Taken from Broks, P. Understanding Popular Science,
(Open University Press, Maidenhead and Philadelphia, 2006).
Note: page numbers do not correspond with those in the book.

7

CONCEPTUAL SPACE

What kind of model would be appropriate for the kind of critical understanding outlined in the previous chapter? In what ways should we think about the contextualised interactivity of science and public? How can we reframe our understanding of popular science to take into account the way it is constituted by the struggles around it? What follows is not so much an answer as a speculative suggestion.

From the history given in the first four chapters we can see that:

1. Popular science generates different meanings:

Not only do these meanings change over time, but alternate meanings will co-exist at the same historical conjuncture. We have seen, for example, how in the formative period of the early nineteenth century it took on radical, religious or utilitarian aspects. Later it was to be a vehicle for technological utopianism, a wide variety of Darwinisms, and the popularisation of efficiency, rationality and modernity. Finally, we saw how it might also be associated with counter-cultural critiques of technocracy, a search for mystical enlightenment and the postmodern interest in uncertainty, instability and ambivalence.

2. These meanings can be linked to political and social struggles:

We could, for example, point to the "legal ideology of science" and its search for a technological fix to social problems, or to the dissemination of "useful knowledge" as a way to ensure social stability. Later, the triumph of scientific naturalism would mark the triumph of the professionalisation of science, and the subsequent demands for efficiency and planning would see a shift in focus, from the state promoting science to science promoting the state. Oppositional voices might be heard in the demand for "really useful knowledge" and in the concerns for responsibility and public accountability.

3. In these struggles popular science is a form of mediation between public and experts: The "invention" of popular science in the early nineteenth century helped define science by excluding the public. The legitimation of knowledge by an appeal to experience would be replaced by legitimation through an appeal to expertise. The professionalisation of expertise would entail the redefinition of popular science as that science which is "popularised". In a modern "disembedded" society the function of such popularisations is to build a sense of trust between experts and public. However, scientists have been the victims of their own success. The distancing that is needed to maintain their authority is the very thing which undermines their legitimacy in the eyes of the public.

Furthermore, this history invites us to reframe current concerns about the public understanding of science (chapters five and six) such that:

4. In studying popular science our concern should be with meanings not information: The dualism of two cultures is inadequate and divisive. Although there is no clear and generally accepted definition, the reasons given *for* scientific literacy shape the definitions *of* scientific literacy. Consequently, the dominant model of science

communication is itself a legitimation of science and as such should be an object of critical study. A more critical understanding of science in public (CUSP) would entail a shift in attention from the transfer of information to the construction of meaning.

It is in picking up this final point and considering what might be an appropriate way of thinking about CUSP that we move into a more speculative realm. The traditional view of popularization sees science as a gift to be handed over like a package or, as Bucchi describes it, "a conception of public communication of science as benevolent alms-giving" (1998: 2). In the previous chapter we saw how this unidirectional linear model has been challenged and what might be called a second generation of models proposed which emphasise the two-way process of dialogue and engagement. What I want to suggest now is a third generation model which can go beyond this passing back and forth of information to think in terms of contextualised interactivity and so be able to give a critical account of the construction, proliferation and negotiation of meanings.

Spaces, spheres, and boundaries

Elsewhere I have argued that popular science is best seen not as a conduit for messages but as a "forum" where what is popular meets what is scientific (Broks 1993 and 1996). I now want to develop this spatial imagery further with the idea of "conceptual space" as a new model for understanding how the meanings of scientific knowledge are challenged and negotiated. I want to shift from thinking about ideas as objects that get passed from person to person and see if it is possible to start thinking about them as spaces. We are already familiar with spatial imagery. Academics "locate" their work within a particular

subject "area" and "orientate" themselves with respect to other researchers in the "field". There are disciplinary "boundaries" and "frontiers" of knowledge. In the rhetoric of science spatial metaphor is a powerful and pervasive presence with its imagery of discovery, surveying, breaking new ground and so on (see for example Jenkins 1998). Moreover, through our experience of cyberspace we are becoming increasingly familiar with non-physical spaces where things happen and people interact. Indeed, so familiar is the spatial metaphor that I often have doubts about whether there really is something new to say. Yet, it is an idea that seems to be abandoned as soon as anyone writes about popular science. Scientists may well have their own subject area in a particular field and see themselves as pushing back the frontiers of knowledge, but as soon as the public become involved it seems that knowledge as a place to be explored becomes transformed into knowledge as a package that can be handed over. I want to see what happens if we take the spatial metaphor seriously and, as a consequence, see what happens to our understanding of popular science.

Let us begin with Oppenheimer's Reith lecture on "Science and the Common Understanding" (1953). Oppenheimer, who headed the scientific team in the Manhattan Project, likened science to a house:

> It is a vast house indeed….There is no central chamber, no one corridor from which all others debouch. … It is a house so vast that there is not and need not be complete concurrence on where its chambers stop and those of the neighbouring mansions begin.

> It is not arranged in a line nor a square nor a circle nor a pyramid, but with a wonderful randomness suggestive of unending growth and improvisation. Not many people live in the house, relatively speaking – perhaps if we count all its chambers and take residence requirements lightly, one tenth of one per cent of all the people in this world – probably, by any reasonable definition, far fewer. And even those who live here live elsewhere also, live in houses where the rooms are not labelled atomic theory or genetics or the internal constitution of the stars, but quite different names like power and production and evil and beauty and history and children and the word of God.
>
> We go in and out; even the most assiduous of us is not bound to this vast structure. One thing we find throughout the house: there are no locks; there are no shut doors; wherever we go there are signs and usually the words of welcome. It is an open house, open to all comers.
>
> (Oppenheimer 1953: 92-94)

Immediately we get the impression of science as a sprawling, rambling enterprise, an "open house" with no need for agreement on where its boundaries are and complete freedom of movement within and around it. However, the imagery also immediately prompts questions. How true is it that there are no locks or shut doors when, for the most part, science is controlled by the military, the government and multinational corporations? Why is the place so empty? Just how welcoming are scientists to letting everyone come in and mess up the furniture? And what will the neighbours think, especially with that lack of concern over where the property ends?

It is this last question which raises a central issue in science studies: the problem of demarcation. Where – and note the spatial imagery – do we draw the line between what is science and what is not? The problem is as important as it is intractable for it raises a second question: *how* do we decide where to draw the line? In other words, how do we define science, particularly with respect to everything else? Here it is worth recalling from previous chapters the important role that popular science has played in carrying out that demarcation. In recent years it has also taken the attention of scholars within science studies in their explorations of "boundary work", "boundary objects" and "trading zones". For example, an examination of boundary work might show the ways that through social, cultural and political action boundaries are maintained between those who have authority to make knowledge claims and those who do not. Boundary objects, on the other hand act as bridges across the boundaries that might otherwise separate a variety of groups (such as professionals, amateurs, administrators) by providing a common focus even though each group may imbue the object with different meanings. Similarly, trading zones are areas where a common simplified language can enable communication between different groups (for instance across disciplinary boundaries) despite other disagreements elsewhere (see Sismondo 2004; Gieryn 1999; Star and Griesemer 1989; Galison 1997). It might also be pointed out that talk of spaces, boundaries and bridges becomes more than a metaphor in the emerging field of knowledge domain visualisation with its attempts to produce maps of all this (Chen 2003).

Oppenheimer's house of science is the starting point for Charles Alan Taylor's (1996) analysis of the problem of demarcation. Taylor takes a non-essentialist approach, that is, rather than trying to uncover some essential quality with which we can define what science is (an enterprise which has a long history of failure) he examines how science sets itself apart through its own rhetoric. Since such rhetoric will always be employed in, and often in response to, specific social and historical circumstances "the identity of science is a product not of its ontological difference, but rather of its contextual connectedness" (7). His account, therefore, "calls for a recognition of the inescapable symbiosis of what traditionally we have called the technical and the public, the internal and the external, or the natural and the cultural" (9). It is this symbiotic, contextual interconnectedness that leads Taylor to describe the system as a "rhetorical ecology", such that what science is "...is a consequence of particular, historical episodes in which the constituents of the ecosystem stabilize" (8). In this way, spatial metaphors not only invite us to think about boundaries but also, and more importantly, to think about the network of interactions needed to sustain any ecosystem. Thus, with spatial imagery our attention is drawn away from objects to the spaces between them, from things to relationships. If we are then to think of popular science as part of a conceptual ecosystem we might then also have to conclude that there is no such *thing* as popular science – which is by no means a trivial conclusion in our efforts to understand it.

If there is no such thing as popular science what are we trying to understand? An analogy with E.P. Thompson's (1963) famous description of class might be a suitable way of explaining what I have in mind. There was, said Thompson, "an ever-present

temptation to suppose that class is a thing", or to think of the working class that "it" exists (10). However, this "it" does not exist. "If we remember that class is a relationship, and not a thing, we cannot think in this way" (11):

> By class I understand a historical phenomenon, unifying a number of disparate and seemingly unconnected events, both in the raw material of experience and in consciousness. I emphasise that it is a *historical* phenomenon. I do not see class as a 'structure', nor even as a 'category', but as something which in fact happens (and can be shown to have happened) in human relationships.
>
> (Thompson 1963: 9)

The same might be said, I would argue, for popular science: it is not a thing, nor a category but a historical phenomenon that happens in human relationships. In the end, for Thompson, class is "defined by men as they live their own history" (11), and the same might be said of popular science, as we have seen in the way it has been defined, redefined and struggled over. Such fluidity need not evade analysis if we see class as "embodied in real people and in a real context", so too with popular science which we might see manifest in certain activities, behaviours and attitudes or embodied in specific objects or practices (e.g. books, exhibitions or consultations with an expert). Indeed, it may be that as we understood the nineteenth century through the language of class, we shall have to develop a new language of knowledges and expertise to understand the twenty-first.

Thus, if we take the spatial metaphor seriously we are led towards a radical reframing of popular science in terms of boundaries, contexts, ecologies and relationships. There is, however, one further analogy I want to consider: an analogy with urban space. Oppenheimer has something like this in mind for his house of science when he writes that "it does not appear to have been built upon any plan, but to have grown as a great city grows" (Oppenheimer 92). Similarly, Wittgenstein once wrote about language that it could "… be seen as an ancient city: a maze of little streets and squares, of old and new houses with additions from various periods, and this surrounded by a multitude of new boroughs with straight regular streets and uniform houses" (Wittgenstein 1958: 8). Substitute "ideas" for "language" and you have something close to my vision of "conceptual space" except what I want to add is the dynamic quality of what people do in and with cities.

Like urban space conceptual space is the result of design, history and use. It can be created, opened up, closed down. As urban space shapes what we do and how we live, so conceptual space shapes what we think and how we think it. In both cases we interact with the space and transform it for ourselves as it, in turn, transforms us. Our thoughts can be as habitual as the way that we move through a city: always taking the same route (or making the same argument, joining the same ideas), or revisiting the same places and avoiding others (our pet hobby-horses or things we prefer not to think about). Some places are more functional and only visited when needed (as is much of science for the public), other places are simply for pleasure (as much of science is not for the public). More particularly, with both urban and conceptual space we should note that:

1. space can be created and closed down. The more open and accessible a space the greater the variety of use and the less control there is over it.

2. we interact with space. Urban space shapes what we do and how we do it; equally, space may be used in ways other than intended

To close down a space restricts what we can do there. Walls, fences and security systems can keep close control over the use of urban space to ensure that it is used properly (albeit at the expense of freedom of movement): epistemological walls and academic security systems can equally ensure that ideas are used properly (albeit at the expense of freedom of thought). Conversely, the more open a space the more open it is for a variety of uses. Public parks can be spaces for ball games, jogging, picnics, lovers' trysts, walking the dog: music and novels can spark our imaginations and take us on flights of fancy. Finally, and for popular science what often seems most important, space can be used in ways that diverge from original intentions (much to the dismay of local authorities and science communicators). A handrail is designed to increase safety, but skateboarders "grind" down them to increase risk; bridges are built to carry traffic, but provide shelter for the homeless; or, more simply, we might use a shop as a shortcut from one street to another. Equally we might look to the ragbag collection that has sheltered under the name of "Darwinism" or to the popular appropriations of chaos theory or quantum theory.

The analogy could be pushed further, but it is enough to see how the idea of conceptual space reframes our understanding of popular science. No longer is there a simple boundary between science and the public, nor any simple line of communication between them. Instead we can see the open spaces where not everything is done or

thought for rational or rationalized ends, and restricted spaces, fenced off and policed, where only the persevering few are able to venture. Thus, our new concern (in CUSP) should be with access to spaces, with freedom of movement, with helping people to navigate and showing them different routes, with opening up the conceptual environment and constructing enabling architectures. For example, if we translate this into science policy we may have to distinguish between: 1. an "open door" policy with the public invited in and scientists retaining control; and 2. an "open space" policy where the science goes out and the public do what they like with it. The first (difficult enough as it is for many scientists) limits the public's access to science; the second (though more "popular") limits science's influence over what gets released. The dilemma highlights a very real problem for scientists since the desire to make science more public may conflict with an equally strong desire to control the meanings that the public construct.

Just how deep the problem goes can be seen if we return to the image of humans as "lumbering robots" that Richard Dawkins uses in his book *The Selfish Gene* and which we looked at in the previous chapter. Dawkins is one of the great popularisers of the modern age. Not only is he a professor of the public understanding of science, but his numerous books (highly readable, clear, and provocative) show him to be a master of the art. Yet he seemed surprised that his image of human robots could be seen differently from the way that he had intended. In a note to the second edition of the book Dawkins tried to explain and justify himself:

> This purple passage (a rare – well, fairly rare – indulgence) has been quoted and requoted in gleeful evidence of my rabid 'genetic determinism'. Part of the problem lies with the popular, but erroneous, associations of the word 'robot'.
>
> (Dawkins 1989: 270)

As Andrew Brown has commented, "Who is he to tell us what the erroneous associations of 'robot' are?" (Brown 1999: 40). What Dawkins associates with the word "robot" may differ from what the public has in mind, but that does not make the popular associations erroneous. Indeed, it makes Dawkins sound rather like Lewis Carroll's Humpty-Dumpty who tells Alice, "when I say a word it means whatever I want it to mean". Consequently, *contra* Dawkins, the problem lies not with those popular meanings that have a general currency, but rather with the expectation that a more circumscribed set of associations should be accepted. In other words, the problem lies in the expectation that there can be some measure of control over the meanings of an idea once it is placed in the public domain. Unfortunately, the problem is exacerbated by the very thing that makes communication possible – the use of a common language. As Steven Shapin has pointed out:

> To the extent that scientific statements are couched in, or even appear to be couched in, ordinary public language, problems may be endemic. On the one hand, scientists may decide that certain scientific conceptions simply cannot be expressed in the public language. On the other hand, scientists' endeavours to use

> that public language may involve metaphors and analogies whose resonances they cannot expect to hold in place and control.
>
> (Shapin 1990: 997)

It is then a question of control. To open up conceptual spaces means to lose control over them. The challenge for scientists is not just whether they are able to do this but whether they are willing. It is also a challenge that must be faced by democracy.

Science and democracy

With only a few exceptions, until the mid-nineteenth century "democracy" was a "strongly unfavourable term" especially at the time of the French Revolution when Burke thought that a perfect democracy was "the most shameless thing in the world". Democracy, in the sense of direct rule by the people, was thought of as nothing more than uncontrolled rule by the mob, or as we might now say mob-rule. Indirect rule by the people (or representative democracy) developed as a modified form in the United States, but it was only from the mid-nineteenth century that the term lost some of its revolutionary overtones. By the twentieth century it was so prized as an ideal that it became the common claim of rival traditions across the political spectrum (Williams 1976: 82-7). Consequently, as we have seen in previous chapters, in the early-nineteenth century respectable science sought to distance itself from democracy's radical associations. The French Revolution provided an all too clear example of what "democracy" meant and so the legitimation of respectable science was to be found in appeals to its piety and utility. By the twentieth century, however, the consolidation of

science's position within the state naturally meant the legitimation of science through appeals to the state's ideals, in particular "democracy" which was by now seen as a favourable term. The growing respectability of the democratic ideal and the desirability of its representative form match, step by step, the rise in status of science and the enhanced prestige of its experts. However, as we have also seen in previous chapters, the rise of the expert was at the expense of public participation in science. What we see then, is science's appeals to democracy becoming more common at the same time as science itself was becoming less "democratic", that is, as it increasingly disenfranchised the public by excluding them from the scientific enterprise. Not surprisingly, on close inspection the relationship of science with democracy turns out to be an uneasy partnership.

It has often been suggested that science is inherently democratic. Jacob Bronowski (1964), for example, thought that an ethic for science would arise naturally from its own practice. There were no technical or philosophical rules for doing science, he argued, instead the conditions for the practice of science were to be found in its values, “independence and originality, dissent and freedom and tolerance” (68). Thus, dissent and tolerance were not only essential conditions *of* science they were also essential preconditions *for* science. “The society of scientists must be a democracy. It can keep alive and grow only by a constant tension between dissent and respect; between independence from the views of others, and tolerance for them” (69). This liberal pluralist vision was particularly attractive in the West at the height of the Cold War – its clearest, and most often cited spokesman, Karl Popper. In large part, Popper’s “open

society" was the product of his philosophy of science, a philosophy which emphasised the provisional quality of scientific knowledge. Not only was science vulnerable to the development of new ideas, he argued but that was how it should be; true science opened itself to be proven wrong and much scientific activity was directed towards such falsification. Consequently, only democracy offered the possibility for an "open society" because only democracy embodied these provisional qualities, could see society simply as work in progress, would have the "openness" to admit to mistakes and so have the necessary flexibility to make improvements based on experience. Nor was Popper alone in such views, as Steve Fuller (1997) notes, "most democratic theorists in the Western tradition…..have regarded the form of critical inquiry that characterizes science as the model of the ideal polity" (4). However, Fuller also immediately points out a major problem for this approach, and one which relates directly to our concern for popular science. The problem is "…whether the model is limited to an elite whose internal divisions mirror the interests in society at large or whether the model can be extended to include everyone's direct participation" (4). In short, should our democracy (and science) be participatory or representative?

Far from the easy equation of science and democracy, it might even be that science poses a threat to democracy and we might have to consider how the two could be reconciled. Steven Turner (2001) for example, highlights two problems that scientific expertise poses for liberal democratic theory. The first is the problem of equality: whether it violates the conditions of rough equality presupposed by democratic accountability giving power to those privileged to possess the expertise which others cannot control,

acquire or share. The second is the problem of neutrality: “whether the state can preserve its neutrality in liberal ‘government by discussion’ while subsidizing, depending on, and giving special status to, the opinions of experts and scientists" (123). Indeed, even though the liberal democratic state is supposed to be neutral:

> …the state not only protects and subsidizes science, it attends to the opinions of science, which is to say it grants science a kind of authority, and reaffirms this authority by requiring that regulations be based on the findings of science or on scientific consensus, and by promoting the findings of science as fact.
>
> (Turner 2001: 124)

The problems are compounded by low levels of scientific literacy, as Christopher P. Toumey (1996) says "scientific thinking can hardly enhance democratic culture when barely anyone can think scientifically" (38). Nevertheless, Toumey looks to American democratic culture for what he calls “democratic science”:

> … a matrix of cultural conditions (including values, meanings, symbols, judgements, and the opinions of nonscientists) in which both the style and the content of science are shaped by direct and indirect democratic processes, including elections, referenda, legislation, litigation, consensus, and compromise. Nonscientists' understandings of science intersect with the expertise of credentialed scientists, and scientific thought is subject to extrascientific

considerations. The subject matter is science, but the framework is American democratic culture.

(Toumey 1996: 41)

In some respects, Toumey's "matrix of cultural conditions" seems to have much in common with Habermas's "public sphere", but there are important differences. In Habermas's analysis ([1962] 1989) the public sphere developed in the eighteenth century as a mediation between private interests (family, work, everyday life) and the power of the state. Initially constituted through public discourse in newspapers, journals, coffee houses and political societies it was associated with, and eventually institutionalised by, the rise of bourgeois liberal democracy. In both Toumey and Habermas, therefore, we can see an appeal to public debates within a framework of democratic culture. However, for Habermas the public sphere was in decline from the late-nineteenth century onwards to be replaced by the spectator politics of welfare state capitalism. Toumey's democratic science, on the other hand is very much concerned with the present day, making reference to the recent development of consensus conferences and science shops as important ways that science can contribute to the democratic process. More importantly, whereas for Habermas the central feature of the public sphere is rational-critical debate, for Toumey democratic science encompasses values, symbols and a plurality of meanings making it "politically gracious and hermeneutically promiscuous" (56).

Habermas looks to the open and unrestricted communication between experts, politicians and the public as a way of reviving the public sphere and realising the Enlightenment ideal of a rational society. As such, it is a model of deliberative democracy that gives a central role to the relationship between science and public. Not surprisingly we can see it being taken up as part of that move from the Public Understanding of Science to the Public Engagement with Science and Technology but, as Elam and Bertilsson (2003) argue:

> As a basis for the 'democratic turn' in PUS, and its self-mutation into PES, deliberative democracy provides a model of democracy where scientists have good chances of appearing before others as already model scientific citizens. By valuing rationality, reserve, selflessness and powers of argumentation, deliberative democracy is a democratic politics played out on scientists' home turf.
>
> (Elam and Bertilsson 2003: 242)

This takes us back to the asymmetries of science-public dialogue that were discussed in chapter six – asymmetries of power, authority and epistemology. If we want to examine science and democracy with the kind of model that was proposed in that chapter (directional, contextualised, broadens notion of expertise and concerned with meanings), then we need to move beyond the rational debates of Habermas's public sphere.

Much of what we need can be found in the analysis of Nowotny, Scott and Gibbons (2001) and their attempts to "rethink" science. The organising perspective for their rethinking is the increasing contextualisation of science. The history they present contrasts the period of growth, predictability and planning in the quarter century after the Second World War with the period of uncertainty at the end of the century (the turning points being the oil crisis of the 1970s and the collapse of communist regimes in the 1980s). Society in this new period of uncertainty (what they call "Mode-2 society") is characterised by pluralism, diversity, volatility and transgressivity as the boundaries between the domains of private, public, market, culture and media are broken down. In short, it is the type of society we saw in chapter four and which is often analysed in terms of post-industrialism and post-modernity. Furthermore, with Mode-2 society comes Mode- 2 science – contextualised science that arises from the closer interaction of science and society, and the emergence of socially distributed expertise. In turn, increasing contextualisation demands that we "rethink" science as it moves from the production of reliable knowledge to the production of socially robust knowledge. It also means that the formulation of scientific problems and solutions has moved from institutional sites within government, industry and universities out into what they call the "*agora*":

> The *agora* is the public space in which 'science meets the public', and in which the public 'speaks back' to science. It is the domain (in fact, many domains) in which contextualization occurs and in which socially robust knowledge is continually subjected to testing while in the process it is becoming more robust. Neither state nor market, neither exclusively private nor exclusively public, the

> *agora* is the space in which societal and scientific problems are framed and defined, and where what will be accepted as a 'solution' is being negotiated.
>
> (Nowotny, Scott and Gibbons 2001: 247)

The "agora" was the city centre in ancient Greece. Often translated as "marketplace" it was more than simply a site for commercial transactions. It was a meeting place; a cultural centre; a social hub; a place of gossip, news and intrigue; a place filled with hustle, bustle and noise. This is where, metaphorically speaking, science has to survive. Nowotny, Scott and Gibbons, however, do not see this as a threat to science but rather as a great opportunity. Moving science into the agora will mean, amongst other things:

- raising questions about social justice, economic equality and the democratisation of knowledge as we face the challenge of how to cope with the proliferation of uncertainties
- finding flexible strategies to explore the unknown implications of a contextualised science
- seeing people as active agents in generating contexts
- seeing the "social" as a key resource of creativity and innovation rather than an intrusion into science
- valuing trust as an even more scarce and precious resource in the social distribution of expertise

Science in the agora certainly seems to be a way forward in our thinking (or rethinking) about science in public.

The agora also picks up the spatial imagery of the previous section. How can we apply this way of thinking (forum, conceptual space, agora) to the problem of science and democracy? After all, the ancient Greek agora might be seen as the birthplace of democracy. Could our conceptual agora help us develop a conceptual democracy? It is with this in mind that I want to put forward one final speculative suggestion. Rather than try to see science as an essential part of a democratic society that attempts to share power, could we not see science as an essential part of a "demosophic" society that attempts to share knowledge? As democracy is a system to manage power, balancing individual interests through collective rule: so demosophy would be a system to manage knowledge, balancing individual experiences through collective wisdom. In the clamour and confusion of the agora where knowledge is contextualised, uncertain, contested we would certainly need formal and informal institutional machinery to bring some degree of epistemological order, much as democracy has formal and informal institutional machinery to bring some degree of political order. Indeed, when we say that democracy does or does not work most often we are passing judgement on the machinery of democracy rather than democracy itself. Rarely is the democratic principle itself questioned. Can the same be said for the sharing of knowledge as it is for the sharing of power? Can there be a similar acceptance of the demosophic principle at the same time as there is a rigorous examination of the machinery that puts that principle into practice? But is this not a pernicious form of relativism with all its attendant dangers? Maybe not. A fully functional demosophic society is no more likely to fragment into epistemological anarchy than a fully functional democratic society will break down into political anarchy. Each is based on a collective individualism. As we maintain legal institutions to deliver

"justice", so we need scientific institutions to deliver the "truth", even though justice and truth may not ultimately be obtainable. We need, therefore, to hold to our ideals of justice and truth while recognising the imperfections of our systems to deliver them. We must not confuse what ought to be with what is.

It has taken centuries of cultural, social, economic and political struggle to develop the mechanisms and institutions that enable the sharing of power in a democratic society (nor should we see that struggle as complete or current institutions as any more than work in progress). We should, perhaps, expect an equally long struggle to develop the cultural, social, economic and political machinery for the sharing of knowledge in a demosophic society. A proper understanding of popular science will help us do that.

Further reading

Gieryn, T.F. (1999) *Cultural Boundaries of Science: credibility on the line.* Chicago: University of Chicago Press

Habermas, J. [1962] (1989) *The Structural Transformation of the Public Sphere: an inquiry into a category of Bourgeois society.* Cambridge: Polity

Nowotny, H., Scott, P. and Gibbons, M. (2001) *Re-thinking Science: knowledge and the public in an age of uncertainty.* Cambridge: Polity

Smith, C. and Agar, J. (eds) (1998) *Making Space for Science: territorial themes in the shaping of knowledge.* Basingstoke: Macmillan