toxicity of saccharin have also been actively pursued. Since there are good reasons for supposing that one route by which carcinogens can initiate or promote tumour growth is by causing a mutation (that is to say by damaging the genetic material in the nucleus of cells) it may be useful to test materials to see whether or not they cause mutations in bacteria and/or in tissue culture systems *in vitro*, that is to say in glass dishes. *In vitro* mutagenicity tests did yield some relevant information, but the emerging evidence has not been decisive, and could not have been decisive. Since some carcinogens may act through non-genotoxic routes, a compound might show no *in vitro* mutagenic activity, and yet be a genuine carcinogen.

Appropriate human epidemiological studies are difficult and expensive to conduct, and the evidence that they generate may also be inconclusive. It is, nonetheless, possible to conduct epidemiological studies to try to establish whether or not any correlation can be found between saccharin consumption and the incidence of cancers, not just in the bladders of human males, but perhaps at other sites too. There have been two main ways in which potentially pertinent epidemiological studies have been conducted. Firstly, some teams studied groups of male bladder cancer sufferers to try to establish whether they were more or less likely than otherwise comparable groups to have consumed saccharin. Alternatively, some teams studied diabetics to try to establish whether or not they had elevated rates of cancer of the bladder, since typically diabetics use more artificial sweeteners than other groups, and for most of the twentieth century that meant consuming saccharin. Since it is well known that compounds can cause cancer at one site in one species but at a different site in other species, a failure to discover significantly higher rates of saccharin consumption among adult human male bladder cancer sufferers would not be conclusive. On the other hand, even if there were a higher rate of cancer among diabetics, we could not be certain that it was the artificial sweeteners or even the saccharin that had been responsible, because it might have been causally related to the diabetes, but independent of saccharin consumption.

All those caveats notwithstanding, numerous epidemiological and bacterial mutagenicity tests have been conducted, but their combined results have been profoundly equivocal. Bacterial mutagenicity testing started in the early 1970s.⁴² The early test systems use salmonella bacteria cultured in a standardised growth medium, but by

the end of the 1980s a wide range of different types of bacteria, in a variety of growth media, were being used. When the putative mutagenicity of saccharin was investigated, it emerged that while saccharin was not evidently mutagenic in some bacteria under some conditions, it did appear to be mildly active in other bacteria under certain conditions.⁴³

More than 20 epidemiological studies on saccharin had been conducted by the early 1980s, but all of them on relatively small samples, using relatively poor control for possible confounders. While some of them found no significant correlation, a few of them did yield evidence of a possible link between saccharin consumption and bladder cancer.

Given the uncertainties and equivocation both within and between animal feeding studies, *in vitro* mutagenicity tests and human epidemiology, a bold attempt was made in the late 1970s by a US National Academy Sciences panel to estimate the upper and lower bounds of the risk that saccharin might pose to the US population. The panel estimated that if, on average, the population of the USA were to continue to ingest some 120 milligrams of saccharin daily for a period of 70 years (which corresponded to the average level of consumption in the USA in the early 1980s) it was unlikely that fewer than 0.22 extra deaths from bladder cancer would occur throughout the entire US population over that period, while on the other hand it was unlikely that more than 1,144,000 extra deaths would be caused.⁴⁴ In other words, estimates of the potential carcinogenicity of saccharin for humans are characterised by uncertainty of six orders of magnitude.

Despite that profound and chronic uncertainty official expert advisory committees in the USA, the UK, at the European Commission and at JECFA have continued to portray the available

evidence as if it could be interpreted unequivocally and unproblematically. Not merely have they been willing to ascribe a quantitative ADI, but on some occasions they have represented those figures as if they were accurate to at least a first decimal place.⁴⁵

In the case of saccharin, therefore, the judgements about the risks that saccharin might pose provided by the leading official expert advisors were very poorly warranted. Despite conspicuous uncertainties of at least five orders of magnitude, the regulatory bodies set ADIs defined to the first decimal place. There were gross deficiencies between the prevailing epistemological and evidential conditions on the one hand and the benchmarks that would have needed to be met, on the other, for those judgements and assertions to have been warranted. In most of those jurisdictions, and for most of the time, the judgements have corresponded to the most optimistic and forgiving interpretation of the available data. Ascribing an ADI implies, in any case, assuming that a threshold has reliably been identified below which significant adverse effects will not occur. In the case of saccharin, a threshold has never been established, but routinely and discretely assumed.

The reasons why such choices were made were almost entirely institutional; they were a product of the ambiguous objectives of the institutions and their relationships with other 'policy stakeholders'. Those institutional factors account for the otherwise inexplicably bizarre way in which studies and evidence were opportunistically selected or ignored, included or discounted.

A minority of the scientific community doubts the wisdom of ever ascribing an ADI to compounds that have been found to be carcinogenic in laboratory animals. While the evidence of animal carcinogenicity was uneven and equivocal,

saccharin did appear to increase tumour incidence at several sites in various varieties and species. A majority of the scientific community are uncomfortable with the assumption that any threshold can be ascribed to a genotoxic carcinogen, since the relevant theories of carcinogenicity imply that just one molecule of a genotoxic carcinogen could be sufficient to trigger tumour formation. In the case of saccharin, evidence of genotoxicity is present but partial and equivocal, and consequently a case can be made for the suggestion that even if it were a weak carcinogen, it could well act through non-genotoxic mechanisms. The judgements of the official advisory bodies were not demonstrably false, but they were very poorly warranted, and other rather different judgements would have been far more robustly warranted. The official bodies pretended that ADIs were natural constants that could be measured precisely, while the reality was that they were institutional, bureaucratic and industrial artefacts masquerading as if they were purely scientific.

The kinds of assertions that would have been more warranted would have provided far less precise, and far less reassuring, judgements. They would have acknowledged the scale of the uncertainties, and would have been far less sanguine about saccharin's biochemistry.

The report from the US NAS panel on saccharin was a remarkable document in several ways. It provided one of the only expert reviews of the toxicity of saccharin ever to have tried to review all the available studies it could identify, and ever to have exposed the complexities and ambiguities in the evidence, or to have estimated the magnitude of the resultant uncertainties.

The NAS panel's work was unique in another important respect. I am not aware of any subsequent attempt within food chemical toxicology to

estimate comprehensively the scale of the uncertainties, in relation to any other chemical compound. The EPA's review of dioxins was comprehensive, but aimed to quantify the magnitude of the risks rather than the magnitude of the uncertainties.⁴⁶ I find it striking and slightly puzzling that the NAS saccharin panel's type of exercise has never been repeated with any other compound, and has never been referred to in the reports about saccharin from the FDA, the CoT, the FACC, the SCF or JECFA. The anomalies have not been solved, and they have not gone away.

Moreover, if saccharin constitutes a special case it is only because an unprecedented effort was made quantitatively to estimate the scale and extent of the uncertainties that surrounded judgements about the toxicological consequences of its use. There are no prior or independent grounds for expecting saccharin's toxicology to be more complex or uncertain than those of other constituents of our diet or environment. The historical evolution of toxicological debates about saccharin suggests that scientific beliefs about the hazards and risks that chemicals might pose can sometimes be rudimentary and chronically imprecise, and consequently that they have often provided a poor basis for policy decision-making. The historical evolution of saccharin's regulatory history suggests that despite the fact that science provided a very poor basis for policy decision-making, that poverty was first concealed and then ignored or discounted.

The social, economic, political and scientific history of toxicology shows that toxicology is, to paraphrase Lord Kelvin, a 'meagre and unsatisfactory' branch of scientific inquiry. The sociological analysis of the construction of the judgements of official expert advisory bodies shows that they typically misrepresented the available knowledge as if it were more reliable

47. Millstone, 'Adverse reactions to food additives: the extent and severity of the problem', *Journal of Nutritional and Environmental Medicine*, Vol 7, No 4, 1997, p 323-32.

48. Juhlin L, 'Additives and chronic urticaria', *Annals of Allergy* November 1987, Vol 59, Part II, p 119.

49. Committee on Toxicity Report to the Food Advisory Committee, Final Report on the Review of the Colouring Matter in Food Regulations 1973, FdAC/REP/4, HMSO, 1987; Rowe KS, Briggs DR 'Food additives and behaviour', *Medical Journal of Australia*, 1994, Vol 161, Pt 10, p 581; Szatmari P et al, 'Ontario Child Health Study: prevalence of attention deficit disorder with hyperactivity', *Journal of Child Psychology and Psychiatry*, 1989, Vol 30, No 2, p 19-230; Schachter R et al, 'The characteristics of situationally and pervasively hyperactive children – implications for syndrome

education', *Journal of Child Psychology and Psychiatry*, 1981, Vol 22, p 375-92; Werry JS et al, 'Connors teacher questionnaire – norms and validity', *Australian and New Zealand Journal of Psychiatry*, 1976, Vol 10, p 257-62; Trites RL et al, 'Prevalence of hyperactivity', *Journal of Pediatric Psychology*, 1979, Vol 4, p 179-88.

50. Millstone E, 'Adverse reactions to food additives: the extent and severity of the problem', *Journal of Nutritional and Environmental Medicine*, Vol 7, No 4, 1997, p 323-32.

51. Joint Expert Committee on Food Additives, Toxicological evaluation of certain food additives and contaminants, WHO Food Additives Series 21 (World Health Organisation Geneva 1987), p 195; Committee on Toxicity report to the Food Advisory Committee, Final Report on the Review of the Colouring Matter in Food Regulations 1973 (FdAC/REP/4 HMSO 1987); *Journal of the Royal College of Physicians*, April 1984; Vol 18, No 2, p 83-123; Young E, et al, 'The prevalence of reaction to food additives in a survey population', *Journal of the Royal College of Physicians of London*, October 1987, Vol 21, No 4, p 241-47; 12th Report of the Scientific Committee for Food (EEC Commission Luxembourg 1981), EUR 7823.

52. Sharp C et al, FDA Searle Investigation Task Force, Final Report of Investigation of GD Searle Company, 24 March 1976, p 13-16.

53. *ibid* p 1-7 and p 13-16.

54. Bressler J et al, Establishment Investigation Endorsements of Searle Laboratories Division of GD Searle, Chicago, for the US FDA Bureau of Foods, 18 July 1977 and 7 August 1977; and FDA Bureau of Foods Task Force Memo, Authentication Review of Data in Reports Submitted to the FDA Concerning Aspartame, from the Bureau of Foods Task Force to Howard R Roberts,

Acting Director of Foods, 28 September 1977. Note that Roberts, the primary recipient of this paradoxical document, subsequently left the FDA and became the Director of the US Soft Drinks Association.

55. *ibid*, Bureau of Foods Task Force Memo, 28 September 1977, p 3.

56. Bressler (n 55) p 2.

57. *ibid* p 2-8.

58. *ibid* p 2-4.

59. Bureau of Foods Task Force Memo (n 55) 28 September 1977, p 3.

60. Bressler (n 55) p 4.



and precise than was actually the case. It also shows that they consistently provided optimistic assessments that served the interests of producers and were contrary to those of consumers. In this context, therefore, a form of scientific connoisseurship provides interesting information that could be important to many stakeholders and citizens and not just to fellow connoisseurs.

Shoddy estimates of the incidence of acute adverse reactions

Quantitative imprecision is a problem in many fields of regulatory science, not just in toxicology. To take another food-related example, policy-makers and their official expert advisors in the USA, the UK and at the European Commission have repeatedly argued that the incidence of acute adverse reactions to food additives, even among relatively vulnerable groups such as young children, are sufficiently low not to require any special measures, such as restrictions on the use of particular compounds in certain product categories or warning labels on certain types of products.

There is, nonetheless, evidence that a so far undetermined proportion of the population endures acute symptoms that are provoked and/or aggravated by exposure to some food additives including colours, preservatives and antioxidants.⁴⁷ There is an extensive debate about the scale of the problem, as well as the validity of much of the data and the conclusions to be drawn. Investigating the issue is difficult, in part because there are no animal models with which researchers can investigate problems of acute intolerance, nor are there any *in vitro* methods with which adverse reactions to food ingredients can be predicted, and therefore we have to rely on the results of human clinical and epidemiological studies which are difficult to conduct and interpret.⁴⁸

Reports of studies are available that indicate possible rates of adverse reactions ranging from as infrequent as 1 in 10,000 to as common as 1 in 5.⁴⁹ Not all these studies are equally relevant, robust and reliable, but several official expert advisory committees in the UK and EU have consistently represented the science in exceptionally optimistic ways, for example by highlighting the shortcomings of the studies that they choose to discount, while discounting the numerous shortcoming of the studies they highlight.⁵⁰

UK and CEC expert advisory committees, and representatives of the chemical and food processing firms, have tended to prefer data and conclusions based, for example, on studies conducted in groups from which the most vulnerable (such as those reporting allergic symptoms and/or all peoples aged in some cases less than 12 (or in others less than 20) were excluded, and from studies conducted in Scandinavia in the 1970s and early 1980s when only very few synthetic colours (which have frequently been cited as prominent triggers of adverse reactions) were permitted for use in food and drinks. The problem with these estimates of the prevalence of additive intolerance is that they have not been based on sufficiently representative samples of the relevant groups suffering from the full range of symptoms. Given the imprecision in the evidence from underlying studies, estimates ranging from 1 in 10,000 to 1 in 5 have been published, ie with uncertainty as great as four orders of magnitude. Nonetheless, the official advisory committees have frequently published estimates of the scale of the problem to the second decimal place.⁵¹

So, in relation to acute adverse reactions to food additives, we find that claims were made by official expert advisory bodies that substantially under-acknowledged the uncertainties, and made assertions that were poorly warranted by the avail-

able evidence. Moreover, different assertions for which there were fewer deficiencies in the conditions of their warranted assertibility were not made, but rather contradicted by those bodies. Once again, the reasons for the choices made were primarily institutional rather than scientific, in any strict sense.

The discussion of the empirical case study conforms to the methodological processes outlined earlier, and indicates how scientific connoisseurs can make some well-informed discriminating quantitative judgements about scientific assertions by regulatory scientific institutions and about the extent to which the conditions for their warranted assertion have been met; moreover they may contribute insights over and above those that the participating experts themselves may provide. In the following section, an example of qualitative shoddiness will be outlined. The case study refers to the main chemical competitor to saccharin, namely aspartame.

Qualitative shoddiness

Aspartame

The aspartame saga is considerably longer and more complex than the saccharin story, but in this context, only a few highlights can be outlined. Aspartame is an artificial sweetener formed by combining two amino acids, namely aspartic acid and phenylalanine. In the early 1970s, a US pharmaceutical company, GD Searle, which owned the patent on the chemical, decided to market it as an artificial sweetener. In July 1974, the Bureau of Foods at the US Food and Drugs Administration (FDA) announced that it was satisfied that aspartame was safe, and that it would shortly be allowed on to the US market. Subsequent work by the FDA's Drugs Division cast doubts on the safety of the chemical, and in

December 1974, the FDA announced that the imminent introduction of aspartame was being postponed, pending the results of an urgent investigation.

The FDA decided to investigate the testing and safety of aspartame after a Bureau of Drugs pathologist called Adrian Gross noticed a puzzling anomaly in a GD Searle submission on an anti-bacterial pharmaceutical product called Flagyl: the summary at the start of a document did not accurately reflect the detailed data presented in subsequent chapters. The report was returned by Gross to Searle with the expectation that the company would change the summary to fit the data. Gross was surprised when a fresh submission arrived in very short order with data altered to fit the summary.⁵²

Officials from the FDA's Drugs Division consequently made an unannounced visit to Searle's offices and laboratories, in the course of which questions were raised about the conduct and reporting of tests on the safety of aspartame and another pharmaceutical product called aldactone: as with Flagyl, the documents submitted by Searle did not accurately represent the conduct of the experiments which they were supposed to be reporting.⁵³ The FDA responded by establishing two task forces, one to concentrate on the pharmaceutical products and the other, within the Bureau of Foods, to concentrate on aspartame.

The Bureau of Foods Task Force identified 15 studies on the safety and toxicity of aspartame that it thought needed to be examined in detail to determine whether or not they had been properly conducted and reported. Senior FDA officials, however, claimed that the Agency only had sufficient resources to review just three of the 15 studies; it assigned the other 12 to be reviewed by an independent organisation called Universities Associated for Research and

Evaluation in Pathology Inc (UAREP), under contract to GD Searle. That decision was puzzling; the FDA had just received fresh funding, following Senate Committee hearings, to enable it to scrutinise toxicological dossiers, studies and data from the firms whose products it regulated. Furthermore, it was problematic to involve Searle in choosing the terms of reference for the UAREP investigation, and the UAREP did not possess the appropriate expertise to evaluate the conduct of animal experiments. Gross insisted that the main problem lay in the manner in which the studies had been conducted, yet UAREP was a professional organisation of pathologists whose expertise lay in the interpretation of tissue samples, not in the conduct of experiments with live animals.

The Bureau of Foods Task Force focused on three pivotal studies, two involving the possible effects of aspartame on reproduction in rats and mice (including possible embryo toxicity), and one examining the possible carcinogenicity in rats of a decomposition product of aspartame called diketopiperazine (or DKP).⁵⁴

The Task Force found "...significant deviations from acceptable procedures for conducting non-clinical laboratory studies..." in all three studies. Its detailed investigation (known collectively as the Bressler Report) emphasised that Searle's conclusions failed accurately to reflect the raw data generated in the laboratories.⁵⁵ In some cases, there were simply no data to back up the supposed results. In others, it was impossible to determine what were the original results and what were subsequent revisions or summaries. It was even impossible to identify from the laboratory records exactly when a particular animal had died. In one example, what were supposed to be observational records indicated that "...animal A23LM was alive at week 88, dead from week 92

through week 104, alive at week 108, and dead at week 112".⁵⁶ Nonetheless, Searle was not trying to market a compound with the power to revive corpses.

No fewer than 52 major discrepancies were found in the Searle submission on the DKP test alone.⁵⁷ For example, the Task Force was unable to establish how much DKP had actually been consumed by the rats. The FDA investigators found no fewer than three separate documents with different specifications for the content and purity of the test substance, and they were unable to establish precisely which specification, if any, correctly represented the material(s) used. It was impossible to reconcile the quantities of the chemical requisitioned from stores with the quantities supposedly fed to the animals. There was evidence indicating that the test substance had not been properly ground, and had been inadequately mixed, so that the animals may have avoided the DKP while eating their food.⁵⁸

However, although acknowledging these discrepancies, the Task Force concluded that they "...were not of such a magnitude that they would significantly alter the conclusions of the studies" – a finding that startled many observers.⁵⁹ Critics argued that, in reaching such a conclusion, the Task Force appeared to be repeating the same mistake for which it had criticised Searle, namely that its summary failed to reflect accurately the information contained in the reports upon which it was supposed to have been based.⁶⁰ As Dr Jacqueline Verrett, one of the members of the Task Force, subsequently explained:

"We were limited in what we could actually conclude about the studies. We were not allowed to comment on the validity of any study. It was an explicit instruction based on administrative rather than scientific considerations. We were supposed to figure out what the conclusions would have

61. Verrett J, statement to a hearing on 3 November 1987 of the US Senate Committee on Labor and Human Resources. See "NutraSweet" – Health and Safety Concerns, Hearing before the Committee Labor and Human Resources, 3 November 1987, US Senate, p 383-90.

62. UAREP 1978, Authentication Review of Selected Materials Submitted to the Food and Drug Administration Relative to Application of Searle Laboratories to Market Aspartame, Universities Associated for Research and Education in Pathology Inc, 18 November 1978.

63. Graves F, 'How Safe Is Your Diet Soft Drink?', Common Cause, July/August 1984, p 25-43; McCann JE, Sweet Success: How Nutrasweet Created a Billion Dollar Business, Business One, Irwin, Homewood, Illinois, 1990.

64. FDA, Aspartame: Decision of the Public Board of Inquiry, Department of Health and Human Services, Food and Drug Administration, Docket No 75F-0355, 30 September 1980.

65. Federal Register, Friday 24 July 1981, Part IV, Department of Health and Human Services, Food and Drug Administration, Aspartame: Commissioner's Final Decision, Docket No 75F-0355, p 38284-308.

66. US GAO (1986), Food and Drug Administration: Six Former HSS Employees' Involvement in Aspartame's Approval, US General Accounting Office, Briefing Report to Senator Howard Metzenbaum, Reference No GAO/HRD-86-109BR, July 1986.

67. Statement of Robert Shapiro, Chairman and Chief Executive Officer, Nutrasweet Company before US Senate Committee on Labor and Human Resources, in 'NutraSweet' (n 62) p 412-23.

68. Letter from Richard Merrill, Chief Counsel, Food and Drug Administration, Washington DC to Samuel Skinner, US Attorney, Chicago, 10 January 1977.

69. Dossier of documents released by Senator Howard Metzenbaum, Washington DC, US Senate, 6 February 1986.

70. Letter from Richard Merrill, Chief Counsel, Food and Drug Administration, Washington DC to Thomas Sullivan, US Attorney, Chicago, 20 July 1977.

71. Wurtman RJ (1988), ch 1, 8, 16 of Dietary Phenylalanine and Brain Function, ed RJ Wurtman and E Ritter-Walker (Birkhaeuser Boston 1988). Compare chapters in the same volume: Elsas LJ and Trotter JF, 'Changes in Physiological Concentrations of Blood Phenylalanine Produce Changes in Sensitive Parameters of Human Brain Function'; Matalon R et al, 'Aspartame Consumption in Normal Individuals and Carriers for Phenylketonuria'; Partridge WM, 'Phenylalanine Transport at the Human Blood-Brain Barrier'.

72. *ibid* Wurtman ch 1, 6 and 8; see also Wurtman R, 'Neurochemical changes following high-dose aspartame with dietary carbohydrates', *New England Journal of Medicine*, 18 August 1983, p 429-30; Wurtman R, 'Aspartame: possible effect on seizure susceptibility', *Lancet*, 9 November 1985, p 1060.

73. Olney J, 'Excitotoxic Food Additives – Relevance of Animal Studies to Human Safety', *Neurobehavioral Toxicology and Teratology*, 1984, Vol 6, p 455-62; Olney JW, Farber NB, Spitznagel E, Robins LN, 'Increasing brain tumor rates: Is there a link to aspartame?', *Journal of Neuropathology and Experimental Neurology*, 1996, Vol 55, p 1115-23.

74. Olney JW et al, 'Increasing brain tumor rates: is there a link to aspartame?', *Journal of Neuropathology and Experimental Neurology*, Vol 55, No 11, November 1996.

75. Two year toxicity study in the Rat: Final Report and Appendix, Hazelton Laboratories study number P-T 838H71, submitted to the FDA 25 January 1973, Master File numbers E-33 and E-34.

76. Shephard SE et al, 'Mutagenic activity of peptides and the artificial sweetener aspartame after nitrosation', *Food and Chemical Toxicology*, 1993, Vol 31, p 323-29.



been if the studies had been fully and correctly reported. We were obliged to ignore the protocols and the non-homogeneity of the DKP. The Bressler Report did show that non-homogeneity. Some animals did reject the DKP. Searle initially said that it may not have been fully mixed but that it did not matter: they later said that it had been fully mixed. We were not allowed to consider those issues by the Bureau of Foods administrator... We were hamstrung in being able to comment. The fact is that the studies should not have been considered at all, and that was the position from the beginning.⁷⁶¹

In 1978, UAREP delivered its 1062-page report, which concluded that the 12 studies it had audited were "authentic".⁶² The UAREP had, however, replicated the shortcomings of the FDA Task Force report: it concentrated on the interpretation of samples of tissue on microscope slides rather than on considering the procedures which had led to those particular tissues fixed being on those slides.

Despite the fact that these two reviews concluded that aspartame had been properly tested and that the substance was safe, a vocal lobby in the USA – including the Community Nutrition Institute led by James Turner and supported by Dr John Olney – was still not satisfied.⁶³ In 1979, in an attempt to resolve the controversy once and for all, the FDA set up a Public Board of Inquiry (PBOI) which presented its conclusions in October 1980.⁶⁴

The PBOI confined itself to examining two questions, both relating to aspartame's possible effects on the brain. It took the view that aspartame consumption would not pose an increased risk of brain damage resulting in mental retardation, but it concluded (by reference to data from two of the studies examined by UAREP) that it was unable to rule out the possibility that aspar-

tame could induce brain tumours. Consequently the Board recommended that aspartame should not be permitted for use, pending the results of further tests. The central issue in relation to brain tumours concerned the way in which the results of the experiments were interpreted. The results of at least one experiment were contentious because the incidence of tumours in the concurrent control group of animals was unusually high. If the test group had been compared with average historical control groups of the same type of animals, it would have indicated a statistically significant increase in cancers. This touches on a problem that affects many parts of toxicology. The degree of variability in the background incidence of pathological symptoms in laboratory animals is considerable, and there is an unresolved debate about whether significant comparisons should be with concurrent controls or with historical averages. It is not yet possible to decide who is right or wrong on that particular topic.

But the role of the PBOI was merely advisory, and it was the responsibility of the FDA's Commissioner, Arthur Hull Hayes Jr, to make a ruling. In July 1981, he announced his decision to approve the use of aspartame in food products other than soft drinks.⁶⁵ Hayes made it clear that he disagreed with the PBOI and that he was satisfied that aspartame did not cause brain tumours in laboratory animals.

Hayes subsequently resigned from the FDA, but only after approving the use of aspartame in soft drinks, and a very wide range of other products too. Two months later he became a senior scientific consultant to the public relations firm Burson-Marsteller, which provided consultancy services to GD Searle and NutraSweet.⁶⁶

In 1985, Searle was acquired by the US chemical company Monsanto, which separated the aspartame operation from the rest of its

activities and placed it under the NutraSweet Corporation. NutraSweet has repeatedly claimed that all the safety tests on aspartame were properly conducted, pointing out that no charges have ever been preferred.⁶⁷

The absence of charges, however, would appear to have been in spite of, rather than because of, the efforts of the legal staff of the FDA. In 1977, the FDA's Chief Counsel, Richard Merrill, instructed the US Federal Attorney in Chicago, Samuel Skinner (later to become Transportation Secretary and then Chief of Staff under President Bush Sr) to convene a Grand Jury investigation into Searle and three of its senior officers for their wilful and knowing failure to make reports to the FDA, concealing material facts, and making false statements in reports of animal studies conducted to establish the safety of the food additive, aspartame.⁶⁸

Merrill's contention was that many of the studies had been incompetently conducted and poorly managed, but that when the company had learnt of the incompetence, it chose to conceal information that was material to the FDA's deliberations.

Early in 1977, Searle's lawyers, Sidley and Austin, invited Skinner to join their firm. Skinner accepted and placed the aspartame case in the hands of subordinates, pending the appointment of a new Federal Attorney.⁶⁹ The case never met the deadline imposed by the US Statute of Limitations, despite repeated warnings from Richard Merrill at the FDA, and the indictments were never filed.⁷⁰

Since the original investigations, a number of other tests have been conducted on aspartame, many of which have provided results consistent with aspartame being innocuous. Others, notably the studies of Professor John Olney and Professor Richard Wurtman, have raised disturbing questions concerning aspartame's short-term toxicity,

particularly in relation to brain function.⁷¹ Wurtman has produced both clinical and theoretical evidence that high doses of aspartame may provoke epileptic seizures,⁷² while Olney has raised the possibility that aspartame may cause chronic brain damage, especially when consumed in combination with monosodium glutamate.⁷³ But despite such doubts over aspartame's safety, the three pivotal tests on long-term toxicity, reviewed by the FDA's Bureau of Foods Task Force, have never been repeated.

A particularly serious set of fresh doubts has recently emerged in a paper in the journal *Neuropathology and Experimental Neurology*, focusing on the possibility that aspartame might be contributing to the increasing incidence of brain cancer.⁷⁴

Professor John Olney of Washington University St Louis and his colleagues have based their hypothesis on several sets of considerations. First, they analysed the cancer statistics gathered by the US National Cancer Institute from catchment areas representing approximately ten per cent of the US population for the period since 1975. They found that the introduction of aspartame in the USA, into dry goods in 1981 and soft drinks in 1983, was followed by an abrupt increase (of approximately ten per cent) in the reported incidence of brain tumours. The change was most noticeable between 1984 and 1985, and it corresponded to approximately 1500 extra cases of brain cancer per year in the USA.

Their second main finding was that there has also been a marked change in the incidence of particular types of brain tumours, with a reduction in the proportion of a less aggressive (and often preliminary) type of tumour (astrocytomas) and a sharp increase in the incidence of a far more aggressive (and all too often terminal) type of tumour (glioblastomas).

The investigators argue that the reported changes in tumour incidence were unlikely to have been artefacts of improvements in diagnostic technologies. The introduction and rapid diffusion of computerised tomography in the early to mid-1970s, and of magnetic resonance imaging technology in the early to mid-1980s, certainly improved diagnostic precision. But they contend that the impact of those innovations upon the reported incidence of these central nervous system (CNS) tumours had worked their way through before aspartame was introduced.

Before these imaging technologies were introduced, it was far harder to diagnose brain cancer. Consequently, it was often not until tumours developed into glioblastomas that they were diagnosed, and a relatively high portion of tumours at the earlier astrocytoma stage went undetected. When the imaging technologies were introduced, brain tumours tended to be detected at the earlier stage, and consequently in the late 1970s the number of reported astrocytomas went up, while the number of glioblastomas exhibited a corresponding decline.

After aspartame was introduced, however, the opposite pattern can be found. The incidence of glioblastomas rose sharply, and starting in the late 1980s, the number of astrocytomas declined even more sharply. Since those latter changes ran counter to the direction that could be attributed to the introduction of better diagnostic technologies, it is hard to see how the reported changing tumour incidence could be ascribed solely to innovations in diagnosis. If the apparent increase in overall incidence had been due to improved diagnostics, then we should expect a marked change in post-diagnostic survival rates, but no such change was evident.

Olney and his colleagues suspect aspartame to be implicated in the aetiology of the extra cases of

brain cancer for three main reasons. First, the type of CNS tumour found to be increasing most rapidly in the USA is the same kind of lesion that was found in one of the animal studies conducted on aspartame in the 1970s.⁷⁵ Indeed, when the safety of aspartame was considered by a Public Board of Inquiry in 1980, it recommended against the approval of aspartame primarily because of a concern that aspartame appeared to be a brain carcinogen in rodents. A team of scientists at the US Food and Drug Administration concurred with the judgement of the Board, and they too recommended that further studies be conducted to clarify the issue before aspartame could be considered acceptably safe for use. Both the Public Board of Inquiry and the FDA staff scientists were, however, over-ruled by the incoming FDA Commissioner, Arthur Hull Hayes, who asserted that the brain cancer risk was minimal and that further research was not necessary.

Olney and his colleagues have also drawn attention to the results of a study by Shephard et al published in 1993.⁷⁶ Shephard and her colleagues attempted to simulate in vitro the conditions that can occur in the human digestive tract and in particular the conditions that result in the nitrosation of dietary ingredients. They reported that the nitrosated aspartame had significant mutagenic action. That evidence may be important because it suggests not only a mechanism through which aspartame might exert a carcinogenic action, but also why the interval between the compound's introduction and the elevation of brain cancer rates appears to have been so brief.

Olney and his colleagues also suggest that aspartame may reasonably be suspected of responsibility because the other main candidates for responsibility, such as ionising radiation, smoke inhalation, pesticides, electromagnetic

THE SOCIOLOGICAL AND HISTORICAL EVIDENCE ADDUCED ABOVE DEMONSTRATES THAT THE SCIENTIFIC EVIDENCE BY REFERENCE TO WHICH ASPARTAME WAS APPROVED WAS SO POORLY BASED AND CONSTRUCTED THAT NO RELIANCE CAN ANY LONGER BE PLACED UPON IT.

77. Collingridge and Reeve, *Science Speaks to Power*, 1986.

78. J Schwartz, 'Societal benefits of reducing lead exposure', *Environmental Research*, 1994, Vol 66, p 121.

79. Yule W and Rutter M, 'Effects of Lead on Children's Behavior And Cognitive Performance', ch 8 of *Dietary and environmental lead: human health effects*, ed Kathryn R Mahaffey (Elsevier Amsterdam 1985), p 212.

80. Millstone (1997), *Lead and Public Health*, ch 3, 'Earthscan'.

81. *ibid.*

82. Yule and Rutter (n 80) p 213.

83. Needleman H and Gatsonis C, 'Low-Level Lead Exposure and the IQ of Children, A Meta-analysis of Modern Studies', *Journal of the American Medical Association*, Vol 263, No 5, 2 Feb 1990, p 673-78.



fields and various other chemicals were introduced gradually over recent decades, rather than all at once in the early 1980s. Exposures to these potential hazards are occupationally linked, and it is hard to see how they could explain why males and females seem to be equally affected.

One way of checking Olney's hypothesis could be to analyse long-term brain cancer time-series data sets for other countries, covering the period both before and after the introduction of aspartame. If aspartame were to act by modifying already present or nascent brain tumours then we should expect that its impact would vary across countries in ways that depended on the age structure of the population of aspartame consumers. Anecdotal evidence suggests that a far larger proportion of 50 to 70 year old US citizens consumed aspartame-sweetened products in the 1980s and 1990s than was the case in the UK or in many other European countries. Consequently, the patterns allegedly found in the USA by Olney et al might not be reproduced in Europe. An alternative line of investigation might involve conducting new long-term animal feeding studies, but their relevance to humans is inevitably arguable.

Detailed dossiers outlining all these concerns have been provided to key national and international bodies, especially in 1990 and 1996, but so far none of the 'competent authorities' in the USA, the UK, at the European Commission or at JECFA has directly addressed the issues outlined. They have discounted them as unsubstantiated allegations or as simply false, but without supporting evidence. One obvious explanation is that these institutions cannot cope with admitting that they ever accepted at face value evidence that was fundamentally spurious and profoundly unreliable. That particular Pandora's box is one of several that they have been distinctly reluctant

to open.

The sociological and historical evidence adduced above demonstrates that the scientific evidence by reference to which aspartame was approved was so poorly based and constructed that no reliance can any longer be placed upon it. It shows, moreover, that the institutional processes by which the acceptability of aspartame was assessed and evaluated were deeply flawed. None of the institutions has properly addressed or answered the allegations of irregularity in the conduct of the original tests. They have merely asserted that the available scientific evidence is sufficient to show conclusively that aspartame is safe. They have never cited, and apparently cannot cite, any subsequent laboratory or epidemiological studies that cover the disputed territory and which make up for the inadequacies revealed by Gross, Verrett, Bressler, and Olney et al.

The repeated assertions from the FDA, MAFF, the CoT, the FAC, the SCF and JECFA, to the effect that aspartame has been shown to be entirely safe, were not warranted by the available evidence when they were made, and are not warranted now. The evidence that was, and is, available warrants profoundly different assertions. Given what we now know about the ways in which aspartame was tested, the ways in which the data were interpreted, and the processes by which it came to be approved, to suggest that we should be entirely impartial to all and any claims about the safety of aspartame seems bizarre and perverse. The socio-political information we have about flaws in the testing and approval of aspartame indicate that official reassurances have been at best premature.

Interesting though these examples might be, there is a risk that the impression might have been given that disclosing the social processes

underlying the construction of regulatory science invariably shows the institutional assertions to be poorly warranted and frequently over-optimistic, while assertions of profound uncertainty would have been more fully warranted.

To overcome that risk, one more example will be provided to illustrate the point that scrutinising the social construction of regulatory science does not always reveal substantial unacknowledged uncertainties. I will outline the evolution of assertions about the putative toxicity of metallic lead, and of lead-based compounds, to provide a counterpoint to the previous examples. In the case of the neurotoxicity of lead, a sociological and scientific analysis of the evolution of regulatory science and expert judgements, especially in the USA, shows that a key part of the science changed over time by becoming significantly less uncertain and less imprecise than had previously been the case. Moreover, public policy has shifted in ways that reflected the evolution in scientific knowledge.

In their challenging analysis of the role of science in public policy, Collingridge and Reeve argued in 1986 that the closer science came to policy-making institutions (as it mutates from 'normal' academic science to 'regulatory science' forged in and for the policy-making arena) the more it is prone to becoming increasingly uncertain, because of the 'over-critical' contested arenas in which scientists are then obliged to operate. They illustrated their argument by reference to the debate about the toxicity of lead, claiming that since the 1970s, when an argument had erupted over the putative toxicity of low-level lead exposure, the dispute had polarised the debate and thereby provoked greater dissent than had previously been the case, or would otherwise have obtained, with the effect of increasing the uncertainties.⁷⁷ One of my con-

tentions is that their judgement was premature and unduly pessimistic. Since the mid-1980s, the science of lead neurotoxicity has become increasingly robust and certain; in key areas those uncertainties did not increase, they diminished.

Lead and children's health

One of America's leading public health experts, Joel Schwartz, has argued convincingly that: "More is known about the health effects of lead than about those of any other environmental pollutant."⁷⁸ The following discussion outlines one part of the toxicological debate about lead, namely that concerning child neurotoxicity.

In the context of lead toxicology, a standard indicator of body lead loads is the level of lead circulating in the blood stream. Blood lead levels are usually measured in terms of the number of micrograms (μg) of lead per 100 millilitres (mls) of blood; and 100 mls is usually abbreviated as a decilitre (or dL), because it amounts to one tenth of a litre. Until the 1970s it was generally assumed that increased lead levels were of little clinical importance if there were no overt signs of poisoning, and if PbB levels were below 50 or 60 $\mu\text{g}/\text{dL}$.⁷⁹ During the last thirty odd years a heated debate has ensued over the existence and severity of adverse neurological effects of lead, primarily in infants and children with blood lead levels below 50 $\mu\text{g}/\text{dL}$.

To cut a very long story as short as possible, the sophistication of ways of studying the problem has strengthened, in part because of the policy-sensitivity of the issue, and investigators have developed increasingly sensitive, specific and precise ways of estimating both body lead loads and putative adverse neurological effects. There were moreover public and non-commercial bodies in both the USA and Australia willing and able to fund a sustained programme of research into lead

and public health. If the US National Institutes of Health had not invested public resources into active epidemiological research over a sustained period of time, the story I might be outlining would have been radically different.

Some of the earlier studies had used relatively crude IQ tests to estimate neurological performance and then tried to draw inter-group comparisons in ways that came, rightly to be seen as problematic.⁸⁰ Over time, more varied and sophisticated methods to test neurological performances were developed and deployed, and greater care came to be taken to emphasise intra-group comparisons rather than those across social group differences. Increasing care and diligence were also shown in the ways in which leading investigators endeavoured to identify, and adjust for, confounding factors. Not only did methods of data collection gain in sophistication, so too did methods of data analysis.⁸¹

The earliest studies were clinical studies of small groups of children with high blood lead levels. To try to explore possible links between lead contamination and neurological performance, at least nine different teams of clinicians studied children they deemed to be mentally retarded or behaviourally deviant to try to establish the extent to which lead might be responsible for some of their problems. Studies conducted during the 1970s suggested that in cases where lead levels were consistently raised above 60 $\mu\text{g}/\text{dL}$, a decline of three to four IQ points could be detected, even among apparently asymptomatic children.⁸² At that stage, however, the evidence of an adverse effect was much less clear in the PbB range of 40-60 $\mu\text{g}/\text{dL}$, in part because they studied very few children with blood lead levels at or below 50 $\mu\text{g}/\text{dL}$, but there were some indications of an effect. Those early studies were unfortunately unreliable, partly because they

used very small samples, and partly because of a lack of statistical controls for the confounding effects of physical and social characteristics of the children's backgrounds. But by 1980, evidence had accumulated to indicate that lead was capable of causing impaired neurological functioning at blood lead levels below those which had traditionally been associated with clinical lead poisoning. During the intervening 24 years, however, research methods have improved to the point where adverse effects are now being demonstrated in children who are not even suspected of being lead-poisoned.

By the mid-1970s it had become increasingly clear, and widely recognised, that studies of the neurotoxicity of lead would only provide clear results if several conditions were satisfied. First, population samples had to be sufficiently large to permit the detection of marginal effects, secondly care needed to be taken to estimate and adjust for confounding factors, and thirdly which was the direction of causation? Did lead poisoning damage children's brain functions, or were less bright children more likely to be ingesting lead-contaminated material, such as domestic dust?

The first two of these issues were addressed in a cluster of over 20 separate general population cross-sectional studies, while the third condition was addressed only later (during the late 1980s and 1990s) in a subsequent set of five prospective longitudinal general population studies.

General population cross-sectional studies

Between 1972 and 1990 a total of 24 cross-sectional studies on the effects of lead-exposure on children's performance in IQ tests were published.⁸³ Several of them provided consistent evidence indicating that lead, at relatively low levels of exposure, can exert adverse effects on performance in neuro-psychological tests. A few

Figure 2. Meta-analysis of 10 cross-sectional studies on lead and children's health

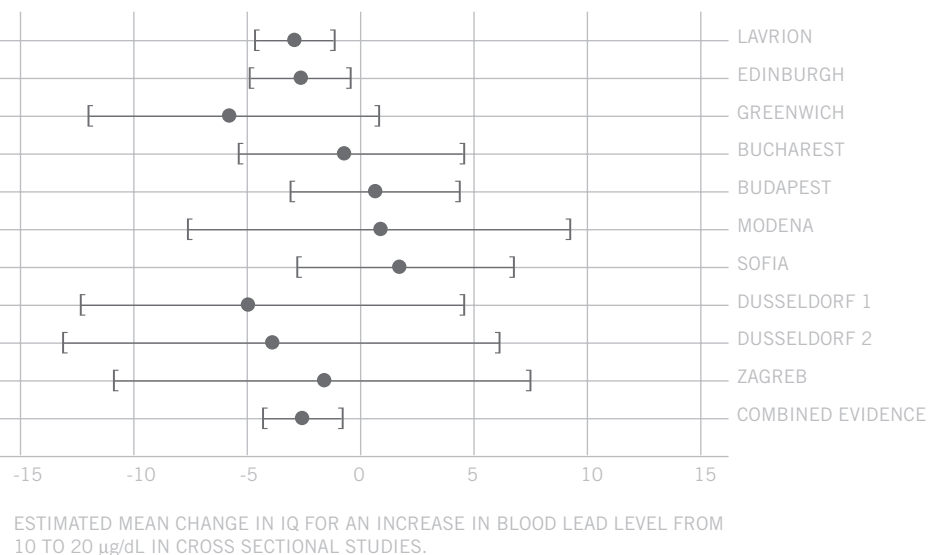
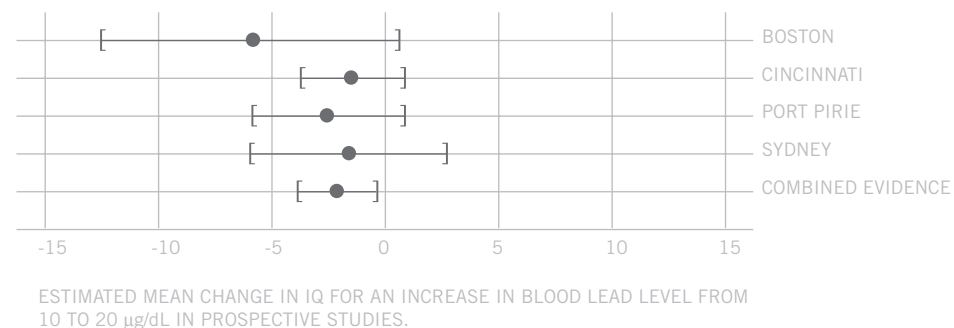


Figure 3. Meta-analysis of mean blood lead and full-scale IQ (mean changes and 95% confidence intervals)



studies produced equivocal results and some were negative. By the end of the 1980s, more than 20 studies of the relationship between childhood body lead loads and neuropsychological functioning had been conducted. Some of their results were strongly suggestive of adverse effects significantly below blood lead levels of 30 µg/dL, but the results of others were equivocal and inconclusive. Scientists disagreed about what the weight of evidence indicated overall.

A key development occurred in 1990 when, for the first time, the relatively new technique of what statisticians call 'meta-analysis' was applied to judge the weight of evidence in a systematic fashion. In a meta-analysis the summary results of a multiplicity of separate studies can, under certain conditions, be pooled together. The meta-analysis in effect treats each individual study as if it were providing a set of data points in a larger meta-study. The key advantage of a meta-analysis is that it enables the size of the population sample in the meta-study to be increased, in effect, to the sum of the sizes of the individual samples in the separate studies. Needleman and Gatsonis conducted the first comprehensive meta-analysis of all the studies that had used sufficiently similar protocols and variables to enable them to be so combined. They concluded in 1990 that: "The overall evidence... establishes a strong link between low-dose lead exposure and intellectual deficit in children."⁸⁴ In that context, 'low-dose' exposures were blood lead levels below 20 µg/dL, but also at or below 10 µg/dL.

Two other meta-analyses were subsequently conducted, one confirming Needleman and Gatsonis' conclusions and the other not doing so. The meta-analysis that suggested no evidence of adverse effects below 40 µg/dL was funded by the lead industry and excluded several of the studies that the other two independent meta-

analytical teams had deemed worthy of inclusion.

The WHO's International Programme on Chemical Safety summarised its meta-analysis with the graphic illustration represented by Figure 2.

In response to these results, the Lead Industries Association argued that none of these cross-sectional studies demonstrated that the link was a causal one; it remained possible that the correlation was purely coincidental or even a product of reverse causality. They demanded an answer to the question: "Was lead damaging the children's mental performance, or were the children absorbing more lead because they were not particularly smart?"

Was the link a causal one?

Cross-sectional studies, on their own, could never resolve the issue of causation, but as several protagonists insisted, the hypothesis of a causal link was strongly supported by several relevant considerations. First there was biochemical evidence, primarily from animal models, indicating well-defined mechanisms that explained the effects that had been detected. Secondly there was no evidence that any single social, economic or nutritional confounding factors or set of confounders could account for the evidence of neurological deficits. Thirdly, the adverse effects of lead exposure had been demonstrated consistently in numerous studies under similar circumstances. Those considerations make a causal link more likely, but they could not demonstrate a link. In the 1980s, the research and public health community, especially in the USA and Australia, increasingly came to acknowledge that the most effective way of establishing causality would be to conduct 'prospective longitudinal' studies, namely studies that would follow children as they developed, to see whether

correlations could be found between body lead loads and subsequent performances and developments. The question therefore became: "Could evidence be found that body lead loads are an antecedent cause of their later performance?"

General population prospective and longitudinal studies

A conference in Cincinnati in 1981 debated the design and conduct of studies of a possible causal link between body lead loads and children's mental development.⁸⁵ A consensus was reached that problems concerning the identification and measurement of socio-economic and nutritional covariates and confounders, as well as better documentation of the lead exposure variable and exposure histories, could best be addressed by undertaking several prospective studies simultaneously. Subsequently, five major prospective research projects were initiated, and in some cases are still continuing, in Boston, Cincinnati and Cleveland in the USA and in Port Pirie and Sydney in Australia.

During the course of these longitudinal studies, neuropsychological performances have been tested at regular intervals, as the children have developed through several quite distinct stages. The neuropsychological performance tests could not be thought of as if they were measuring a single property or constant characteristic as children grow up. It almost goes without saying that a six-month-old baby can do quite different things from the actions characteristic of a two-year-old child. Researchers sought to develop some standardised and normalised scale with which to estimate how children are developing by comparison with the average for their age, but they were not tracking the development of one single property.

In all five major prospective studies body lead

loads were estimated by measuring blood lead levels, although in Cleveland Ohio teeth were also collected and analysed. In all the studies, samples of blood were taken from mothers prior to the birth of their children (prenatal samples), from new-born babies (postnatal samples) and from children. Umbilical cord blood samples were often also taken.

When viewed as a whole, the balance of evidence emerging from the five prospective studies indicates that, other things being equal, children with higher blood lead levels perform less well in neuropsychological tests than children with lower PbB levels. There was no evidence of a threshold below which these adverse effects ceased to occur. As time has passed, the evidence of both a correlation and a causal link has become stronger, and the levels of exposure down to which the effects have been detected have also declined steadily.

In 1995 the World Health Organisation published a long-awaited report on lead.⁸⁶ When preparing its report, the WHO working party conducted a meta-analysis on the five prospective longitudinal studies. The working party would have liked to have been able to pool the data from all five of the studies, but the team in Cleveland did not publish their data in a way that allowed their results to be included; consequently only the data from the other four could be pooled. The WHO-IPCS team calculated the likely adverse impact on IQ scores which would result from an increase in blood lead levels from 10 µg/dL to 20 µg/dL.⁸⁷ They provided, moreover, a graphic illustration of the results from each of the four studies, and a pooled analysis, which is reproduced in Figure 3.

A noticeable feature of Figure III is that it suggests that none of the studies on their own provided a definitive result – in each case the 95

84. *ibid.*

85. International Lead Zinc Research Organisation, Comments on Health and Toxicology Sections of the OECD Lead Document, August 1991, Appendix I, p 7.

86. World Health Organisation's International Programme on Chemical Safety, Inorganic Lead, Environmental Health Criteria 165, Geneva, 1995.

87. *ibid* p 181, table 21.

88. Canfield et al, 'Intellectual Impairment in Children with Blood Lead Concentrations below 10 µg per Deciliter', New England Journal of Medicine, Vol 348, No 16, 17 April 2003, p 1517–26.

89. Bellinger & Needleman, New England Journal of Medicine, Vol 349, No 5, 31 July 2003, p 500.

90. Hill AB, 1965, 'The Environment and Disease: Association or Causation?', Proceedings of the Royal Society of Medicine, Vol 58, p 295–300.

per cent confidence interval intersected with the vertical axis. That meant that in 1995 there was at least a five per cent chance that each positive result indicating neurotoxicity was a random artefact. When the data from the four separate studies were pooled, however, and meta-analysed, the larger effective sample size produced a distinctly more robust conclusion. The line that represents the combined result from all four of the studies provides clear evidence that an increase in PbB levels from ten to 20 µg/dL results in a deficit of approximately two IQ points, and that there is less than a five per cent chance that the conclusion has been reached accidentally. While the conclusion may fall short of that required to produce total certainty or consensus, the evidence is strong enough to satisfy all but the most perverse and recalcitrant members of the scientific community.

As recently as April 2003, Canfield et al reported the latest findings of the Cincinnati-based study in which they concluded that blood lead concentrations were inversely and significantly associated with IQ scores, and that each increase of 10 µg/dL in the lifetime average blood lead concentration was associated with a 4.6-point decrease in IQ scores (with P=0.004, ie less than a 1 in 250 chance that the result was a random finding). And that as lifetime average blood lead concentrations increased from 1 to 10 µg/dL, there was on average a 7.4 per cent deterioration in neurological performance, as represented by IQ scores.⁸⁸ In July 2003 the team responsible for the Boston-based study confirmed that they too had found an inverse relationship between blood lead levels and neurological performance below 5 µg/dL, although "...the precise shape of the dose-effect relationship below 10 µg/dL remains uncertain".⁸⁹

The history of our understanding of the subtle

effects of lead pollution at relatively low level of exposure has provided a striking counterpoint to the previous examples taken from the domain of food additives. In the previous examples, the evidence showed that the phenomena were far less certain and robustly understood than the official portrayals by the regulatory bodies suggested. In the example of lead, however, the evidence indicates that a detailed knowledge of the ways in which representations of lead toxicity have been constructed enables us to appreciate that that particular area of scientific knowledge has become increasingly secure, reliable and precise, and many of the assertions made by regulatory scientific institutions have become increasingly warranted.

The evidence of a link between body lead loads and neurotoxicological harm satisfies at least eight out of nine of the Hill criteria that can be used to distinguish between accidental associations and causal relationships in epidemiological studies.⁹⁰ Hill's criteria are:

1. Strength of evidence.
2. Consistency of evidence.
3. Specificity of effect.
4. Temporality of the effect.
5. Dose-response of the effect.
6. Plausibility of the effect.
7. Coherence with existing knowledge.
8. Experimental evidence.
9. Analogy (structural activity).

The only reason why criterion 9 is not satisfied, in this particular case, is because the knowledge is direct and not mediated by structure-activity analogies. Criterion 9 is therefore irrelevant.

As those conditions have increasingly come to be satisfied, and to be satisfied more comprehensively, assertions that lead contamination exerts adverse effects on children's neurological performance at blood lead levels below 30, then

IF POLICY-MAKERS ABANDONED THEIR TRADITIONAL PRACTICE OF HIDING (OR TRYING TO HIDE) BEHIND THEIR EXPERT ADVISORS, AND USING THEM AS THEIR SHIELDS, THEN EXPLICIT RESPONSIBILITY COULD BE TAKEN FOR UPSTREAM FRAMING GUIDANCE AND FOR DOWNSTREAM CHOICES ABOUT TRADE-OFFS AND LEVELS OF ACCEPTABLE RISK AND UNCERTAINTY. UNDER THOSE CONDITIONS, THE DEMOCRATIC LEGITIMACY OF POLICY-MAKING COULD BE ENHANCED.

Figure 4. The co-evolutionary model of science in policy-making: reciprocal links between science and policy



25 and then 10 µg/dL have become increasingly warranted. Once again, suspension of judgement would be inappropriate, and it becomes evident that some assertions are far better warranted than others.

Summary and interpretation

Several conclusions can be drawn from this analytical framework and empirical examples. First, well informed connoisseurial judgements about the quality of regulatory science and institutional judgements can be warranted by drawing on the findings of studies into their construction, using the investigative methods of the sociology of science, just as long as neither anti-realist nor anti-constructivist assumptions are made.

Secondly, prior to those investigations, it may well be important for scholars to be impartial to the truth value of the scientific assertions to be deconstructed, but once these investigations have been conducted, impartiality *ex post* is often inappropriate and can even be bizarre or perverse. Using the methodology outlined above enables more discriminating judgements to be made than would otherwise be the case, and these connoisseurial judgements can themselves be warranted, if they are well-grounded conceptually and empirically.

Thirdly, I have indicated how social historians (and historically-oriented sociologists) of regulatory toxicology have shown, by reference to both technical and institutional considerations, that some parts of regulatory science are at very rudimentary stages of their intellectual development. I have also indicated how much evidence there is in favour of the assertion that many knowledge claims in regulatory science are not yet fit for the purposes many would wish them to be able to serve. The extent to which the intellectual poverty of those parts of regulatory science is recognised

varies very widely. Ironically, some of those who are best informed are among those who are most reluctant to acknowledge their limitations.

Fourthly, that account implies that a scientific connoisseurship of that sort can make a valuable contribution by helping to articulate proposals for research programmes and projects that could contribute to diminishing some of the important policy-sensitive uncertainties. Since some of those best placed to recognise uncertainties may have powerful reasons for being reluctant to acknowledge those uncertainties publicly, scientific connoisseurs may have an important contribution to make to public research policy-making. If the uncertainties are to be diminished, and if timely and effective action is to be taken to try systematically to diminish those uncertainties, and if public R&D funds are to be well-used, then sociologically-trained connoisseurs of science may have a valuable contribution to make.

In 1999, Peter Weingart argued that, while the inadequacies of the technocratic and decisionist models of science-based public policy-making are widely appreciated, no adequate alternative model was yet available. The evidence and analysis provided by the tradition I have applauded does provide the kind of alternative for which Weingart has called. It has, appropriately, been referred to as a 'co-evolutionary model' of science in policy-making; it is graphically represented by Figure 4.

The key features distinguishing this model are that it represents regulatory scientific deliberations as located in particular contexts, with both policy and broader social dimensions. The model assumes that those contexts can affect the content and conclusions of those deliberations. Representations of risks are assumed to be hybrid judgements constructed out of both scientific and non-scientific considerations, even if they

may be presented as if they were purely scientific.

Scientific deliberations are represented as playing a pivotal role in the overall process of risk appraisal and decision-making. But rather than pretending that those deliberations are disconnected from, and independent of, their social and policy context, they are portrayed as sandwiched between a set of upstream considerations about the scope of, and agenda for, scientific deliberations and a discrete set of downstream considerations focusing for example on trading-off risks against anticipated benefits. The ways in which the policy contexts frame the scientific and advisory deliberations can then be acknowledged, rather than concealed, and conducted in accountable and legitimate ways.

If that model provides a more accurate representation of the role that science has played, and does play, in policy-making than its predecessors, then the clear implication is that a regime structured and operated in accordance with that model could and should be implemented explicitly and accountably, rather than continuing with the *anciens régimes* that misrepresented policy-making processes either technocratically or by trying to invoke an unproblematic separation of 'risk assessment' and 'risk management'.

Instead of structuring policy-making institutions and procedures as if they were grounded in unproblematically objective, reliable and adequately sound science, scientific deliberations about risks should be explicitly framed by a set of 'risk assessment policy' guidelines, as they are increasingly coming to be called. These guidelines could and should be set by democratically accountable policy-makers, in the light of advice from, and debate with and among, a broad range of stakeholders. When, for example, the European Parliament revised the 1990 Directive on the Deliberate Release into the Environment of Genetically

Modified Organisms (90/220) by agreeing in 2001 its replacement (2001/18) it stipulated in effect that a scientific assessment of the risks of the deliberate release into the environment of GM crops would only comply with the revised Directive if long-term and indirect effects are assessed as well as direct and short-term effects.

Expert advisors could and should be provided by policy-makers with an indication of the range of policy options available and under consideration so that the experts can provide well-informed judgements about what is known and not known (within the scope of the framing guidance) about the consequences of following or failing to follow particular policy options. If the advisors were to provide downstream policy-makers with what Stirling usefully calls 'plural and conditional' advice rather than 'monolithic and prescriptive' advice (of the type that was traditionally expected and provided), then the practice of allowing policy-judgements to masquerade as if they were based only on sound science would no longer be sustainable.

If, moreover, a freedom of information regime prevailed, in combination with the conditions specified above, we could expect that the kinds of judgements expert advisors would provide would become more warranted than were the judgements of their predecessors. The scientific legitimacy of the policy-making process could thereby be enhanced.

If policy-makers abandoned their traditional practice of hiding (or trying to hide) behind their expert advisors, and using them as their shields, then explicit responsibility could be taken for upstream framing guidance and for downstream choices about trade-offs and levels of acceptable risks and uncertainty. Under those conditions, the democratic legitimacy of policy-making could be enhanced. But under those conditions

government ministers would have to take overt responsibility for difficult choices, and as Sir Humphrey Appleby knew only too well, elected politicians are often timid about taking responsibility for difficult decisions. But what else are they being paid for?

The invitation to this meeting asked *inter alia*: "...has connoisseurship led or lagged behind social debate on the issues? What forms of social analysis already embody (at least elements of) scientific connoisseurship in action? What effects might fully developed scientific connoisseurship have on the forms, participants, processes and outcomes of modes of public engagement?"

Would the individual scientific connoisseur as traditionally defined be able to deal with the potential criticism that judgements that were becoming more transparent in their basis and more widely social shared were, through connoisseurship, becoming the more opaque property of a particular elite? Or would it be possible to create a broader mass social connoisseurship movement, in which the connoisseurs proper might be mavens – discriminating collectors and energetic disseminators of what might be broadly termed 'consumer information' – bolstered by connectors and salesmen?"

My response is that in some cases scientific connoisseurs have led social debates, and that sociological deconstructions of regulatory science have provided a valuable methodology for a form of scientific connoisseurship that is of quite general interest, and not merely of interest to other scholars or connoisseurs.

As mavens, scholars have been able to mediate between a relatively closed and exclusive secular brotherhood/priesthood and the general public. It is, however, important to acknowledge that not all who claim to serve as mavens are equally reliable.

One such organisation is called Sense about Science. The chair of that organisation, Lord Dick Taverne, recently said: "the Food Standards Agency is an objective body set up by the government to safeguard and protect the consumer..."⁹² Those remarks were curious, because although Taverne implicitly acknowledged the importance of institutional factors, he nonetheless portrayed them as if they were entirely unproblematic. In practice, it takes more than calling an institution 'objective' to warrant an assertion that objectivity has been achieved. One implication is that a connoisseur of science may also need to be a connoisseur of those purporting to be connoisseurs?

APPENDIX 1

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ANNEX D

POTENTIAL SCIENTIFIC CONNOISSEURSHIP INITIATIVES

A. ADJUDICATING (OR MORE MODESTLY ASSISTING THE ASSESSMENT OF) SOCIALLY CRITICAL KNOWLEDGE CLAIMS



1. Issue

Even the improved system under which scientific advice crucial to policy is put together and scrutinised occasionally yields results which embody straightforward methodological error or which more subtly fail to deal properly with ignorance and uncertainty, or inappropriately apply risk analysis.

2. Potential experiment/service

Extend the kind of 'constructive deconstructive analysis' that is done on food research into a range of domains and into institutional settings (eg FSA) where it might demonstrate benefits; use shadowing of policy/regulatory processes as technology demonstrators of the discriminatory value-added of this approach.

Provide a forum for the discussion of ways forward to systematise these approaches and discuss the implications for education for discriminating citizens and consumers.

3. Benefits and beneficiaries

Scientific advice for policy or regulation, and therefore much of scientific and technological governance.

4. Funders/collaborators

Very sensitive to existing interests at the heart of government – may need a private funder (Leverhulme or Nuffield on track record?) to fund.

However, no reason why an approach should not be made to OST or Defra.

Echoes some of the experiments (or at least the aims) under the FP6 NEST programme.

5. Potential difficulties and risks of this approach

This type of initiative may encounter institutional sensitivity to change of embedded practices; on the other hand there is increasing recognition that new institutional practices need to be developed, and learning from the shortcomings of past practice may be one way forward.

Scaleability and social learning – although the methodological equipment to scrutinise their own work is something natural scientists should possess, the reflexivity/social distance required in practice may be hard to teach.

Resistance from others to learning from social scientists; criticism that it adds to the 'social amplification of risk' by working one way in ratcheting up the precautionary approach.

B. SOCIO-TECHNICAL VIGILANCE



1. Issue

Pharmaco-vigilance is a feedback mechanism involving the (more or less systematic) gathering of patient evidence of intended and unintended effects of new drugs. There is no equivalent mechanism on socio-technical change, and no sustained attempts to learn from those forms of connoisseurship/knowledge brokerage which already exist.

2. Potential experiment/service

Science shops are a form of scientific connoisseurship, growing in number in the UK, which are more established in continental Europe.

More evidence on their social functions and uses would be useful – through systematic information about who uses science shops, the issues presented, the negotiation of framing issues, the expertise and experience brought to bear by the shops, and the outcomes of use. This would allow a much broader understanding of how science shops operate, and their scope and limit as wider knowledge brokerage mechanisms.

Later stages could involve enhancement of the science shop service and other mechanisms which would tap more directly into socio-technical vigilance.

3. Benefits and beneficiaries

Some broad meta-analysis which would add to our understanding about relationships between science shop 'customers', processes and outcomes.

Indicative statistics for socio-technical vigilance. Recruitment of science shops to a research process which may lead to more useful and robust findings.

Potentially interesting international comparisons of the way problems present, are framed, and are developed.

4. Funders/collaborators

A natural for the Science in Society programme itself, perhaps with ring-fenced additional resources from ESRC.

5. Potential difficulties and risks of this approach

Very partial and soft results, at least in the early stages, from operations that have varying styles but are widely under-resourced.

Self-selection from science shop users (although the nature of this self-selection would itself be of interest).

C. OPEN-DOOR RESEARCH



1. Issue

The core flow of funds through the research councils is still largely science and technology push, although with greater participation of proxy customers from government and industry since the 1993 White Paper. More recently, the SBIR scheme has sought to increase the flow of government research meeting the demands of one category of under-represented user, small business. However, civil society institutions have little input into research priorities, and little opportunity to assess or access research that may be relevant to their needs.

2. Potential experiment/service

From 1977 until 1987 ESRC ran an Open Door scheme for research. This linked proposals from outside academia with research consultants and the research community in Management and Industrial Relations. Despite a recommendation to extend it, the Council decided to close this scheme. A new open door scheme could be wider in disciplinary remit and allow for consultancy between the social and natural sciences in helping to assess the research needs of civil society customers. The social scientist could also have an important analytic role (see C3).

A more controversial variant would see consultants undertake an initial assessment of the case of potential 'Erin Brokovichs'.

3. Benefits and beneficiaries

The Open Door scheme was seen as positive public relations benefit for the Research Council.

More substantively, it was seen as broadening research engagement in the most direct way – by helping its target audience 'make good use of existing research resources', and contributing to the research agenda 'by rooting this more firmly in real experience.'

These might be seen to be the chief benefits to the contemporary research councils too.

For those in STS, it would provide a potentially fertile research site of the interplay of different forms of expertise and experience.

4. Funders/collaborators

This would be an interesting cross-research council experiment and should be funded by them.

It may be possible to collaborate with (and use the momentum of) other bodies such as Defra.

5. Potential difficulties and risks of this approach

The relative lack of impact of the science and society agenda on the research councils to date is based in part on the privileged and confidential role of traditional peer review arrangements. This would need to be a cautious ring-fenced experiment, backed strongly by OST, to have much hope of success.

D. COMMUNITY RESEARCH



1. Issue

Science shops have a geographical presence, usually in a local university. As such, they can be broadly seen as one mechanism for mediating local or community based research. A variety of other public engagement tools can be used to tap systematically into social demands for research at local level.

2. Potential experiment/service

The Loka Institute, a leading advocate of community/democratic based research in the US, is currently working with the University of Massachusetts-Lowell, to evaluate the applicability of scenario workshops to the definition of a community research agenda in Lowell. The research applies 'contestable democratic design criteria' – developed by Dick Sclove – to the process. A workshop to evaluate this and other approaches would be worthwhile.

3. Benefits and beneficiaries

This might be useful as one of the bottom-up inputs to a new meso-level decision-space for research priorities in which bottom-up and top-down research requirements would be brought together.

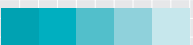
Links to the current Lancaster work on upstream decision-making on nanotechnology.

4. Funders/collaborators

The OST Public Engagement Programme.

5. Potential difficulties and risks of this approach

This is the broadest constituency but one that would be seen as noise by many current science policy interests but might attract support as a thought-experiment.



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