Experts and anecdotes

Shaping the Public Science of Mobile Phone Health Risks

Jack Emrys Zachariah Stilgoe

University College London

Doctor of Philosophy

Abstract

This thesis reports on a case study of scientific and public aspects of the recent controversy over the possible health risks of mobile phones and their base stations. The research for this project involved 31 interviews with key actors (scientists, advisory scientists and representatives from interest groups and industry) and archive and documentary research. Using theoretical perspectives from Science and Technology Studies, I recount the move from a style of scientific advice in which non-experts were prevented from engaging with science to one in which their concerns and knowledge were ostensibly considered.

These advisory discourses are described as constructing (and reconstructing) not only a level of scientific uncertainty, but also the limits of public engagement. In this way, scientific and social orders are co-produced in the course of public science. 'Public concern' about mobile phones is revealed as a malleable, dynamic set of interests and actions. Experts, in taking public concern into account, reshape it, controlling areas of public engagement. As well as the narrative of changing scientific advice which prompts these insights, I consider the meanings attached to the term 'anecdotal evidence' as a site for the contesting of uncertainty and public concern.

Contents

Some anecdotes

A picture

Acknowledgements

My thesis is a (relatively) tidy product of a long and complicated period of research. My ambitions, interests and ideas would never have become words without the guidance of some important people. Thanks go to the following, who have discussed and helped shape my case study: Adam Burgess, Darrin Durant, Sheila Jasanoff, Jerry Ravetz, Arie Rip and Andy Stirling. Above all, thanks to my supervisors, Jon Turney and Brian Balmer, who have been informative, critical, supportive and realistic at all the appropriate times in this thesis' production. I also benefited hugely from working in an environment as enthusiastic and supportive as UCL's Department of Science and Technology Studies. In addition to my supervisors, staff and fellow PhD students in the department contributed greatly to my academic development. An audience at the 2002 EASST conference in York was constructive in discussing what would later become chapter six. Thanks are also due to all of my interviewees, whose careful consideration of the themes I presented them with has made interpretation much more straightforward. For the speedy release of the records of the IEGMP, I thank the staff at the NRPB library and the Public Records Office. The majority of the research for this thesis was funded by the Economic and Social Research Council, so thanks are due to them.

Also thanks, as ever, to my family. And Faith, for everything else I can think of... and probably much more.

List of Abbreviations used

- **3G** – Third Generation (mobile phone networks)
- **AGNIR** – Advisory Group on Non-Ionising Radiation
- **ANT** – Actor Network Theory
- **ELF** – Extremely Low Frequency (used to refer to EMFs such as those generated by powerlines (50Hz)).
- **EMF** – Electromagnetic Field
- **EPA** – Environmental Protection Agency
- **GSM** – Global System for Mobile Communications (also 'Groupe Spéciale Mobile')
- **HCSTSC** – House of Commons Science and Technology Select Committee
- **HCTISC** – House of Commons Trade and Industry Select Committee
- **ICNIRP** – International Commission on Non-Ionizing Radiation Protection
- **IEGMP** – Independent Expert Group on Mobile Phones (The 'Stewart' group) (1999-2000)
- **MTHR** – Mobile Telephones Health Research programme (2001→)
- **NRPB** – National Radiological Protection Board
- **RF** – Radiofrequency (the range of EMF frequencies in which TV, radio and mobile phones transmit)
- **SAR** – Specific Absorption Rate
- **SSK** – Sociology of Scientific Knowledge
- **STS** – Science and Technology Studies
- **WHO** – World Health Organization

1 Introduction

Or: *How we learned to start worrying, while loving the mobile phone*

In 1989, long before 'text' was a verb, mobile phones were safe. They were not safe because they were built to more stringent standards, nor because they operated at different frequencies from those used today, nor because all scientists agreed that they were harmless. They were safe because they were not objects of public or scientific health concern. Ten years later, mobile phones were risky. They were not a *definite* risk in the way that playing Russian Roulette is a *definite* risk,[1] but the interactions between experts, non-experts, interest groups and the media through the late 1990s had unearthed sufficient uncertainty to support concerns that mobile phones and their base stations might be harmful. By 1999, an opinion poll could tell us that, "Nearly half of frequent users of mobile phones fear health risks,"[2] whatever that might mean.

This thesis considers how questions about knowledge of mobile phone risk or safety were opened up. It is retrospective, but it is also an explanation of a controversy that is ongoing. My attention was drawn towards the mobile phones health controversy in 2000, when it was at its most fevered. Since then, I have followed its public developments in my capacity as a researcher. National newspapers seem to have relaxed, reporting new scientific studies with less vigour. Nevertheless, the controversy has not left the public sphere. As I was starting to think my account was historical, new developments tested my understanding of the contested areas described in these chapters.

Since starting work on this introduction, a study (TNO 2003) claiming that worrying symptoms can be experienced by people who live near third generation mobile phone masts has attracted a share of media attention (in October 2003). On bonfire night (5th of November), in the Warwickshire village of Wishaw, protesters removed

[1] It might seem clumsy to interrupt the text of my thesis with a first footnote so early, but, for the sake of completeness, I should point out that one commentator has explicitly likened the risks of mobile phones to playing Russian Roulette (Kane 2001).

[2] MORI (Market and Opinion Research International) – "Nearly half of frequent users of mobile phones fear health risks," –Press release, 3rd June 1999
http://www.mori.com/polls/1999/tecnoao.shtml, accessed 10th November 2003

the bolts from the base of a mobile phone mast that was seen as the cause of local clusters of illness. Advisory scientists (such as those interviewed for my project) were called upon to explain why the presence of such illness does not count as valid evidence, but that robust research was needed to look at the question of base station risk.[3] The day before these protesters destabilised the target of their concerns, the programme that currently manages relevant UK research into the health effects of mobile phones had held its third public meeting to discuss how public concern (about things like mobile phone masts) should be taken into account in finding out about mobile phone risks.

It is the purpose of this thesis to explain, from a Science and Technology Studies (STS) viewpoint, the development of what science knows about the risks of mobile phones and the context of ongoing research into the problem. This thesis is about the emergence of a controversy. But it does not aim to explain in any real depth how *we* – the general public – began to worry about mobile phones. I hope instead to contribute an understanding of how *we* – experts, non-experts and all – came to realise our level of uncertainty about the science that might previously have reassured us of safety. My thesis aims to explain how our experts came to know about the possible health risks of mobile phones, and how subsequent changes forced the re-evaluation of this knowledge. My thesis is a study of science, but it is a study of science in context. In particular, it is a study of scientific *uncertainty* in context, asking how public science emerged from negotiations about what different groups did not know. These uncertainty claims carried varying degrees of credibility among other groups, but all were put forward as part of a controversy in which the stakes were potentially very high. The vast majority of the British population now owns a mobile phone,[4] and the number of base stations (necessary for reliable networks) has expanded hugely in the last decade. Even the smallest problem that can be attributed to this now-ubiquitous technology would have enormous

---

[3] For example, 'GMTV' on the morning of 11th November 2003, reporting from the scene of the ongoing protest (preventing the network operator from replacing the downed mast). Professor Lawrie Challis was interviewed in his capacity as chair of the Mobile Telephones Health research Programme.

[4] According to the MORI (Market Opinion Research International) 'Technology Tracker', ownership of a mobile phone is now beginning to tail off at about 75% of the population (Updated August 2003) http://www.mori.com/emori/tracker.shtml, accessed 10th November 2003

implications, a fact which lies behind the arguments of the actors in this narrative and my own.

The controversy over mobile phones, which emit Electromagnetic fields (EMFs), arrived as part of a lineage of controversies about the health effects of EMFs. The 1980s saw similar arguments take place over the hazards of living near powerlines, generators of Extremely Low Frequency (ELF) EMFs.[5] Later, doubts were made public about the safety of microwave ovens, which cook with fields of similar frequencies to mobile phones, albeit at hundreds of times the power. Similarly, concerns have been expressed about various radar, TV and radio transmitters and about computer monitors. Each EMF technology carries its own controversies and its own stories (see Brodeur (1989) for a popular summary) but they share interest groups, styles of reasoning, and the types of evidence that emerge as significant.

The first suggestion of harm from a mobile phone which received global attention was a single story of a man in America who decided to sue a mobile phone company in 1992, blaming his wife's brain tumour on her exposure to mobile phone radiation. The publicity granted to David Reynard's story caused a large dip in the stock market value of American mobile phone manufacturers. In 1995, a federal judge had ruled that the evidence submitted in the Reynard case was not "scientifically valid"[6]. But by this time the controversy was well under way, and was about to cross the Atlantic, where it began to excite UK newspapers in 1996 (Burgess 2004).

My analysis aims to suggest a context for the role played by such stories in science, given that most scientists would argue that one man's account of his wife's brain tumour (often pejoratively categorised as 'anecdotal') is as weak as evidence gets. Scientists claim to have been studying the health implications of EMFs for as long as EMFs have been used, since Marconi first broadcast radio.[7] And yet, in the case of

[5] Much of the controversy over power lines was powered by an epidemiological study that detected an increase in childhood leukaemia in families living near powerlines (Wertheimer and Leeper 1979). New studies continue to aim towards a definitive confirmation or refutation of this link.

[6] The judge in this case followed the precedent set by the landmark Daubert vs. Dow case. For an insight into the importance of this case to the assessment of science (and 'junk science') in the courtroom, see Solomon and Hackett (1996).

[7] One scientist told me: "The study of electricity, magnetism and microwaves has existed since, what, Guglielmo Marconi started producing, you know, we started getting electrification in the '20s and the

mobile phones, knowledge has emerged not just from science, but from the conflict and resolution that has occurred between experts and non-experts, between robust evidence and evidence that might traditionally have been considered inadequate.

This thesis is about experts, non-experts, evidence, uncertainty and policy. It asks questions about two main themes. *Firstly*, it asks how uncertain science is constructed and communicated. *Secondly*, it asks what part non-experts play in building scientific knowledge about an ongoing health controversy. The theme of 'anecdotal evidence' is pertinent to both questions, but fits most easily within the latter. The focus of my study is 'scientific advice', but I have used the phrase 'public science' to describe both the outcome of these negotiations and the process of ongoing research. 'Scientific advice' seems to me to suggest that there is a body of knowledge that exists to be communicated to non-scientists. As we shall see in this thesis, the 'science' that I am talking about is a dynamic, politically-located set of interactions between knowledge, research and public engagement. 'Public science' therefore describes the ongoing production and communication of expert knowledge about an issue in the public gaze.

As with many recent controversies (and the mobile phones controversy is still less than ten years old), there is a time lag between events and their representation or explanation in academic terms. Much has been written about the scientific and policy challenges of mobile phone health uncertainties, but most has come from scientists themselves (e.g. Foster et al 2000, WHO 2003) (notable exceptions being Leiss and Paoli (2001) and Burgess (2002; 2004)). My account offers a different perspective on the issue. It problematises the state of expertise about mobile phones and comes to some very different conclusions about the concerns, both scientific and public, that other commentators have taken for granted as fixed (cf. Burgess 2004). This thesis looks primarily at expertise. It therefore represents only a partial treatment of an issue that could have been covered in any number of other ways (as a 'moral panic', as a risk perception problem, or maybe as a mass hysteria fuelled by media attention). This thesis questions experts and it questions science. However, its ambitions lie beyond providing an explanation of the science of mobile phone risk

---

'30s. I think people started to worry. This electricity, does it cause cancer? Does it cause [cell] reproduction? Does it affect my brain? The questions we're still asking now, and I think those were the questions they were asking then" (Interview transcript, No. 32).

assessment. The issues surveyed and developed tell us about more than just mobile phones science and more than just science. They tell us about science *in public*, shaping the world about which it claims to know and being shaped by the people inside and around it.

This thesis does not contribute an assessment of the reality of mobile phone health risks. My own opinions, which I keep to myself, are borrowed wholesale from those interviewees whose opinions and expertise I most trusted. But a perception of risk is not a fixed opinion. The certainties and worries of many of my interviewees have adjusted with their experiences of the public science of mobile phone risks. And many of my own preconceptions and concerns have been reversed in the researching of this controversy.

Thesis structure

Chapter two considers some theoretical insights from Science and Technology Studies that winch my perspective away from 'fact-making' to 'sense-making' (Jasanoff 2004b). My analysis adopts a constructivist approach to science, informed by insights from the broad field of Science and Technology Studies (STS). I consider the influence of the Sociology of Scientific Knowledge, but I suggest reconnecting with the growing field of Public Understanding of Science to better explain how, in public, experts, non-experts and policymakers combine and react to create some kind of social order around an issue. My aim in this chapter is to understand how STS sees experts making sense of the actions of non-experts and vice versa. I conclude with the adoption of a recent STS synthesis known as co-production, which steadies the progress of subsequent chapters.

Chapter three explains how I found out about the case of mobile phone health risks – the methods I used and the methodologies that enlightened these methods. The research for this thesis was qualitative, based around more then 30 interviews with many of the key players in the building of this area of public science. These interviews, supplemented by archival and documentary research, allowed me to reconstruct the specifics of the case of mobile phone risks, which I retell and explain in chapters four, five and six.

Chapter four explains what science claimed to know about EMF risks before mobile phones became a ubiquitous technology. I introduce the National Radiological Protection Board (NRPB), whose responsibility it was to suggest, scientifically, what level of EMF exposure was safe for the public. By explaining some of the challenges to the existing consensus and its advisory offshoot, I argue that the defence of science and regulation formed a 'discourse of compliance', limiting the scope for public engagement.

Chapter five considers how the Government responded to a loss of advisory credibility, prompted by a public realisation that science was no longer answering their questions. This chapter is about the Independent Expert Group on Mobile Phones (IEGMP) and their report (the Stewart report). This report reconfigured scientific advice, prying open new areas of uncertainty and prompting new areas of research. Chapters four and five are ordered chronologically, but they are also ordered thematically. My analysis suggests that the IEGMP significantly contributed to the re-engagement of certain groups, reconstructing science and social order around mobile phone risks.

Chapter six looks back over the stories of the previous two chapters and asks about the importance of one easily-neglected term in the controversy: 'Anecdotal Evidence'. I describe anecdotal evidence as a microcosm for the broader negotiations taking place between experts and non-experts. I take the term to be flexible and poorly-defined, which allows an illustration of how contested epistemological and political status can construct a debate. In a general sense, this chapter says more about what it means to dismiss something as 'anecdotal' or take on board anecdotal evidence than it does about what anecdotal evidence *is* in a public science debate.

Chapter seven considers the messages of the previous chapters and argues that the pattern of public engagement that has emerged since the Stewart report forces us to think more deeply about the co-production of science and society. 'Public concern' emerges alongside scientific uncertainty as a malleable resource in the shaping of public science. The styles of public science described through earlier chapters are summarised as constructing not only the state of scientific uncertainty but also the legitimacy (and limits) of public engagement. Expert and public understandings (scientific and social orders) are co-produced in the shaping of public science.

Another point needs to be made before we continue. This thesis reports on a UK case study. It looks at the changes that have occurred in British public science. But, as with any study of science, its target – knowledge – is, or often claims to be, global. Much of my source material is international, and I hope that my work will inform similar studies abroad, but the focus throughout is on the development of science as it applies to the UK experience of mobile phone health risks.

At three points in my text, I provide anecdotes for consideration. They are not interpreted in great detail in the main text. I leave it to the reader to determine their relevance. They might simply provide introductions to sections of analysis, or they might ground the theoretical work in experience. My thesis is deliberately hesitant in its judgement of what an anecdote is, or what counts as anecdotal evidence. For some readers, my three anecdotes might begin to suggest this interpretative flexibility. For others, they might confirm the disadvantages of what many people understand by anecdotal evidence.[8] Two are based on my own experiences and one is second-hand, relayed to me by the transcription of a public meeting.

As I implied in this chapter's first paragraph, the safety of mobile phones will be a societal decision rather than a scientific one. Expertise and evidence will play a part in the assessment of risks, but the public science of studying and managing risks will necessarily include broader issues of credibility and trust. It is for this reason that we must look at scientific knowledge and research in its context. The next chapter considers theoretical insights that have considered both the laboratory contexts in

---

[8] Using an anecdote to introduce anecdotes is a useful tool to provide depth to my explanation, and also to address issues of reflexivity, helping to frame my own research according to the insights gained from constructivist studies of scientific research (see Woolgar 1988). (Although another level of self-conscious reflexivity might show that using anecdotes in this way might be a clumsy way of protecting my research from criticism).

which science is produced and the broader contexts in which it is communicated and understood.

2 Perspectives on Public Science

As explained in the introductory chapter, the mobile phones controversy is a controversy about scientific uncertainty, public engagement and the acceptability of different types of evidence. The theory with which I interpret my research therefore needs to consider these features. It needs to consider science, but it cannot ignore the public dimension of this controversy. It needs to consider how people make facts, but it also needs to consider how people make sense: how experts make sense of non-experts and vice versa.

The purpose of this chapter is to suggest a theoretical framework to illuminate my empirical research. I will explore a number of broad themes that are pertinent to science and its relationship with its publics. I will explore in detail some themes that are particularly relevant to my case study, themes that I will return to as I explore the findings of my research. This chapter aims to contribute an explanation of the dynamics of public science, a realm in which experts must communicate with decision-makers (whoever they may be) and the public, providing credible advice while controlling knowledge production, while appreciating the political context of their own actions.

I begin by looking at the sociological insights that help us to problematise science, expertise and, by implication, its public context. The next section deals with the thorny issue of scientific uncertainty – how it is defined, shaped, represented and dealt with by experts and decision-makers. I then look at the social context of scientific evidence, asking what evidence means in science and how evidence is dealt with when it emerges from non-scientific sources. These middle sections both aim to ask important questions about the role that the public might play in changing scientific and policy practices, and about the changing nature of the science/public boundary. My conclusion takes the form of a discussion of a recent overarching explanation for the nature of scientific and public debates. The concept of *co-production* helps to resolve some of the difficulties I have felt with other social explanations that concentrate on scientific practice. This chapter contributes to a richer understanding of issues that affect science and policy, when the development of knowledge cannot be prised away from its public context.

In one sense, the way I will illuminate my research falls neatly into the field of Sociology of Scientific Knowledge (broadly defined). I will discuss, as SSK does "ideas such as the experimenters' regress, the enchanting effect of distance from the research front, the impact of the evidential context in which claims are cast, and so forth" (Collins 1995, p. 308). However, I adopt a critical position with respect to large sections of SSK literature. Internationally, this follows the call of authors who have suggested the need to contextualise SSK analyses beyond 'science' (e.g. Richards 1996), complementing research that gives itself the broader umbrella title of Science and Technology Studies (STS).

The UK tradition of SSK[1] has doubtless been hugely influential in furthering a view of science that emphasises the vagaries of its practice rather than its neatly-packaged products. But it has limitations in dealing with science in its broad socio-political context. Incorporating insights from further afield in science and technology studies will hopefully illuminate many aspects of the poorly-understood realm of public scientific advice. One of the areas that has grown up concurrently with SSK in the UK, but has crossed over little, is the field of Public Understanding of Science (PUS), a term which unfortunately brings its own ideological baggage. My research aims to further the relationship between SSK-based studies of science and PUS analyses of knowledges in their context (Wynne 1991).

My theoretical toolkit, as well as plotting some theoretical movements within science and technology studies, also aims to contribute to explaining a subtly different context of public science. As a result of past lessons, general social trends and, perhaps, some acceptance of the established insights I describe below, the relationship between science and the public and political spheres has been revealed as flexible.[2] It can no longer be taken for granted that 'science' occupies a single

---

[1] The UK tradition of SSK might be best characterised by the work of Barnes, Bloor, Edge, Collins and Pinch (e.g. Bloor [1976] 1991, Barnes and Edge 1982, Collins 1985 and, for a more popular introduction, Collins and Pinch [1993] 1998).

[2] A recent report from the House of Lords Science and Technology Select Committee, to which Brian Wynne was a special adviser, contains a number of STS-inspired recommendations for a new appreciation of science-in-public (House of Lords 2000). One paragraph recommends, "That direct dialogue with the public should move from being an optional add-on to science-based policy-making and to the activities of research organisations and learned institutions, and should become a normal and integral part of the process." (paragraph 5.48)

position in society or politics. It is therefore necessary for any framework I suggest to be able to adequately explain a shifting context for public science. It would be all too easy to whisk the rug from beneath a canonical view of science without checking whether anyone was still standing there.

I begin my investigation of the literature by asking whether the influential canon of constructivist thought that has formed the bedrock of STS literature is as informative when we move from the laboratory into the open ground of public engagement.

Making Science Public, Making Public Science

Social studies of science have convincingly demonstrated over the last three decades that social factors are vital to the establishment of facts, the closure of areas of controversy and the preservation of authority. A key to the success of these studies is a symmetrical treatment of 'right' and 'wrong' science. This 'symmetry postulate' (see Bloor 1991, p. 7, also pp. 175-9) allows for exploration of the construction of scientific knowledge regardless of whether it is later expertly considered to be true or false. To take one example, Bruno Latour's story of Louis Pasteur's success in gaining acceptance of his theories and techniques does not rely on the *correctness* of Pasteur's work. It describes how, through attracting the interests of other necessary parties, in effect bringing his laboratory into their context, Pasteur convinced others of the problem of anthrax and the efficacy of his solution. This allowed him to establish his laboratory as the only location through which applicable knowledge about anthrax can be reached (Latour 1983).[3]

SSK has, over the years, taken up the task of problematising and taking apart the construction of certain facts in 'black boxes' (Latour 1987), which are difficult, if not impossible to open after they have been sealed. The analysis of science-in-the-making (ibid.) [4] can reveal the troubles of the route to the closure of these black

[3] This is a simplistic description of what would be referred to as 'mutual enrolment of actors' in a real Actor Network Theory analysis. Pasteur's lab becomes an 'obligatory passage point' for knowledge about anthrax.

[4] In later work, Bruno Latour uses the word 'research', as distinct from 'science' to better capture the practice of what scientists do rather than the packaged products of their efforts (see Latour 1999)

boxes. In another obligatory passage point[5] of the SSK canon, Harry Collins' influential work on the nature of experimentation and replication has demonstrated that, in the absence of experimental closure of debates, due to inevitable disagreements about what counts as replication, social factors will necessarily contribute to settlement (Collins 1985). This 'experimenter's regress' has provided a cornerstone for thought about the socially contingent nature of scientific knowledge. We will see in later chapters how reports of scientific results fuel debate about the adequacy and relevance of particular experiments in determining the presence of risk from mobile phones. When decisions have important implications beyond the scientific community, the impotence of experimental results in sealing debate is even more marked.

Studies within SSK are enormously helpful in understanding science's social contingencies, and I will return to other examples during this chapter. But many of these studies, Collins' especially, have tended to deal *within* science. Steven Shapin, reviewing Bruno Latour's science is action, points out that there is as much politics inside science as outside it (Latour 1987, cited in Shapin 1995a) and it is this that SSK has proved so effective in unravelling. But, without wishing to detract from their influence, many SSK studies do not say as much about the activities at the edges of science. Looking *into* the constitution of scientific knowledge has provided a rich seam of literature which helps us to understand the contingencies of science as a social activity.[6] But there is still much work to be done looking *around* science, where scientists and the public are forced into problematic and highly asymmetric

---

[5] Bruno Latour, coiner of the phrase 'passage point' is a hugely influential contributor to the STS field. While many authors fully subscribe to Actor Network Theory, developed by Latour and others, many more see Latour's major contribution in providing a "descriptive vocabulary for construing scientific success and power" (Shapin 1995, p. 307). In effect, his enrolment is working better than his control (ibid., note 17).

[6] Scientific *knowledge* had previously been considered unworthy of sociological attention. Early attempts to explain knowledge in social terms formed a 'sociology of error' (Bloor [1976] 1991, p. 12), providing socio-political explanations for scientific deviance from truth. This represented a positivist exercise in which, "It is the task of the social scientist to investigate the psychological, cultural and social foundations of the popular adherence to such scientifically disproven treatments" p. 510 (Martin and Richards 1995, p. 510). The symmetry postulate (see Bloor [1976] 1991, p. 7) of the strong programme opens up 'good science' to investigation, removing any assumption that it might be 'value-free'.

relationships. These relationships are built on trust. The public, if they want to live calmly and benefit from the innovation science contributes to, must place a degree of trust in expertise, but this trust must be built, rather than assumed.

The times when the question of public trust resurfaces tend to be when there is an element of disagreement among the experts we rely on. Scientific controversies call into question the aspects of science which might be taken for granted, such as the nature of consensus, uncertainty and credibility, and the difficulties of experimentation. Controversies have therefore provided a rich seam for research within science and technology studies.[7] As Harry Collins says, it is only by examining scientific controversies while they are in progress that the mechanism by which ships (scientific findings) get into bottles (validity) can be understood. "If this process is not seen in operation it may be thought that the ships were always in the bottles, and that all scientists did was to find them ready assembled, as it were" (Collins 1981, p. 45). Controversies, revealing expertise as plural, fractured voices, can lead the public to justifiably question the source and the extent of their reliance upon experts. Controversies provide a site for the public to engage in 'informal technology assessment' (Rip 1987). In this sense, a public science controversy allows both the public and scientists to gain a wider and deeper appreciation of the risks, benefits and interests. From the perspective of a social researcher, controversies provide a valuable opportunity to observe the political dimensions of debates that are often obscured or inaccessible (Petersen and Markle 1989, p. 7; also Brante 1993). This is because "debates *within* science are simultaneously debates *about* science and how it should be done – or who should be doing it" (Epstein 1996, p.3, original emphasis). Issues involving risk and expert disagreement, such as the mobile phones health debate, are thus easily prised open by social research.

However, we should not assume that a scientific controversy is purely scientific. As we will see later in this chapter, and through the bulk of this thesis, the uncertainty that is revealed by controversy is not purely a scientific creation (see section on uncertainty and ignorance in public science, below). Nor is its resolution a purely scientific task. A 'scientific' controversy might hide a multitude of political sins and public disenchantment which will contribute to its development, and ultimately decide its closure.

Although this thesis draws upon the school of thought that has emerged from the sociology of scientific knowledge, I question the applicability of some of SSK's more well-known insights (particularly the notion of 'core-sets' of experts) to the study of 'public science', a term I will develop over the next few pages. First, I will use the work of Harry Collins to illustrate some possible limitations of SSK-in-public. I will then, using the work of Brian Wynne and other writers, illustrate a more constructive way of looking at public science, reconnecting SSK with recent insights from the related study of the Public Understanding of Science.

SSK and core-sets in public

The sociology of scientific knowledge tradition has provided some useful insights into how facts are 'black-boxed' by groups of scientists before being presented to those outside. This communication between groups of scientists and others has occupied much of the most influential thought in STS. Collins uses the term 'core-set' to refer to "the group of technically informed specialists who participate in the resolution of scientific controversy through their esoteric technical activity" (Collins 1988, p. 728).[8] It is only the core-set who fully understand the uncertainties inherent in their activities as they negotiate their way around the limitations of experiment and replication, towards the closure of disputes and the creation of certifiable, consensual knowledge (Collins 1985 p. 143).[9]

In a recent article with Robert Evans, Collins brings the core-set model further into the public domain, attempting a representation of the problems of lay and expert input during the process of scientific controversies (Collins and Evans 2002). As we move away from the core-set into the larger scientific community, in a normal scientific situation, the doubts and disputes of research are suppressed, with knowledge acquiring certainty and facticity as it moves into the public domain. In this model, the core-set are an inner circle, with concentric outer rings representing first the scientific community and then the public (ibid. pp. 244-5). For normal

---

[7] For a review of social explanations of scientific controversy, see Mercer (1996)

[8] This quote is taken from one of a pair of papers by Collins looking at the interactions between the core-set and the public. For the other, see Collins (1987).

[9] Michael and Birke (1994) make the point that "this set of persons does not necessarily act as a "group". They are bound only by their close, if differing, interests in the controversy's outcome." (note 1)

scientific situations, in which non-experts do not show much interest, knowledge is communicated outwards, and formalised (certified) as it travels past the eyes of other scientists.

According to Collins and Evans, problems occur for science when the public are able to sneak a look at science before any kind of consensus has been reached, before a 'black box' can be sealed. This is the domain of public science, and it raises myriad questions about the relationship between science and the public. However, Collins' (and Evans') model, based around a core-set of experts, is ill-equipped to provide answers. In an earlier article, Collins footnotes a comment from Trevor Pinch that core-sets cannot be applied to technology because public involvement is necessary for closure (Collins 1988, note 10). As we will see, in the realm of public science, similar doubts emerge. The types of issue that are likely to involve the public are those in which experts alone cannot negotiate closure. This thesis will argue that core-sets of experts become meaningless when an issue is recast through public engagement.

As well as being informed by the evidence presented throughout this thesis, my criticisms of models base around core-sets of experts 'becoming' public are informed by other STS perspectives that deal explicitly with the *public* nature of public science. Hilary Arksey studied an issue that shares some important features (such as expert confusion and public accumulation of evidence) with the mobile phones debate. Through looking at the construction of a contested condition, Repetitive Strain Injury, Arksey concludes that both the descriptive and (implicitly) normative elements of Collins' work are unhelpful in informing public science (Arksey 1998, p. 173). Collins' work has tended to rely upon esoteric scientific pursuits rather than public ones (a distinction he makes himself (Collins and Evans 2002)). In esoteric science, the boundary of the core-set will largely match the boundary of the problem, because of the lack of external interest. Controversies are therefore opened and closed by experts. In public, however, problems can be extended beyond expert control, at which point it is no longer clear that whatever core-set might once have existed should remain solely responsible for determining the outcome of the issue. As we will see throughout this thesis, the role of the public, as providers of evidence, experience or expertise, fundamentally reshapes the construction of expertise and controversies.

SSK and the Public Understanding of Science

Brian Wynne has made the point that the strand of SSK that developed from the work of Barnes, Bloor, Edge, Collins et al needs development if it is to offer insights about science in the public sphere on a par with those that it offers about the production of knowledge within scientific communities (Wynne 1993, p. 331). Wynne commends Actor Network Theory, the other major strand within constructivist studies of knowledge, for its location of science in the public domain, and its refusal to assume easy boundaries between science and society.[10] But he points out that because enrolment in networks is mutual, the interests and identities of actors overlap (ibid.). We can therefore say little about the asymmetries of relationships with expertise. ANT is "disempowering and essentially conservative in its effects" (Richards 1996, p. 328). (I will return to the theme of expert and public enrolment in chapter five). Wynne suggests that both SSK and ANT might benefit from the incorporation of research within the new but poorly defined field of the Public Understanding of Science (PUS). This has helped to show that the public, far from being passive receptors of expertise and purveyors of trust, react to public science in an active, reflexive manner.

'Public Understanding of Science' emerged in the USA from attempts to improve the public *appreciation* of science and scientific institutions (Lewenstein 1992), sensing a loss of public attention and confidence. In the UK, the Royal Society (1985) report, which formally framed the problem, suggested that scientists needed to communicate more effectively with The Public to boost wider understanding of scientific issues. As well as becoming more au fait with scientific knowledge (such as the orbits of the planets or the causes of tides), it was hoped that greater scientific understanding would improve The Public's appreciation of risk issues. One of the Royal Society's recommendations suggests that:

> "Understanding the nature of risks and uncertainty is an important part of the scientific understanding needed both for many public policy issues and for everyday decisions in our personal lives... Once again it must be argued that better understanding fosters better public and personal decisions" (Royal Society 1985, p. 10, paragraph 2.10)

---

[10] For the debate between Collins' brand of SSK and Latour's ANT, see the chapters of Pickering (1992)

...the implication being that it is the responsibility of scientists, as objective assessors of risk, to warn or reassure the public about the real risks they face. While few people would doubt that the answer to the question, "How do we make the public understand science?" is, "Teach them about it," recent attempts to better understand how the public understand science have asked very different questions. These questions have largely been informed by insights gained from SSK, which is in a unique position to contribute to understanding of the science-public relationship (see Yearley 1994).

Firstly, it has been suggested that the public should understand how science *really works* rather than what science *is*, or what it would like to be (Shapin 1992; Collins and Pinch, [1993] 1998, pp. 140-3). Such an education would be courtesy of science studiers, exposing the politics within and around science, to better enable the public to make informed judgements, possibly more informed than the scientists (whose views may be clouded by professional commitments). This perspective prescribes teaching the public as 'faulty sociologists' rather than 'faulty scientists' (Locke 2002, p. 90; see also Locke 1999, p. 79).

But this still does not accept the possibility of a two-way relationship between the public and their experts. Once we problematise the social nature of scientific expertise, we can illuminate more clearly its place in society. In doing so, we are implicitly critiquing what is now referred to as the 'deficit' model of public understanding, epitomised by the Royal Society report mentioned above.[11] Rather than an attempt to understand negotiations between science and the public, the deficit model serves only to formalise the implicit constructions of the public often harboured by experts in their interactions with the public (Irwin and Wynne 1996, p.9, pp. 241-2).[12] The constructions of the public implicit in naïve models of science and society illustrate science's lack of reflexivity (Wynne 1993, p. 330). To really observe the dynamics of public science, we should treat the public and scientists symmetrically, assuming that the public can interact with science with a degree of 'lay expertise' (e.g. Irwin and Wynne 1996, Kerr et al 1998; Epstein 1996).

---

[11] For more examples of deficit-style PUS thinking, see Wolpert (1992), Dawkins (1998).

[12] Steve Woolgar has also discussed the degree to which experts (in his case computer engineers) construct homogenised images of the public (users) and their relationship with technology (Woolgar 1991).

A socially-enlightened approach to theorising the relationship between science and the public allows us to observe the public responding actively to scientific information or practice. The public's reaction to expert advice is likely to depend upon previous encounters with expertise (Wynne 1996b) a feature that has become clear in the wake of the advisory mistakes over BSE (see chapter five). Public frameworks of trust, built up over time, help them reflexively interpret expert advice for their own needs. But the public might have no choice but to behave *as if* they trust expertise (Wynne 1996c), which will further lessen their chances of contributing to decision-making. There is evidence to suggest that the public might actively maintain an ignorance of issues which scientists think should concern them (Michael 1996; Wynne 1995). This might be because of constraints of time or effort, because the public do not want to discover uncertainties which may prove upsetting, or because they feel powerless (Wynne 1995, p. 380; Wynne 1991, p. 118). For some, it appears, ignorance might be bliss. But it would be a mistake to interpret this ignorance as an educational hole that needs filling. It could be indicative of an active disenchantment with expert advice. We might expect that, with a hugely popular technology such as the mobile phone, some people will actively maintain ignorance of the (public) science that suggests worrying uncertainties.

Questioning Expertise

Both the sociology and the philosophy of science show that scientific knowledge is indeterminate in providing descriptions of nature or prescriptions for action[13]. Science can however powerfully inform technical decision-making. But numerous case studies of regulatory science have shown that, while policy would like to rely on science as a value-free source of authority the pressures exerted by political demands will remove any 'neutrality' that scientific evidence might once have had (e.g. Nelkin 1975, Jasanoff 1990).

In cases of public science, once we adopt the constructivist approach of denying the neutrality of expertise, we can begin to see better the values and interests shaping aspects of a debate that might otherwise have been ignored as 'technical.' Expertise,

[13] 'Underdetermination' or the "Duhem-Quine thesis" is a concept from the philosophy of science, describing the insufficiency of data in supporting or rejecting theories. It is a useful crossover point between social and philosophical studies of science (Laudan 1998, p. 321).

rather than being relied upon for contributions to 'correct' policy decisions ("speaking truth to power" (see Wildavsky 1979)), can instead be used to add weight to a predetermined position (Nelkin 1975)[14], giving the impression that a decision is technocratic.[15] This has led some commentators to observe that science is largely irrelevant in decision-making. It is either accepted if it supports a policy consensus, or obfuscated in technical debate if it proves problematic (Collingridge and Reeve 1986). This view might seem cynical, but it provides a useful heuristic with which to look at the actions of advisory scientists and the technocratic justifications for policy. As we will see in later chapters, there is no one true road from 'sound science' to 'sound policy'. But there might be better or worse ways of using the available scientific knowledge in credible policy-making.

As science and technology, and their accompanying styles of reasoning and rhetoric, seep more and more into life outside the laboratory, the boundary between *technical* and *social* is blurred. It is not clear where scientific knowledge ends and political decisions begin. The underlying political nature of technical decision-making is one of a number of arguments which prompt calls for extended public involvement in decision-making (Shrader-Frechette 1995). The more uncertain the science, as we will see later, or the more controversial the circumstances, the more important public participation becomes in decision-making (ibid., also Funtowicz and Ravetz 1992). Daniel Fiorino has classified arguments for greater public participation in expert decision-making under three headings: *Normative* – that representation of the public is in itself a good thing according to democratic ideals, *Substantive* – that lay assessments of risk are valid and useful in decision-making, and *Instrumental* – that the appearance of greater public involvement makes decisions more legitimate (Fiorino 1990, pp. 227-8).

In later chapters, I will explore the attempts made by experts to engage the public in a broader advisory process. But this engagement may not easily be described as either normative, substantive or instrumental. Negotiations over expert/non-expert

---

[14] Denying the political neutrality of expertise allows us to account for policy decisions in the past which drew upon the same, globally-recognised 'sound science', but produced very different local responses (e.g. Gillespie, Eva and Johnston 1979, Van Zwanenberg and Millstone 2000).

[15] See Millstone and Van Zwanenerg (2001) for an illustration of the BSE/CJD fiasco: a perfect example of the power and dangers of using expertise to support political decision-making.

interactions are likely to contain elements of all three. As the recognition of the importance of the public in decision-making grows, along with a recognition of a 'crisis in trust', we should be careful to closely analyse any claim of public engagement. As Wynne has demonstrated, mechanisms such as public inquiries can reinforce asymmetries of expertise. Though the public might see inquiries as an important forum, they can have little real impact on expertise (Wynne 1982, p. 72). They therefore become another mechanism for social control. When investigating the contributions of the public in public science, we should therefore bear in mind an important caveat. It is easy for scientific advice to pay lip service to public participation, and the temptation grows stronger as the image of public science moves through a 'political turn' (Cozzens and Woodhouse 1995, p.533; also Michael 1992). This does not mean that control of knowledge is moving towards the public, or even being negotiated in public.

Returning to Actor Network Theory, we can see how the idea of mutual enrolment might be applied to expert engagement with the public. Michael and Birke discuss a group of experts enrolling certain animal rights interest groups into their 'core-set', partly in order to marginalise more extreme groups and partly to increase the legitimacy of their work (Michael and Birke 1994). Their story is an example of the redefinition of a core set through public engagement. However, they suggest that the term 'envelopment' might be more illustrative than 'enrolment' (ibid. p. 92). As we saw above, Actor Network Theory in its most pure form is bad at explaining power asymmetries and the role of interests in such disputes.

The potential of the public to engage in knowledge production has emerged as a key feature of recent science studies literature. Phil Brown talks about the emergence of 'popular epidemiology' in public health issues (Brown 1987, 1992). Brown conceptualises popular epidemiology as the "process by which laypersons gather scientific data and other information, and also direct and marshal the knowledge and resources of experts in order to understand the epidemiology of disease" (Brown 1992, p. 269). The important element of his argument is not, however, the intrusion of lay knowledge acquisition, but the clash of perspectives. Scientists and lay people tend to have very different priorities at all stages of the scientific and policy processes. While these differing understandings may hinder communication, the wider base of knowledge production can be beneficial, especially in questioning the framing assumptions which underlie risk assessments. Lay involvement will also help highlight incidences of 'bad science' or suppressed knowledge, and it will help demonstrate the limitations of normal science in investigating ignorance (ibid., p. 277). Science can obviously not sceptically investigate every knowledge claim, so there will likely be a strong element of 'epistemological dilution'. But, as Brown points out, this is the price that must be paid for past expert policy failures (Brown 1987, p 278). It also leads us to rethink whether epistemological *purity* should be the priority of public scientists.

Steven Epstein has shown, through his study of AIDS activists in the USA, that the public can play an important role in shaping the production of knowledge about disease (Epstein 1996). The aim of the activists in this case was to mould science into a more socially-relevant form, at the expense of what the scientists claimed was 'good science' (moving from 'pure science' to 'impure science'). But in order to interact effectively with scientists, some activists had to become 'lay experts' in the field, reducing the asymmetry that would have prevented communication. The story here, while indicating a small victory for lay involvement, is of the public playing scientists at their own game (albeit with distinct motives) rather than contributing an alternative epistemological stance. Without a degree of expertise, and the credibility that comes with it, their voices in this controversy might not have been heard.

Cases of public knowledge contributions, coupled with a critical, constructivist view of expertise give us a more open view of expertise. Michel Callon gives his impression of what these developments have meant...

> "Since science is at best incomplete, at worst unrealistic and, in any event, incapable of accounting for the complexity of the specific problems to which it is applied, it is advisable to open the forum for discussion and deliberation so as to create the conditions of its enrichment." (Callon 1999, p. 86)[16]

Callon presents us with our current malaise. The interactions between expertise and the public can take one of three forms. Model 1, in which the public are educated and public knowledge is ignored as substandard (akin to the deficit PUS model described above). Model 2, in which it is appreciated that the public are useful in

[16] This article is an enlightening attempt to review and synthesise the insights of three decades of science studies. Oddly, for a review article, it only gives two references.

questioning framing assumptions and providing complementary knowledge (Epstein's story above might be an example). And Model 3, which represents tension between science and the public, between standardised, universal knowledge and knowledge that is locally applicable and contextual (Callon 1999). Crucially, the third model, ignoring questions of its applicability, would induce no crisis of trust, as there is no attempt by experts to redefine identities of different publics (ibid.). Model 1, which *relies* on public trust, undermines itself. It is powerless to address a loss of trust in expertise[17].

The negotiation of knowledge with non-experts puts public science in a difficult position. It is called upon to produce authoritative scientific advice, but it cannot do so in a political vacuum. Public science has to negotiate its identity between the private, scientific world and the political, accountable world. As we will see, the public science of finding out about the risks of mobile phones also involves finding out about the limits of expertise and the limits of public trust.

Protecting Boundaries

Citadels of expertise, when they are forced into a hybrid public domain such as scientific advice, find that they are under attack (or at least under pressure). Policy intervention is likely to undermine the constructed certainties of scientific knowledge, unveiling its fragility (Jasanoff 1987, 1990) [18]. Scientists in such circumstances might be justifiably expected to defend their status as the source of cognitive authority both within science and outside it (Barnes and Edge 1982, p.2). One of the ways they do this is through what Tom Gieryn calls 'boundary work' (Gieryn 1983). Gieryn and others, rather than asking the essentialist question of 'what makes science special?', ask 'what do scientists do to make science special?' (see also Jasanoff 1987). Boundary work is the collection of rhetorical tactics that scientists use to place things inside or outside 'science'. External pressures placed upon science, such as the mixing of knowledge with authoritative advice, will lead

---

[17] This is a similar argument to the reflexive modernisation thesis, in which the institutions of modernity highlight their own weaknesses (see Beck 1992, Giddens 1996, ch. 2)

[18] Jasanoff makes the point that this is especially true in the US, where scientific evidence is *deconstructed* in public, policy and legal fora: "a process that tends to fragment expert opinions and observations into their most elementary analytical constituents and to illuminate constantly changing frontiers of scientific uncertainty and disagreement" (Jasanoff 1989, p. 153).

scientists to rethink the nature of scientific authority and so work their boundaries in new ways.

Scientific experts tend to rhetorically construct *what science is*, to clarify that more problematic areas are actually *politics* or *transscience* (Jasanoff 1987; see also Weinberg 1972). Boundary work analyses have therefore proved very useful in explaining the various constructions of science and politics that occur in the hybrid science/public domain of scientific advice. Sheila Jasanoff notes in her studies of regulatory science that successful boundary work is necessary for scientific advice to hold political legitimacy. Maintenance of this boundary lets scientists refuse entry to non-scientists, so establishing a complete cognitive monopoly. Successful boundary work can mean that a regulatory organisation with scientific and political aims can be both scientifically and politically legitimate by demarcating its activities into one sphere or the other (Jasanoff 1990, p. 236).

However, scientists, while rhetorically demarcating their work to avoid criticism and maintain authority, display an active desire to extend control of their knowledge and ways of understanding into the public or policy domain. David Rier notes that epidemiologists, whose work often attracts media attention, tend to add caveats to papers aiming to control how their findings are interpreted (Rier 1999). But the omni-directional communication of science across the constructed boundary is more complex than scientists might maintain (Hilgartner 1990). Not only do 'popular' representations of scientific knowledge feed into all parts of knowledge production (ibid., pp. 524-8), but scientists also deploy rhetoric of popularisation to defend the 'real' nature of their work (ibid., pp. 530-533).

Expert attempts to maintain effective boundaries (distancing themselves from political debates), while controlling the flows of information (engaging themselves) will always be highly complex in public science. This is especially problematic with the increasing role of 'hybrid' science/policy/regulatory institutions that necessarily work in all areas (Jasanoff 1990, Hilgartner 2000). As we will see in Chapter four with the case of the NRPB, such institutions, and their relationships with explicitly policy-making bodies and with ad hoc advisory committees, tell us a great deal about the boundary problems encountered by scientific advice. The building and maintenance work that occurs at the boundaries between science and the public defines the relationship between the two sides. We will see later in this thesis how different approaches to this boundary produce very different advisory responses, influenced by the scale of uncertainty highlighted by public controversy.

Uncertainty and Ignorance in public science

> "The message is that there are known knowns. There are things that we know that we know. There are known unknowns. That is to say there are things that we now know we don't know. But there are also unknown unknowns. There are things we don't know we don't know. So when we do the best we can and pull all this information together, and we then say well, that's basically what we see as the situation, that is really only the known knowns and the known unknowns. And each year, we discover a few more of those unknown unknowns."[19]

This speech, from US Defense secretary Donald Rumsfeld during America's 'War on Terror', is partly intentional obfuscation and partly an attempted explanation of the problem of policy decisions when it ceases to be clear what exactly it is that decision-makers don't know. Rumsfeld oratorical knots were received with derision at the time, but his comments suggest that the accumulation of evidence to support a decision (in this case intelligence to support military action) rests largely on the identification of what we don't know – what dangers *might* exist. In public controversies such as that over mobile phones, the control and rhetorical deployment of uncertainty is just as powerful as the control of certified evidence to support a case.

Uncertainty and ignorance – areas without consensus or without knowledge – provide a target for the production of scientific knowledge. Uncertainty represents gaps to be filled in. It is as crucial to the scientific representation of reality as knowledge is (Stocking 1998). It has attracted a good deal of science and technology studies attention from those who feel that the study of knowledge necessarily involves considering areas of non-knowledge.

Before looking at the constructions of uncertainty that emerge from social studies of science, we should acknowledge the intricacies of what it means not to know[20].

---

[19] February 12, 2002, US Department of Defense news briefing on intelligence about terrorism.

[20] This problem was recognised by economists such as Keynes (1936) and Shackle (1955), who, in attempting to account for the role of expectations in determining phenomena such as interest rates,

Wynne (1992) has identified four areas of incomplete knowledge, and their implications. These are risk, uncertainty, ignorance and indeterminacy. Science and technology can adequately deal with concepts of risk and uncertainty (indeed these concepts can help define what science *is*). But ignorance and indeterminacy (where we are not aware, or can never be aware, of what we don't know) are more problematic, and science alone cannot provide solutions (ibid.). My discussions of uncertainty and ignorance will flit between the two terms. I am, however, looking at how scientists go about constructing their grey areas, and it is apparent that areas of ignorance and indeterminacy are often represented and treated as though they were simple risks or uncertainties (Shackley and Wynne 1996) (I refer you back to the Royal Society's comments on PUS: "Understanding the nature of risks and uncertainty is an important part of the scientific understanding needed both for many public policy issues and for everyday decisions in our personal lives" (Royal Society 1985, p. 10)). My purpose, as a social researcher, is not to evaluate the 'real' state of knowledge or degree of indeterminacy. And, as will become clear in later chapters, there is no 'real' uncertainty in a public science debate. Uncertainty only exists as that which emerges from negotiations about the adequacy and relevance of current knowledge.

Constructivist commentators have considered how uncertainty is represented within science and outside it. Uncertainty is part of the normal course of all scientific investigations (Wynne 1992), but its representation tends to vary as we move into the public domain. MacKenzie (1990) describes a 'certainty trough' for technological uncertainty as an analogous model to Collins' depiction of uncertainty decreasing with distance from the research front. 'Distance lends enchantment' has become one of the central tenets within the SSK canon. Collins ascribes this reduction in uncertainty to the core-set's appreciation of the craft-based nature of scientific practice (Collins 1987), which gets lost when facts are black-boxed for external presentation. Star has pointed out that the management of local uncertainties, while striving for global certainty, is one of the central aspects which defines scientific work (Star 1985, p. 393). We can begin to see that uncertainty is relevant at many

---

investment and employment, emphasised the presence of 'real uncertainty' as a concept that could not be reduced to probabilities. Central to Shackle's theory of uncertainty was the concept of 'surprise' which, by definition, is unexpected and cannot be built into models (Shackle 1955, pp. 56-62).

levels. As we will see in later chapters, the uncertainties that are problematic for scientists at a laboratory level do not necessarily bear any resemblance to the uncertainties discussed during advisory decision-making.

The ideas presented so far fit with the majority of studies of scientific uncertainty in seeing uncertainty as an embarrassment that should be preferably hidden to preserve authority (Martin and Richards 1995). In a public context, we can see why admissions of uncertainty could be harmful to the authority of advisors. After all, what is the point of seeking authoritative, expert advice if the advisors are plagued by the same uncertainties that concern the public and policymakers? (Zehr 1999).

Other commentators have attempted to explain apparent divergences in the understanding of uncertainty between science and the public. Though much of this literature (e.g. Zehr 1999, 2000) has helped to improve understanding of the relationship between science, the public and the media, some ideas and prescriptions have tended to suffer from assuming that, because scientists often disguise uncertainties, the public are unused to scientific uncertainty, and therefore bad at handling it rationally. One such attempt to theorise the science/policy relationship with respect to uncertainty (Bradshaw and Borchers 2000) sees public confidence as lagging behind scientific confidence (a type of constructed certainty), leading to problems in science communication. One of their suggested solutions for alleviating this 'science policy gap' is to "realign the definition of scientific uncertainty as perceived by the public and policy makers with that of the science community" (ibid.)[21] This recommendation allows us to point out the limitations of a naive concept of scientific ignorance. Such a prescription comes from assuming that, while uncertainties and complexities are features inherent in the processes of science (Wynne 1987), in time, scientists will erase the uncertainties and better understand the complexities. Implicit, also, is the assumption that there is only one correct view of uncertainty, and this lies within the realm of scientific knowledge production. This thesis considers uncertainties about mobile phone risks as flexible, with the potential for expansion as well as reduction, and mouldable by groups outside science. Uncertainties are seen as contributory resources in the production of public

---

[21] See Stilgoe (2001a) for a brief critique of some of the features of this model. The model assumes that the public will increase their confidence as they learn more about the relevant science. SSK and recent PUS research have shown that this is likely to be far from the case.

science about mobile phones. Some recent STS work has considered how scientists actively construct uncertainties for specific ends, with various constructions of uncertainty coming together in scientific disputes.

Constructing Ignorance

Scientific knowledge does not reduce ignorance in any general way. While certain research might lessen targeted areas of uncertainty, scientific investigation expands the potential for further investigation (Nelkin 1979, Ravetz 1987, Funtowicz and Ravetz 1990) shedding light on new areas of ignorance. Ignorance is therefore a product of scientific activity, (often produced in a quest to reduce uncertainty (Wynne 1992)) rather than a target (Smithson 1989). Technological innovation will have a similar impact, opening up new chasms of ignorance. Ignorance is constructed at a *cognitive-strategic* level (Wynne 2001, p.7), as a product of unbounded scientific curiosity.

To better understand scientific and policy debates in nascent, contested scientific areas, we need to follow the lead of a few recent commentators within science and technology studies and adapt the tools developed for understanding knowledge to look at ignorance, and how it is constructed on a more rhetorical level. We begin with the appreciation that "uncertainty *as such* is not a problem" (Wynne 1987, p.95, original emphasis). It becomes a problem when someone authoritatively decides that it has problematic implications (ibid.). For it to be considered an issue of concern, at some stage the level and nature of uncertainty must be authoritatively declared. This authority becomes the focus of constructivist studies of uncertainty.

One of the targets of SSK is the control that scientists are observed to aim for over knowledge. The boundary-work scientists engage in is a way of protecting knowledge from slipping out of their control, which might lessen its cognitive authority (Gieryn 1995). The control of ignorance is more difficult, because 'what we don't know' will always be contested ground. But it is just as important for marshalling the production of knowledge. Scientists will aim to have technical control of uncertainty, specifying areas for attention (Merton 1987) and fashioning it into manageable, do-able, problems (Fujimura 1987). But they will also aim for social control of uncertainty (Wynne 1987), as there is always a danger that 'technical uncertainty' may become, or be perceived as, 'social uncertainty' (Jasanoff and

Wynne 1998). An example of exercising social control is provided by Balmer (2000), who notes that scientists in biological weapons programmes invoked social uncertainty in the form of 'fear of the possible', as part of a strategy to maintain political support. Such claims raise the issue, which will be discussed below, of what is at stake in decisions under uncertainty.

Well-managed scientific uncertainty can become a resource for cognitive authority rather than a problem (Wynne 1987). For policy prescription, the deployment of uncertainty claims can be especially authoritative when used to support the status quo (Campbell 1985). Just as with scientific knowledge, constructing uncertainty is a vital part of maintaining relationships and identities in science and policy (Jasanoff and Wynne 1998, p. 76). In public, however, these identities can be challenged by reconstructions of uncertainty that originate from outside whatever 'core-set' might previously have existed.

Uncertainty in public

In public, managing uncertainty becomes more problematic, because, as scientists know 'more and more about less and less', they are likely to possess a significant level of ignorance about science outside their speciality (Fuller 1993, ch.2). Public science is often interdisciplinary and, as we will see in this thesis, there is sometimes no easily-identifiable core-set, so authority under uncertainty is a huge challenge. This is why regulatory networks are likely in uncertain, dynamic debates to 'anchor' themselves on an area of consensus, around which policy can be discussed in a constructive way (Van der Sluijs et al 1998).

Uncertain situations justify the appreciation of a diverse set of knowledges and a questioning of framing assumptions (what Funtowicz and Ravetz (1992) refer to as 'extended peer-review'). Uncertainty will therefore be negotiated within the public sphere. But, as Stephen Zehr has pointed out, "scientific uncertainty is not only a reflection of what scientists are uncertain about; rather...it can be constructed through scientists individually clear and concise but variable accounts of nature" (Zehr 1994, p. 215). The public reconstruction of expertise is likely to fashion new, public uncertainties in science.

The active construction and deployment of uncertainty has been the focus of a more recent literature in the sociology of scientific knowledge[22]. As mentioned above, it would be a mistake to assume that controversies are purely scientific creations which fuel public unease. Brian Campbell (1985), begins his account of a public science controversy by saying that, while it is tempting to blame controversies on uncertainty, it is more constructive to see uncertainty as constructed in the course of public disputes, just as knowledge is (see also Wynne 1982, p. 64). We can then consider the reasons behind such constructions, and look at how uncertainty claims are managed by scientists to increase or preserve their cognitive authority, rather than allow it to be undermined. This is the essence of a constructivist view of ignorance.

Scientists rhetorically construct areas of uncertainty and ignorance in the day-to-day production of scientific knowledge. Devices such as identifying "gaps in the literature" (Zehr 2000) and "areas of concern" are used to justify investigation and better locate their work around the boundary between existing knowledge and ignorance. Shackley and Wynne (1996) have pointed out that the rhetorical management of uncertainty is vital in preserving ordered boundaries between science and the public. Zehr (2000) has also noted that uncertainty can be used to help separate scientific and public worlds, by emphasising the authoritative definition of the state of the science and necessary directions for exploration of areas of ignorance. But it also gives different communities easy access to public scientific debates, as it provides a common area for discussion (Shackley and Wynne 1996).

Uncertainty and ignorance in public science disputes is a crucial area of debate. Firstly because of the complexity of problems encountered by recent public science. Such situations are multidimensional and often deal with poorly-understood environmental systems, so reductionist science can fail to address the scale of our ignorance (Wynne and Mayer 1993). Secondly, authoritative claims of uncertainty can set the agenda for discussion just as knowledge claims can. Issues can often be *ignored* through ignorance, while others are targeted as relevant for further research. Uncertainty determines the questions that might be asked and the possible range of policies that might be implemented.

---

[22] ...or the sociology of scientific ignorance as some commentators would have it (see Stocking 1998)

Policy in uncertain times

Uncertainty about mobile phone risks points the way for further research. But it also suggests that policy might intervene to prevent unwanted effects if our lack of knowledge is sufficiently great. Scientific knowledge about the health effects of mobile phones might be insufficient to provide a reasonable assurance of safety.

We do not need to take an SSK perspective to see that there are situations in which science cannot answer all that is asked of it. Alvin Weinberg's conception of such situations, which he calls 'trans-scientific', does not problematise 'real' science at all[23], yet it appreciates the need for changing features of sound science, such as a burden of proof, to accommodate the demands of policy and the problems of uncertainty (Weinberg 1972). In the last few decades, such thinking has been furthered by a constructivist picture of science providing a clearer picture of what it means to be uncertain, and by formal expressions of precautionary styles of environmental protection.

Funtowicz and Ravetz provide a normative picture of uncertain science. In situations of what they call 'post-normal science', there is not only scientific uncertainty, but also high decision stakes (Funtowicz and Ravetz 1992). While this model does not include any multi-dimensional notions of uncertainty such as the constructivist ideas mentioned above, it does provide a robust recommendation for 'extended peer review', incorporating lay knowledges and perspectives, as a means to mitigate the problems of making important decisions based upon incomplete information. Before continuing, I should note one criticism of the post-normal science model. Authors have questioned whether the two variables of uncertainty and decision stakes can be seen as independent or whether they control one another to an extent (Yearley 2000, Jasanoff and Wynne 1998). A more constructivist viewpoint (e.g. Wynne 1992) would suggest that the degree of uncertainty in a public controversy is determined largely by what is at stake. As we will see later in this thesis, the stakes involved in policy decisions can be key determinants of the uncertainties which emerge as salient.

---

[23] From an SSK perspective, this maintenance of a domain of real science, removed from trans-science, is boundary work, as Jasanoff has pointed out (Jasanoff 1987).

The categorisation of uncertainty provided by, amongst others, Brian Wynne has some important implications for policy (see Wynne 1992, Stirling 1999). Once we problematise uncertainty by noting that ignorance and indeterminacy cannot be seen as linear extensions of uncertainty, we can justify qualitatively different, precautionary policies.

Precaution is flexibly defined, based around a principle which has a number of formulations. But precaution encompasses some crucial features which differentiate it from narrow, risk-based assessments, allowing it to play a proactive, rather than responsive, role in technological development (Stirling 1999). The precautionary *principle* originally emerged as a concept from German environmental science, justifying action to prevent serious environmental harm in the future, even when no strong proof existed of a threat (Harremoës et al 2002, p. 13). This is not to say that it is not science-based. As Stirling has pointed out, precaution merely asks for a broader definition of science (Stirling 1999). Constructivist perspectives on science can therefore contribute a great deal to policy under uncertainty.

It may be tempting to argue that, if uncertainty is constructed by different groups according to their interests, then policy responses based upon an identified degree of uncertainty will never be robust (Wynne 1992). Just as uncertainty is used to justify precaution, so precautionary approaches might be used to justify uncertainties (Levidow 2001). Uncertainty and ignorance are multidimensional, contested areas. There is no easy way of maintaining control over the construction or removal of uncertainties in public science. The role of policy under uncertainty might not therefore be to measure and respond to uncertainty, but to incorporate diverse perspectives proactively into policy to reduce the possibility of surprise in the future.

Discussions of the relevance of uncertainty to interventionist, precautionary policies are often laden with debate about what counts as uncertainty. We have discussed above what it means not to know about something, but worrying uncertainties are necessarily fuelled by suggestive evidence. In order to be suspicious of whether GM foods, BSE-infected beef or mobile phones are harmful (as opposed to, maybe, face cream or CD players), our suspicions need to be aroused by some form of evidence. Just as the condition of not-knowing is part of the context of knowledge, so evidence is part of the context of not-knowing. As Donald Rumsfeld might put it, we need to know that we don't know. The construction of uncertainty is thus tied to the assessment of evidence, in its many guises.

Standards of evidence in public science

Science and Evidence

In one sense, the majority of theorising which emerges from problematising scientific knowledge is about evidence: how evidence is accepted, rejected or ignored (e.g. Collins and Pinch 1979); or how evidence is gathered, displayed, trimmed and translated into knowledge (e.g. Latour 1987, Gilbert 1976). Science is a way of gathering, sifting and rearranging evidence. And different appreciations of what counts as evidence act as a major factor in causing or sustaining controversy (Engelhardt and Caplan 1987, p.11). But when we bring these ideas into a world of public science and 'evidence-based' policy, we see that there is room for explanations that might better account for the role of standards of evidence, formal and informal, at the boundaries between science, policy and the public.[24]

Constructivist studies of science allow us an insight into the processes through which evidence is accepted or rejected. Again, the SSK tradition has gone a long way, along with some work in the philosophy of science[25], to illustrating the importance of social context in the treatment of evidence. Collins and Pinch's (1979) study of the rejection of para-psychological evidence suggests that, through permeating the boundary between the constitutive and contingent forums for science, evidence can be rejected explicitly in the course of a scientific controversy, or implicitly, when it is ignored by orthodoxy (ibid., p. 239).

A paper by Caroline Joan Picart (1994) uses the Collins and Pinch framework to narrate the high-profile controversy involving Jacques Benveniste's claims in the late 1980s in support of the theory behind homeopathy. The suggested explanation for the closure of this controversy seems sound, but, as has been suggested in a response article, it might miss some crucial aspects. Judith Fadlon and Noah Lewin-

[24] On a reflexive note, in the light of the previous section, it should be clear that I am hereby justifying my own research by identifying (constructing) 'gaps in the literature.'

[25] See Longino (1990, ch. 3) and Feyerabend ([1975] 1993) for philosophical perspectives on the value-laden appreciation of evidence.

Epstein (1997) criticise the Picart account of the controversy for failing to recognise the importance of the science-public boundary in controversy and closure. They suggest that the explanation for the move from implicit to explicit rejection of evidence might lie *outside the scientific community* (Fadlon and Lewin-Epstein 1997, note 3). Picart focuses is on what is going on *within* science rather than *around* it. Fadlon and Lewin-Epstein's comments highlight a limitation of some of the most respected work in the SSK canon, which is similar to the criticisms I have suggested earlier in this chapter. Writers such as Collins tend to consider the sociology of issues within science (Wynne 1996a, p. 361), which limits the applicability of their insights to the more macro-political situations in which questions about risk, regulation and the public must be addressed. In this context, we must move away from looking at how scientists treat one another's evidence. We should instead begin to look at how evidence might travel, or be dragged, across the science-public boundary, in both directions, altering its appearance and its status.

We will see later in this thesis that a defining feature of the mobile phones controversy has been the lay reporting of symptoms attributed to mobile phone technologies. Expert and lay constructions of disease and illness can provide a fascinating site for observing knowledge and credibility struggles over what counts as acceptable evidence. We can most easily observe these tensions in contested or controversial diseases, which challenge scientific understanding through the experiences of sufferers. Zavestoski et al examine the dispute over the existence, aetiology and treatment of Gulf-War related diseases, and conclude that the experience of sufferers of an illness that is not accepted by what they call the 'dominant epidemiological paradigm', can actively contribute to knowledge about the disease (Zavestoski et al 2002, p. 200). But the sufferers in this case do not aim to simply be providers of evidence. They are also contributors to ethical and political debates about treatment, blame, trust and control. However, in order to have these concerns accepted, they must fall in line with science-based advice, downplaying their broader claims (ibid., p. 198). Their status is thus reduced to that of providers of evidence; evidence which falls outside current scientific thinking. We are dealing here with evidence at the science-public boundary. This is evidence at the bottom of the epistemological barrel – what scientists might refer to as 'anecdotal.'

Investigating Anecdotal Evidence

Part of the aim of this thesis is to further clarify what 'anecdotal evidence' might mean, so I will postpone a strict definition until later. For the time being, a workable definition might be *evidence gathered in an unscientific way, often in the form of a single example, sometimes presented as a form of story* (the contrast with science will be problematised later). My research into the nature and role of anecdotal evidence is prompted empirically, by a recommendation that it be paid proper attention in the case of mobile phones, but the topic resonates with much of the STS literature. The term 'anecdotal evidence' has received no explicit attention within science and technology studies, but its appearance in some cases helps us to give it a theoretical place. Wynne studied the scientific and occupational understandings of a pesticide, 2,4,5-T ('agent orange'). Users of the pesticide had reported cases of medical damage…

> "Throughout the 1970s the official Government Pesticides' Advisory Committee (PAC) conducted several inquiries and issued eight reassurances that there was no scientific evidence of harm. The forestry and farmworkers' arguments were dismissed as unscientific and largely anecdotal." (Wynne 1989, p. 36)

Wynne goes on to illustrate how this rejection of evidence is symptomatic of a naive view of the world outside the lab, in which usage of the pesticide was very different from that assumed by scientists in their risk assessments (ibid., p. 37).

Investigating anecdotal evidence is a useful way of observing interactions between scientists and the public, or among scientists themselves. Wynne's example above shows how the term can be used in boundary work, to cordon off areas of scientific risk assessment from intrusion. When evidence is 'dismissed as anecdotal', we should ask what the underlying social processes are. In this case, risk assessors dismissal of the evidence can be seen as an inability to reflexively question their own framing assumptions. Aside from Wynne's brief example, which is not really about anecdotal evidence per se, little mention is made of the term in science studies literature. But a number of science studies insights deal with similar issues, and these contribute to placing my discussion alongside ubiquitous themes such as public engagement, local knowledge and trust in expertise.

Ron Westrum provides a case which demonstrates how anecdotal evidence from French Peasants, demonstrating the reality of meteorites, was ignored by the scientific establishment of the time (Westrum 1978). Acceptance came only through the slow accumulation of reports and the scientific acceptance of plausible causes (ibid.). In effect, the public, anecdotal evidence was scientised,[26] a feature that I will discuss in detail in chapter six.

The case of the meteorites demonstrates one of the most salient features of evidence across the science-public boundary; that acceptance or rejection, far from being determined by quality or quantity of evidence, is contextual (ibid. p. 469). Collins and Pinch, in their study of battling factions in the parapsychology debate, note that some criticisms of parapsychological experimentation were based upon readings of what they call anecdotal evidence. This consisted of information never considered for publication, which should have moved it out of the constitutive forum of scientific debate (Collins and Pinch 1979, p. 257). For Collins and Pinch, this treatment of anecdotal evidence helps illustrate the scientists' flexible definition of what counts as 'real science'.

Just as with any rule in science (see Shapin 1995a, p.303-4), we might expect the application of standards of evidence to be contextual. In public, factors such as trust and status affect how anecdotal evidence from outsiders is received and treated. In a policy context, evidence that might be considered weak and anecdotal evidence can be used to support important decisions if it conforms with the current position of a policy body[27]. Abraham and Sheppard, in their description of the regulation of a drug, triazolam, point out that

> "...the PDAC [Psychopharmacological Drugs Advisory Committee] emphasized the *anecdotal* evidence of triazolam's *efficacy* in clinical practice, while discounting such

---

[26] On another reflexive note, Shapin, among others, has pointed out that we must look at the *large number* of case studies in the Sociology of Scientific Knowledge in order to see support for its theoretical claims (Shapin 1982, p. 157). Many individual studies, without any chance of independent validation, might be considered 'anecdotal'.

[27] Personal Communications: Kevin Quigley, Queen's University, Belfast, Sept 2002; Adrian Ely, University of Sussex, Sept, 2002. Susan Leigh Star has also pointed out the scientific usefulness of generalising from single cases to move from a position of local uncertainty towards global certainty (Star 1985, p. 407).

> evidence when it related to ADRs [Adverse Drug Reactions]. Thus the PDAC altered its boundaries of causal 'proof' over the same drug, according to which boundary emphasized the drug's benefits and which discredited its risks." (Abraham and Sheppard 1999 p. 827, original emphasis)

The above example is particularly worrying because the boundaries of causal proof are not altered, as we might hope, to take account of what is at stake in decision-making. Rather it is skewed in the opposite direction, a reverse of precautionary-style thinking.

Anecdotal evidence is sociologically interesting because it flows across the science-public boundary, but it is also interesting because it might represent a 'subjugated' form of knowledge. Its position at the bottom of any normal hierarchy of evidence makes it an interesting way of looking at power relations in science. Although this is not the main subject of my thesis, a large and informative literature looks at the role of subjugated knowledge in science (e.g. Harraway 1991, ch.9). Donna Harraway makes the point that, although there are insights to be gained from looking at alternative, subjugated knowledges (because they are less likely to fall foul of the myths that come with power), we should be careful not to over-privilege such positions, as some feminist theorists have done (Richards 1996, p. 324). Such positions should also be open to deconstruction (Harraway 1991).

The fluctuating, contextual nature of standards of evidence, whether in explicit rules or as part of the tacit framework used by science to protect its authority, provide us with a fascinating site for considering the layers of a public science debate. We can consider the meanings that 'anecdotal evidence' might have at the boundary between publics and experts. Are these meanings stable enough to place the term as a 'boundary object' (Star and Griesmer 1989), giving actors in different social worlds a common point for understanding, while allowing sufficient flexibility to preserve the identities of those involved? Anecdotes might also provide a 'window on the world': access-points for lay entry into cognitive areas that might previously have been out-of-bounds (Rip 2003a). Through consideration of these themes, I hope to illuminate how the term, its meanings and its deployment in the case of the mobile phones debate help us to understand the relationships that construct public science.
'Anecdotal evidence' is a term that will pepper my thesis. Its relationship with expertise will provide the background to the insights developed through my

research. 'Anecdotal evidence' provides an analytical focus for consideration of the themes of uncertainty, non-expert engagement and the limits of scientific evidence that I have seen defining the public controversy over the health effects of mobile phones.

The Co-production of public science and the public in science

In 1996, a special edition of the journal *Social Studies of Science* sought to contribute answers to a highly pertinent question within science studies: should studies of science politically contribute to the debates they strive to unravel, or should they remain politically neutral?[28]

One contribution, by Sheila Jasanoff, suggested that, rather than worrying about taking sides in controversies, it might be more interesting, when dealing with politicised issues, to see forms of knowledge and social order (and ensuing controversies) as *co-produced* (Jasanoff 1996).

> "An important source of confusion, in my view, is the transposition of the idea of 'controversy' – together with its stripped-down, binary distinctions between 'winners' and 'losers' – from the well-defined context of laboratory studies of science to the complex and shifting terrain of social and political disputes involving science and technology. I argue that a move from the framework of controversy to the more expansive framework of co-production does greater justice to the topography of science and technology in socio-political settings outside, or even inside the laboratory." (ibid., p. 397)

Informed by the teachings of Actor-Network Theory, co-production considers how networks are formed around issues, giving them scientific and socio-political meaning, shaping knowledge and social structures. Scientific research, policy and the public frame and shape one another. We can contrast this to a caricature of how science-in-policy was once considered: as 'science speaking truth to power', causing a (political) policy response, which in turn might have caused a reaction among some members of the public.

---

[28] Social Studies of Science, Vol. 26, No. 2, Special Issue on 'The Politics of SSK: Neutrality, Commitment and beyond', May, 1996

The model of co-production leads us to rethink the dichotomy that separates science from its policy application (Jasanoff and Wynne 1998, p. 6). Distinctions between science and 'science-used' or 'policy-relevant science' presuppose a tidy set of knowledge ready to be employed. This prejudices our analyses of science in public, which is why I have used the phrase 'public science' as a much more general way of describing science with a public context, whether it was intended for policy application or not. Co-production provides a way of integrating the useful lessons of Public Understanding of Science with some of the radical approaches of Sociology of Scientific Knowledge.

This chapter has considered some theoretical perspectives on what goes into constructing science-in-public. I have emphasised that studies of science in itself miss some salient aspects of the construction of public science controversies. It is now an STS staple to argue that controversy in public is not a product of scientific uncertainty, but one of its determinants (e.g. Campbell 1985). So an explanation of public science as a direct extension of explanations of hermetically-sealed private science will tell us little. The co-production idiom allows us to accept a wider set of explanatory factors for the production of science and its public context. To return to the recent paper by Collins and Evans discussed above, in which decision-making is seen within the context of a core-set of experts, we can see that the difference in emphasis is marked. My own reservations about the extension of Collins's core-set model to the public domain have been recently better-expressed by Sheila Jasanoff in a response to the Collins and Evans article.

> "A great deal of the discussion paper's argument turns on C&E's [Collins and Evans'] apparent determination to resurrect Collins' ideas of the 'core-set' in lab-based science controversies and to give it new meaning in the context of public decision-making. That translation unfortunately does not work." (Jasanoff 2003, p. 395)

The core-sets-in-public model presupposes a set of expert decision-makers who are presented with an issue of public concern. However, as we shall see in later chapters, the growth of a public problem does not happen around, or behind the backs of, a core-set. Many problems have no well-defined group of relevant experts when they rear their heads, and experts and non-experts can drastically reshape the nature of the problem itself, disenfranchising any 'core-set' that might have existed. The coevolving patterns of scientific and social order that come from a public science controversy expand the group of actors who play a role in its closure.

The model of co-production is more expansive than previous attempts to explain science as the product of laboratory practice. However, as we gain this explanatory clarity, we perhaps have to sacrifice some of the power that was achieved by early STS work in undermining simplistic science-based assumptions of what makes good policy. The co-production style leaves us with some problems when we consider the use of science-in-policy, or the provision of scientific advice. Here are two to bear in mind while reading this thesis: *Firstly*, if knowledge and social order are co-produced, surely knowledge contributes towards the construction of 'public interest' and 'public concern'. How then should these be 'taken into account' when we make policy? *Secondly*, if scientific uncertainty is co-produced with knowledge as a product of controversy, rather than being its cause (see above), how do actors take the level of uncertainty into account when they make policy decisions? (We will see in chapter five that arguments over precaution illustrate that, just as advisors cannot appeal directly to an unproblematic state of knowledge, so advocates of precaution cannot appeal to an unproblematic level of uncertainty).

Jasanoff has, in attempting to summarise and advocate the development of co-production thinking, claimed that its strength lies in its descriptive richness (Jasanoff 2004a). Jasanoff is less convinced that co-production can offer normative help (but this issue has always been controversial within STS, as shown in the aforementioned issue of *Social Studies of Science*). Co-production can indeed trace the paths of power into previously unconsidered places (ibid.), which can permit symmetrical analysis of policy issues. Just as constructivist studies of science reacted against an unsymmetrical 'sociology of error', so co-production can prevent us from limiting our gaze only to a 'sociology of cock-ups'. Many useful science-policy studies have considered advice that has been shown, post hoc, to be unsympathetic, narrow-minded or wrong. It has been shown that in past cases advisors did not take into account uncertainties (e.g. Millstone and Van Zwanenberg 2001), the expertise of non-scientists (e.g. Wynne 1996b), or the flexibility of what counts as safe (Wynne

1988).[29] But these post hoc *irrationalisations* are illuminated in part by the knowledge that there was a better way things could have been done. In the case of mobile phones, it will not be clear for many years whether scientific advice was as effective, as balanced and as clear as it should have been. But this thesis, informed by the co-production approach, will provide an explanation that contributes to the understanding of scientific advice in which aspects such as 'uncertainty' and 'public concern' cannot be taken for granted. Indeed, public concern might be just as flexible a set of interests as the knowledge that is marshalled as an expert contribution to public science.

Chapter Postscript: Perspectives on mobile phone risk and policy

Mobile Phones are pervasive cultural objects, socially-shaped and contributing to the shaping of our world. They have unsurprisingly attracted a deal of attention from sociologists, although the novelty of their usage suggests that current insights into their history and cultural significance (e.g. Agar 2003, Brown et al 2002, Katz and Aakhus 2002) represent a tiny fraction of the likely attention they will receive. While this literature is sociologically interesting and useful in contextualising my analysis, it is not directly relevant to the controversy over the health effects of mobile phones.

Perceiving Risk

The majority of literature that has emerged around the mobile phones health controversy has come from the field of risk perception and communication. This area has a close relationship with STS, and a similar intellectual history, but from which there are some important points of departure. The field of risk perception emerged as an attempt to conceptualise the divergence between expert and lay risk assessments. The 'psychometric paradigm' (Slovic 1987, 1992) suggested that lay people used psychological heuristics to make sense of the uncertainties that surround them. This approach is helpful to an extent, but from a constructivist STS perspective, it only tells us half of the story. The implicit assumption is that there is a *right* way to view risk, obtainable through expertise, and that lay *perceptions* are

---

[29] Wynne uses the example of the Challenger space shuttle disaster to make the point that, just as scientific controversies provide a site for analysis of context and construction in science, so accidents

distortions. The paradigm therefore slots neatly into the deficit-style thinking that we are trying to move away from.

Constructivist commentators on risk, after Mary Douglas, have suggested that the emphasis should be on how social groups select, accept and construct risks, in line with their cultural beliefs.

> "The perception of risk is a social process. All societies depend upon combinations of confidence and fear… Some fears are physical, some are social… In addressing questions of acceptable risk without considering their social aspects, we could be speaking to the wrong problems" (Douglas and Wildavsky 1992, 6).

This model has contributed a style of thinking from which have emerged many post-deficit ideas about the Public Understanding of Science, namely that a lay understanding does not represent a deficiency, but an active position taken according to deeply embedded cultural norms (see Douglas 1992). But this model is only weakly constructivist (Lupton 1999, p. 28), again implying that there is a correct way of thinking about risk, which is then distorted by social factors. The advantage of STS approaches to risk is that no assumption is taken about correctness. This has allowed commentators to observe the social assumptions that feed into objective scientific risk assessment, and the ones that are left out (e.g. Wynne 1989), and the impact these assumptions can have on the coherence of risk assessments (Stirling 1999).

Mobile Phone risks

The majority of the literature that concerns itself with the construction of mobile phone risk emerges with a positivist spin often from the scientists who have been involved in research. Articles have appeared throughout the debate in popular and scientific and popular scientific journals such as *The Lancet, New Scientist* and the *British Medical Journal*, as well as risk theory journals. Much of the language of risk perception, communication and policy has been readily accepted in scientific circles. Quirino Balzano and Asher Sheppard (two scientists who have contributed research to the controversy) argue in a recent paper that there is no evidence of a risk from mobile phones, so the application of the precautionary principle to mobile phones is

---

provide a site for the research of similar issues with technology (Wynne 1988).

unjustified (Balzano and Sheppard 2002). Their impression is that policy is following fear rather than quantified risk assessment, and so operating sub-optimally. Adherence to the precautionary principle, they argue, produces weak, or erroneous policy (ibid., p. 361). Three other influential scientists criticised the application of the precautionary principle to policy on mobile phones for similar reasons. They noted that the principle is poorly defined and designed for environmental rather than technological problems and that its application undermines the vast body of existing scientific knowledge (Foster et al 2000). In the article, they cite the recommendations of the communication from the EC, discussed above, as a possible road to better policy. Adam Burgess provides a more constructivist account which leads to the conclusion that precautionary approaches to the mobile phone issue, especially in the UK, have contributed to the perception of risk by the public[30] (Burgess 2002). (See chapter five for further discussion of the constructions behind these precautionary and anti-precautionary arguments).

Literature along these lines, and there is plenty more, helps explain some of the public concern about mobile phone risks, but it suffers from requiring assumptions about their safety from the outset. Two Polish scientists who are more wary of the health effects of mobile phones conclude in a risk article that the public *underestimate* risks from handsets, while *overestimating* risks from base stations (Szmigielski and Sobiczewska 2000). This article is mainly a scientific review, portraying risk perception lessons as logical offshoots from its scientific conclusions.

Taking a position on the reality or otherwise of a risk can lead to erroneous or unhelpful conclusions. STS can provide us with a privileged perspective by suspending judgement about the reality of risks, problematising expert risk assessments and appreciating that any level of uncertainty is contested (see above). Suspending judgement allows us to consider the risk *issue* rather than the risk. William Leiss, presenting the mobile phone controversy as a case study, recommends a move from 'risk management' to 'risk *issue* management' (Leiss 2001,

---

[30] Burgess uses the phrase 'construction of risk' (Burgess 2002, p. 175), but this seems inappropriate without a problematisation throughout the risk spectrum, rather than just at the 'social' end. Scientific risk assessments are not seen as problematic. The use of the term 'construction' in this way, disguising a realist account of risk, has been a feature of much risk perception literature.

p.10).[31] This sheds light on risk (as characterised by scientific assessment) as one contested perspective in a wider public context. The risk need not therefore define the controversy. Such a perspective help us to explain how issues emerge and fade, how they should be understood and the role of policy.

STS and EMFs

Mobile phones, as mentioned in the introduction are one of a number of technologies that induce electromagnetic fields (EMFs). Other recent EMF technologies that have caused similar controversies include microwave ovens, power lines and computer VDUs (visual display units). The controversies cannot be seen in isolation from one another, just as the mobile phones debate cannot be seen as separated from other recent debates. There are features of other EMF technological controversies that apply to the mobile phones example without much adjustment. It would be churlish therefore to ignore the insights that STS has provided in these areas. The following section also represents a useful means of introducing the perspectives described above to the case study considered in this thesis.

As I mentioned above, few scholars have dealt with issues around Electromagnetic Fields (EMFs) from an STS perspective. David Mercer has pointed out that the issue has received little theoretical attention, despite a large quantity of more popular literature addressing some of its social and political themes (Mercer 2002, p. 206). As well as active engagement with risk and policy debates, in which an STS perspective is brought to bear on a number of myths which pervade the debate (Mercer 2001), Mercer has addressed the discourse used in credibility debates in public EMF science (Mercer 2002). He uses the EMF debate to explain how, in public science issues, all aspects of science are contested (ibid. p.223). Flexible interpretations of bias, trust, credibility, methodology and epistemology can all contribute to the widening of an issue, maintaining its pertinence in the face of attempted regulatory closure (ibid.). This paper highlights some of the most fascinating elements of the EMF controversy, all of which appear in the mobile phones example.

---

[31] Funtowicz and Ravetz make a similar point. In issues involving post-normal science, in which decisions are urgent, science is uncertain and the stakes are high, research is *issue*-driven (Funtowicz and Ravetz 1992) , rather than driven by curiosity or by client demands.

From a more anthropological perspective, a paper by Lisa Mitchell and Alberto Cambrosio has addressed the issue of how electromagnetic 'pollution' is constructed as a hybrid[32] entity (Mitchell and Cambrosio 1997). The paper looks at the use of measurement devices for EMFs, often used to 'define' the level of EMF risk. In measuring, people are opening a black-box of previously unobtainable knowledge about field that surround us, which can have important impacts on identities (ibid.). These insights, though not explicitly addressing issues of expertise and public science (ibid., p. 223), serve to contextualise the mobile phones debate and highlight some of its most salient features. The fields produced by mobile phone technology, which many people consider to be risky, are invisible but measurable (which raises the question of who has access to knowledge of field strengths); they are natural and simultaneously the product of technology; and they are pervasive. As the limited STS scholarship of EMFs has helped to show, along with the other insights into public science offered in this chapter, perspectives on such technologies and the expertise behind assessing their risk are about more than just 'science'.

[32] The term 'hybrid' is borrowed from Bruno Latour to describe things that emerge in many places, with many meanings, as a product of networks of scientific, political and public actors. Such things are both 'natural' and 'social' (Latour 1993).

3 Methods and Methodology

a methodological anecdote

> Halfway through my research, I had a meeting with an epidemiologist whose particular expertise lay in research design. At the end of the interview, I turned the tape off and our conversation turned to the mechanics of social research. The scientist asked who funded my research, (with an implied 'why?'). He asked, "So what do you do with this?", pointing to the tape recorder. I replied that I would go back to the office, transcribe it, anonymise it, analyse it and try to draw out some themes that would illustrate the workings of a scientific and public controversy. I continued that I would produce a thesis that reflected my conclusions. He asked, "So how does anybody validate it?" After a pause, I answered that no-one really could. My research and its results reflected my interpretation. I could offer my accumulated data to others, but a separate analysis would look at different things, reveal a separate perspective and might lead to different conclusions (although not too different, I hoped).
>
> He then asked who else I would be speaking to. I said that, first, I was planning to speak to about half of the members of the Stewart group (the IEGMP, see chapter five). He jumped in, "Why only half? They might be the wrong half."

This thesis reports on a case study using a combination of research methods. The majority of my data comes from a series of 31 interviews conducted over 18 months. In addition, I analysed policy, scientific and activist literature from the period of interest (c. 1998 – 2003), as well as attending meetings and conferences on the health effects of mobile phones. This additional research contributed a contextual element that would not have been revealed by interviewing alone. My research has provided a picture of the case, but the methodology used to build up such a picture is not straightforward, as the anecdote above shows.

Research Questions

The mobile phones health controversy as a case study does not in itself suggest obvious themes for consideration. It does not beg research questions. Rather it is an interesting case of contemporary public science that yields themes through study.

My research questions therefore developed with the research project, although the topics that emerged in preliminary research remained consistently fruitful. My research began with a desire to probe key themes identified on reading the Stewart Report, the most important scientific advice review of the mobile phones controversy in the UK (see chapter five). These themes (Evidence, Uncertainty, Expertise and the Public) have fuelled my theoretical investigation, and they have directly fuelled my methodological progress. Initially, the direction of my research could be summarised under four main questions:

1. How is uncertain science used in technology policy in the case of mobile phones?

2. How is scientific uncertainty about mobile phone risks communicated in scientific advice?

3. How are lay risk assessments used in the construction of policy for dealing with potential mobile phone health risks?

4. How is anecdotal evidence assessed and handled in public science?

My research questions began with a need to gain a broad perspective on the construction of the mobile phones controversy as a public phenomenon. I began my research early and speculatively, rather than formulating a strict research design. This allowed me to grasp the context of the debate relatively quickly, and therefore shape my research and discover which themes would be the most productive. Throughout, however, there was a consistent emphasis on the expert construction of non-scientific phenomena. My research is therefore a constructivist case study, with an emphasis on the role played by experts.

My research targets the construction of phenomena by expertise. Its questions are best answered by qualitative analysis, but impossible to answer by any form of traditional ethnography. There did, and does, not exist a relevant community of scientists or advisors which can be culturally observed, a point I reiterate in the concluding chapter. My research considers an issue whose expert construction emerges from meetings, occasional conferences, scientific literature and policy documents. The only cultural feature the cast of my research all share is involvement in 'The case of the mobile phones health controversy in the UK'.

Studying cases

Yin defines a case study as "An empirical inquiry that investigates a contemporary phenomenon within its real-life context, especially when the boundaries between phenomenon and context are not clearly evident" (Yin 1994, p. 13). As with any 'issue', 'debate' or 'controversy', the unit of study is defined along with its emergence. The 'case' of the mobile phones controversy is not easily separated from the related EMF controversies mentioned in chapter one, and the scientific debate certainly cannot be considered outside its public policy context, as will be argued.

A case study brings methodological baggage which must be considered in carrying out research and extrapolating findings to other situations. Some features of case studies, drawn out by Jennifer Platt, illustrate their advantages and disadvantages.

<u>What can case studies do? (after Platt 1980)</u>

1. Case studies can suggest the plausibility of a social situation when generalised beyond the case in question. At their weakest, therefore, they can provide hypotheses of interest.

2. Case studies, by not aiming for generalizability, can illustrate the importance of a particular context to the construction of a feature. As an issue moves through a social world, it illustrates features of this world (what Platt calls a 'social barium meal' (page 10). The theoretical insights described in chapter two emphasise that the analysis of "Knowledges in Context" (Wynne 1991) is central to constructivist studies of science.

3. Case studies can clearly be useful to further interpretation of the same case. (I would hope that another analysis of the case of mobile phones and health, whether of a particular aspect, from a different perspective, or at a later stage, would consider my work as influential).

4. Case studies can (sometimes) claim to generalise their descriptions (and sometimes prescriptions) beyond their frame of reference. It might be that, in a particular case, the prevailing wisdom is shown to be false.

5. Similarly, case studies, because they are open to surprising developments, can produce novel and interesting conclusions that might not have emerged from a stricter *ex ante* research design.

In a vigorous defence of the power of case studies in social research, Bent Flyvbjerg notes a scientistic criticism of case studies that their lack of scientific method will lead to results that are likely to fit pre-existing interpretations (Flyvbjerg 2001, p.81). However, Flyvbjerg rejects this 'bias towards verification', noting that there are myriad examples of case study research which, because of its flexibility, has been able to cast off theoretical constraints and advance understanding (ibid. p.84). This criticism, and others, of single-case research resonates with many of the insights into some of the negative constructions of anecdotal evidence which are presented in chapter six.

So, given the singularity of my study, what ambitions can it have for generalizability? Can it aim to say anything beyond its own frame of reference? And how can readers be sure that my analysis is robust? The scientist from this chapter's opening anecdote appreciates how problematic these questions are, just as he appreciates my inability to answer them in a way that would traditionally be considered scientifically robust. After all, as qualitative social researchers, "what we call our data are really our own constructions of other people's constructions of what they and their compatriots [or colleagues] are up to" (Geertz 1973, p.9). Geertz is discussing the study of cultures defined in part by their geography, but we could apply this insight equally to the construction of a study of a group of experts (even when it is not immediately clear who that group is, or whether there exists one core group to be studied).

My research is qualitative. The merits and problems of qualitative research have been addressed in depth in sociological literature. The following is as good a summary as I have come across of the differences between quantitative and qualitative approaches:

> "The quantitative goal is to isolate and define categories as precisely as possible before the study is undertaken, and then to determine, again with great precision, the relationship between them. The qualitative goal, on the other hand, is often to isolate and define categories during the process of research... For one field, well-defined categories are the means of research, for another they are the object of research." (MacCracken 1988, p. 16)

Qualitative research is necessarily flexible, and the questions (and perhaps methods) of research must respond to the insights developed during the project. Qualitative research affects how research subjects are accumulated and handled during interviews:

> "The purpose of the qualitative interview is not to discover how many, and what kinds of people, share a certain characteristic... It is the categories and assumptions, not those who hold them, that matter. In other words, qualitative research does not survey the terrain, it mines it." (MacCracken 1988, p. 17)

Yin (1994) makes the point that case studies necessarily draw upon multiple sources of evidence. The use of a single method would produce a skewed analysis of an issue that emerges within a particular context. My methodology is therefore mixed, although the bulk of my evidence comes from a series of interviews.

Gathering data

The interviews I conducted were semi-structured, based around a set of themes and key questions but flexible enough to follow the natural conversation and thought-processes of the interviewee (see Appendix 2 for a typical set of themes and questions). Semi-structured interviewing attempts to reconcile the advantages of a structured interview (comparability, control, coding) with the broader perspective gained in an open-ended interview. Open-ended interviews provide the opportunity to let the interviewee determine the discussion and so reveal their own perspective. "The rationale behind open-ended interviewing is that the only person who understands the social reality in which they live is the person themself" (Burns 2000, p. 425). How this social reality is represented in conversation is problematic. There needs to be an appreciation that, especially with intelligent, reflexive interviewees, there will be an unavoidable difference between accounts and behaviour, a topic addressed below.

My interviews were originally based upon a long set of questions which emerged from my main research questions. These questions represented some initial points of exploration for the case study. However, once I became more familiar with the salient issues of the case study, it became clear that interviews would run more clearly with a list of topics to be covered. This not only allowed for the possibility of the interviewee defining their perspective on issues such as evidence and uncertainty, but it also proved a more effective way of taking into account each interviewee's perspective as individual actors rather than as representatives of a group. Each interview was tailored towards the interviewee. I researched the background and role of each interviewee before we met. Later interviews were conducted with topic sheets that included contextual information about the specific interviewee, what work they had done and whether they had publicly stated their position on an issue. It was important to consider the *individual* impact interviewees had on the controversy, rather than as typical representatives of a group.

Elite interviewing

At least half of my interviews might be considered 'elite interviews'. That is to say that the interviewees were "people used to exercising power and influence" (Arksey and Knight 1999, p. 122). This is unavoidable when investigating the social processes that go into expert decision-making, particularly if the credibility of scientific advice depends on the esteem of individual scientists (Jasanoff 1997). Elite interviewing raises a set of special methodological issues, which are considered below.

Access

Access to these subjects can be difficult. They are usually busy and difficult to contact except through assistants etc. (although this can sometimes greatly simplify arranging a meeting). On the whole, however, once contact was established, the subjects readily agreed to speak, often with the most senior being the most welcoming.

In elite interviewing, the *individual* is the target of research rather than the group they might represent, so providing a complete picture can hinge on their co-operation. Thankfully, all but two of the key subjects identified agreed to speak to me, although in some cases interviewees were pressed for time. Time constraints forced me to cover only the issues raised by my research questions, and prevented me from exploring other nuances of the discussion. However, the interviews that were truncated tended to be later in my research, by which time the important themes had emerged.

Anonymisation

As mentioned above, my intention from the outset was to keep all interview data non-attributable (i.e. anonymised). I am convinced by my experiences that this was the right thing to do.[1] In almost all cases, an assurance of anonymity helped to build rapport and encouraged an openness of discussion. Many of the interviewees required reassurance that knowledge of 'who said what' would go no further than me. Some wished for information or opinions to remain off the record, demonstrating that even more prosaic debates can have controversial implications if opinions are associated with individuals.

I had one major reservation about anonymising my interview data. The controversy, as will become clear through my narrative, was largely constructed and steered (at the expert end) by the actions and charisma of a few individual actors. To therefore refer to them as a 'scientist' or a 'committee member' removes some of the narrative purpose of recounting what was said. However, I was fairly sure that, given the public nature of this controversy, such individuals had made their feelings known in public, quotable fora. Where possible, if an opinion has been expressed in the public domain similar to one that emerged from an interview, I have used an attributable version.

In anonymising interviewees, I wish to retain their position in the debate to contextualise their data. Where a quote is used, therefore, I indicate who said it according to their most relevant position. Many of my interviewees were members of multiple committees, or committee members and research scientists. I therefore choose which role is most illustrative in the interpretation of their comments. Opinions differed in some cases as to whether another interviewee was a 'scientist' or an 'activist'. The distinction, as with the title of 'expert', is by no means fixed. I have therefore tried to select the most relevant and most consensual job title.

I interviewed the three chairs of relevant committees (AGNIR (see chapter four), IEGMP (chapter five) and MTHR (chapters five and six)). Not wishing to reveal their status, I have however demoted them to 'committee members'. Similarly, I have not indicated which member of the IEGMP was a lay member. Only two of

[1] There are ethical as well as expedient reasons for anonymising interviewees. The British Sociological Association's statement of ethical practice demands that privacy and anonymity be respected even if the material is not especially sensitive (BSA 2002).

my interviewees were women. To retain their anonymity, I have not used the word 'she' in referring to either of their comments. The referenced interviews are numbered to protect identities, with the numbers assigned randomly.

I conducted 31 interviews over 18 months (see appendix 1). These ranged in length from 25 to 100 minutes, with the average length being around 50 minutes. Initial interviews tended to be longer, as I was interested in letting the interviewees speak for themselves and so define their own key themes. My initial interviewees were university scientists who had conducted some of the widely-reported research that played a large part in the construction of the controversy (see chapter four). I then spoke to members of the Stewart Committee (see chapter five) who, in 1999-2000, had conducted one of the most important scientific and advisory reviews of the subject. In addition, I interviewed some members of the MTHR committee (see chapter six) who did not take part in the Stewart report (the two committees shared much of their membership). I spoke to activists, scientists, industry representatives and the civil servants who oversaw the production of the Stewart report. One of the interviews was a joint interview with two civil servants. Of my 31 interviews, 22 were with people who would consider themselves scientific experts in an area directly connected with the mobile phones case. The remaining 9 (including civil servants, industry and a lay member of the Stewart committee) were with people who possess enough expertise to contribute eloquently to the construction of scientific and policy debate. As might be expected in a scientific controversy, some interviewees, though considering themselves experts, would not be considered experts by others. My final interview was with a scientist I had interviewed near the start of my project. I was keen to follow up on some of the themes he had introduced and to get his perspective on the developments within the controversy in the 18 months since we had previously talked.

The large majority of my interviews were recorded onto tape and transcribed, although the last two were recorded directly onto MP3 files. Two interviewees refused to be recorded[2], so I took notes on important details, opinions and short quotes where relevant.

---

[2] One interviewee refused to be recorded for practical reasons of electromagnetic compatibility (the electromagnetic fields generated by the tape recorder might interfere with experiments being conducted).

At the start of each interview, I provided only the briefest explanation of my research project, usually saying that I was interested in the mobile phones health issue, with an emphasis on the way scientific advice is provided in a public context. I was concerned that, with an intelligent and sometimes highly reflexive group of interviewees, their responses might be shaped by a desire to please or a view of me as an implicit advocate of their position. My interviewees were, on the whole, eager to help and interested in the scope of my research project, but I tried to keep the conversation running on their terms as much as possible.

It became clear during the first few interviews which topics prompted fruitful discussion. Interviewees were keen to talk about anecdotal evidence and the part it played in science and policy. Questions about scientific uncertainty and its problematic nature over time tended to cause more confusion.[3] I found it difficult to engage scientists with the reflexive question of "how uncertain is the science of EMF health effects?" However, often opinions on uncertainty fell out of answers to other questions during the interview. Few scientists only wanted to discuss 'the facts' or restrict discussions only to their narrow area of expertise. Most appreciated the contingencies and the public context of their work and were willing to discuss these elements openly. Laboratory scientists were keen to explain their work and activists were keen to expound their views on general and specific aspects of the debate. Advisors, often with lengthy policy experience, tended to provide considered and balanced opinions, viewing me as an 'audience' rather than a researcher.

A clear line could be drawn between those interviewees who were involved in the debate and those who still are involved in the debate. Some of my interviewees worked on the Stewart committee, but have since had little to do with the issue. Even those who worked on the Stewart committee and have since worked with the subsequent MTHR programme had difficulty remembering exactly what happened two years previously (although they might have considered this a politically expedient amnesia – at the time of writing, the Mobile Telephones Health Research Programme (see chapters five and six) was still allocating research funds, based on the priorities identified by the Stewart report).

[3] It is easy to forget the confusion that some questions caused during interviews. A blank stare can sound like a considered pause on tape, and look great on paper.

At the end of each interview, I asked the interviewee who I should speak to broaden my perspective or investigate a certain theme ('snowball sampling'). The names I was given determined to a large extent who I would speak to subsequently. Names were replaced on my list of prospective interviewees until I was confident that I was receiving a balanced picture of the issue. Interviewees tended not to discriminate between scientists, advisory scientists, activists and others in recommending targets for interview. And most encouraged me to speak to people who had views wildly different from their own. Addressing the concerns raised in my opening anecdote, qualitative research cannot make claims based on its research sample to generalizability. The sample was therefore constructed to provide a balanced picture, centred on the UK case but informed by international perspectives. To answer the scientist's final question in my opening anecdote, I interviewed those members of the Stewart committee who had been recommended to me by other interviewees. Rather than aiming to present the typical views of the committee, therefore, my project reports on the opinions of those who were seen as influential.

My interviews were occasionally bookended by activities which were equally interesting. Some interviews were followed by discussions over lunch, some by tours of laboratories and one by a walk through London towards a public meeting of the MTHR committee. Such situations provided research material which would not have arisen from the more sterile environment of an interview held at my request. Informal discussions highlighted the inadequacy of interview data alone in acquiring knowledge of a case study.

Other sources of evidence

Rather than interviewing exhaustively, which I felt would provide a partial perspective on the construction of public science, I supplemented my interviews with documentary research of scientific papers, policy literature, Internet publications and public record archives of the workings of the IEGMP (released January 2003).[4] The public records from the IEGMP emerged surprisingly soon, a benefit of a governmental desire for greater openness. However, this openness brought some unforeseen hurdles. There is a tendency, in situations where authority

[4] Documents at the Public Record Office are usually closed for 30 years before release.

is called upon to be publicly transparent, to 'backstage'[5] the most interesting parts. In sifting the public records, I came across a letter from the deputy director of the NRPB (featured in chapter four) to the IEGMP's chairman, Sir William Stewart (the frontman of chapter five):

> "I also felt that we should not attribute too much to individuals other than where it was necessary. I can foresee that in due course these minutes might go on the Public Record and it would be better not to have specific views attributed to individuals on the group."[6]

In addition, many of the meetings of the IEGMP are poorly-represented by their public minutes. Of the IEGMP's five public meetings, a transcript survives only for the meeting in Liverpool. Reading this transcript alongside the sanitised minutes from the IEGMP web site crystallises the attempt to 'backstage' the more problematic elements of scientific advice, particularly when non-scientific groups are involved. I will return to this theme in the conclusion of this thesis, where I discuss expert attempts at constructing 'public concern'.

While it is not my intention to attribute views to individuals (see 'Anonymisation' above), this consciousness of the future public status of documents often prevents such views from being revealed at all, attributed or not. These public records must be seen as expert constructions, rather than as a straightforward representation of the workings of a committee.[7] Researching the records of the IEGMP revealed plenty, but it also revealed how much was unresearchable, either because it had not been submitted as part of the records, or because key discussions had taken place informally.[8]

---

[5] See Hilgartner (2000) for an exposition of scientific advice as a public performance, in which aspects are emphasised or kept hidden to make the drama more effective. Hilgartner notes the tendency to hide aspects of ostensibly 'transparent' scientific advice that might prove embarrassing (ibid., p. 149).

[6] Letter from John Stather to Sir William Stewart, 29th July 1999 (PRO HP4, file 25

[7] It is perhaps a feature of research in a reflexive modern environment that it is often very easy to get hold of material, but that this material might be a very partial resource. While it is therefore easy to get hold of information, research requires a new level of analysis to assess to what extent the picture is complete.

[8] Jeremy Paxman's account of British political life reveals the extent to which Hansard, the official record of proceedings of the UK Parliament, is tidied up before publication. According to Paxman,

Publications such as Microwave News (MWN) provided a valuable introduction to the breadth of the controversy. MWN is itself an important and well-respected publication, reporting scientific and policy developments in the health effects of EMFs. Its tone is unashamedly sceptical of assurances of the safety of EMF technologies, but it is well-researched, and regularly reports the opinions of international scientists.

Such publications, along with policy, regulatory and industry documents, contribute to a discursive variety which can be analysed to get an idea of the competing interests and definitions in the controversy. I analysed texts as rhetorical devices (see Woolgar, (1989) for a short explanation), contributions to discourse rather than explanations in themselves (for example, the 'discourse of compliance' explained in chapter four). Some of these texts were purely scientific, but the more valuable texts were from advisory scientists, regulators or industry, operating (without clear distinction) between repertoires of 'science' and 'politics' (cf. Gilbert and Mulkay 1984).

In addition to my interviews and documentary research, I attended conferences and meetings[9] that dealt directly with the question of mobile phone risk, including three public meetings of the Mobile Telephones Health Research committee (See chapters 5 and 6). These meetings allowed me to look at scientists in a professional environment (although it quickly became clear that there was no context in which advisory and research scientists could be observed 'doing public science'), as well as seeing how experts interacted with activists and members of the public.

It was important to keep a perspective on the accounts given by my interviewees and to consider how their behaviour might differ from this. To a large extent, I presumed that the validity of my research emerged from the rapport built up during conversations. However, because some interviewees were less forthcoming or busier

---

Hansard is "not a verbatim record of what was said in the chamber of the House of Commons, but what the participants wanted to say" (Paxman 2002, p. 172).

[9] "Mobile phones – Is there a health risk?" conference, 20th-21st September 2001; MTHR meeting, 8th November 2001; MTHR meeting, 11th November 2002; WHO meeting on the Application of the Precautionary Principle to EMF, Luxembourg, 24th February 2003; "RF Interactions with Humans" conference, 27th-28th February 2003, MTHR meeting, 4th November 2002

than others, there was a need to complete the picture with other forms of observation.

Internet research was a valuable tool for information-gathering[10] and for providing context. I could easily discover who an interviewee was, what they had done and why it would be enlightening to talk to them. Most policy documents and many scientific papers were accessible online, making them easy to find and search through. I also subscribed to a number of mailing lists that were relevant to the issue.[11] These were run by scientists and activists, and their postings provided an important context to my analysis, forcing me to consider my conclusions in relation to the daily developments of the debate.

As William Leiss and Greg Paoli have discussed in their account of the mobile phones health issue in Canada (Leiss and Paoli 2001), the Internet can reveal a richness of what unorthodox scientists and activists know about a controversy. For the social researcher, the Internet can provide access to a 'virtual community' (Hine 2000) of actors drawing connections between issues, empowering local communities and dispersing expertise as an alternative to the received wisdom. Internet research can reveal the breadth of expertise that is made relevant to a public science controversy. Although it is not the explicit subject of this thesis, the Internet has contributed to the construction of interested groups around the mobile phones controversy, particularly campaigners against mobile phones and their masts. My research of the web sites that are most readily accessed by concerned and interested non-experts has provided valuable context for my representation of the expert side of the controversy.

This thesis accounts for a fraction of my research. The unrepresented remainder, much of which was not analysed in detail, served to keep my analysis in context, reminding me not to over-extrapolate interesting conclusions reached as a result of analysing the interviews or policy documents. I am confident that, in my

---

[10] Although, of course, it is important to be wary of the trustworthiness of much of the information online.

[11] The mailing lists to which I subscribed were the Bioelectromagnetics mailing list, the Society for Radiological Protection (SRP) mailing list, 'EMF-L' (managed by Roy Beavers), and 'EMFacts' (managed by Don Maisch).

combination of methods, I have approached a description of a case that is not 'observable' in any easy sense.

I converted the material from interviews and archive research (minutes, publications, media transcripts etc.) into a project which could be analysed as one unit within QSR NVivo. NVivo is a qualitative data analysis (QDA) package that provides easy sorting, coding and searching of data. It helps clarify analysis of a large amount of documents, which is vital in the mixed-methods handling of a complex case study involving many actors and perspectives. But it is also useful in building analytical rigour, forcing new lines of inquiry while being flexible enough to accommodate changes in data and approach.

I coded each transcript, document or set of notes in NVivo according to the themes derived from my research questions and the themes presented by each source. The list of 'nodes' (coding categories) expanded during the initial stages of data analysis, totalling 54 by completion (the complete node list is reproduced in Appendix 3). The coding scheme allowed for search, retrieval and analysis of relevant material, as well as analysis of relationships between nodes – considering whether, for example, 'anecdotal evidence' was talked about in the context of 'scientific uncertainty' and/or 'public concern'. In addition, sources were labelled with attributes indicating their position in the mobile phones health controversy. This allowed for comparison between, for example, written and spoken opinions, or opinions from different types of scientist. As I have mentioned, the collection, coding and analysis of project data was an iterative process of refinement. As such, it fits well with qualitative methodological developments originating in grounded theory, in particular the constant comparative method that can be used to analyse categories defined both by researcher and by subject (Glaser and Strauss 1967).

In this thesis, much of the evidence for my narrative comes from publicly-available sources – minutes, web sites, advisory reports etc. This thesis is an analysis of public science, so many of the themes I elucidate can sufficiently draw upon public discourse. However, my interview data provides another perspective on the issues, revealing both the thinking behind much of the public discourse and some themes which have not been openly discussed.

Conclusion

This thesis represents my reconstruction of a case that might not have existed in any real sense. As with any case study, it is bounded essentially by my desire for a controlled research project. Towards the end of my research, I began to think that the only thing all of my research targets (people, documents, groups and places) might have in common is their contact with me. However, my doubt is largely dispelled by my interviewees' eager engagement with the topic. Few seemed to think it was not worth studying or that 'it' – the public science controversy over mobile phones – did not exist.

This thesis fits within the broad tradition of Science and Technology Studies (STS), as outlined in the previous chapter. This theoretical alignment contributed some methodological insights which are useful in trying to understand the place of my research and its limitations in speaking to the outside world. STS has grown up with some distinct interpretative and methodological maxims. The symmetry postulate in particular (see chapter two) serves to remind the social researcher that they should remain agnostic about the truth or falsity of science, or the reality of the risk they are investigating. My research led me to form opinions of the riskiness of mobile phones, based on which of my interviewees I trusted. But these opinions (should) have no bearing on the presentation of my research in this thesis. This agnosticism raises the question of how real the 'issue' is if we cannot say anything about the reality of the risk that is its centrepiece. Again, the only consolation I can offer is that the 'issue' (my case study) was considered important and distinct by my various interviewees and correspondents.

In researching and reconstructing my case study as this thesis, I rely upon expert constructions. I made an early methodological decision not to produce a complete story by trying to get an accurate picture of the state of public opinion on the mobile phones health debate. To do so might have been to fall in line with naïve conceptions of the public understanding of science (as in the previous chapter) in which the gap in understanding between experts and everyone else is accentuated. This thesis is *about* the public, the non-experts within this debate, but its insights are based largely upon expert constructions of public concern and public engagement. To aim to represent public opinion on the health risks of mobile phones would be to ignore the mechanisms by which a small section of the public become engaged in such a debate.

Examining the controversy over mobile phones while it is ongoing has allowed me to observe the (re-)construction of science that might have been disguised by history, but it has also allowed me to observe the emergence of scientific and social order (or disorder). In looking back at a controversy, I suspect that the temptation is to prise politics apart from science in an attempt to see better what is going on, but this would obscure the intermingling of the many aspects that go into producing an 'issue'.

This thesis is a reflexive pursuit, so the questions that fuel my case study research are similar to the questions with which one might doubt the validity of my research. This thesis poses bigger questions, only a few of which it can hope to answer, such as 'what counts as evidence?'; 'How does research become authoritative?' 'Should research be scientifically robust, publicly credible, or just useful?' These questions and others hang over the representation of my own work and maintain my case study as an accurate and interesting analysis of a controversy and a context. My account, while unashamedly constructivist in its approach, is designed to be constructive. It is my hope that, while its findings should not be extrapolated way beyond its scope, it will contribute to decision-making in similar areas, in similar circumstances.

As a case study, this thesis could be considered anecdotal. Based on my analysis of anecdotal evidence that provides the centrepiece of my research (see chapter six), I leave it to readers to make up their minds as to the advantages and disadvantages of my research and its representation. If it is anecdotal, I hope that, as with the most optimistic constructions of other anecdotal evidence we will see later, it questions previous assumptions and provides a necessary perspective that would not have otherwise emerged.

4 Controversy, Science and a Reliance on Compliance

In the course of my research, a story emerged from the development of the mobile phones controversy and my interviewees' opinions. This story was guided by the intended direction of my research, but revealed unforeseen elements during its construction. This section of the thesis narrates the short history of scientific advice on mobile phone health effects with an emphasis on the features of scientific advice and public concern that have defined the building and undermining of public credibility. I aim to show how an 'issue' develops as the product of negotiations, between scientific research, lay involvement, rhetoric and policy intervention.[1] My narrative is informed by the theoretical ideas developed from the constructivist studies of science discussed in chapter two, so will emphasise the vagaries and contingencies of knowledge. However, as was discussed in chapter two, a case study in public science is partial if it considers the science away from its broad public context. Whatever controversy exists at any time about mobile phones is both scientific and public, with the interests of different groups waxing and waning for a number of reasons, which might be more closely connected than is conventionally understood.

A major aim of this chapter and the next is to illustrate how, in addition to knowledge claims, claims of *what we don't know* contribute to constructing the content and context of a controversy. In line with the approach outlined in chapter two, uncertainty and ignorance claims are therefore treated as a flexible resource and a vital part of authoritative knowledge claims.

This chapter aims to describe how mobile phone scientific advice stood when the public, the British media and I found it in the late 1990s and how challenges to the dominant science/policy consensus undermined the authority of this advice. I will begin to unpick the politics of the state of scientific consensus and scientific advice

[1] This study does not aim to provide a thorough description of the claims-making that contributed to the emergence of a 'social problem'. This job, using ideas drawn from the literature on the sociology of social problems (see Schneider (1985) for a review) has been done with respect to the mobile phones controversy elsewhere (Burgess 2004). My account offers an explanation for the role of experts in this debate, although inexpert claims are considered if reflected in the actions or rhetoric of experts.

as it faced public challenge. Particular emphasis is placed on the concurrent roles played by science, technology, politics and the public. The following chapter considers how this controversy, once established, was understood and managed by experts.

This chapter begins by looking at the source of the health controversy over mobile phones – the radiation and Electromagnetic Fields (EMFs) that are emitted from the antennae of handsets and base stations. I then describe 'what science knows' about the effects of such radiation. I introduce the National Radiological Protection Board and describe the science and politics of setting guidelines for protection from EMFs. Behind these guidelines lies a thermal consensus, and in front lies a discourse that, with the onset of controversy, is used to defend both the scientific consensus and the authority of the guidelines.

This chapter acts as a constructivist critique of scientific advice whose public credibility plummeted with the onset of controversy, and might therefore be seen as laying the blame at the door of the institution responsible, the National Radiological Protection Board (the NRPB, who will be formally introduced shortly). However, I hope it contributes more as a diagnosis, explaining the circumstances that contributed to a lay disenchantment with scientific advice. While the NRPB's handling of the public controversy was not immaculate, its ills were largely caused by circumstance rather than self-inflicted.

"If it doesn't heat you, then it doesn't harm you"

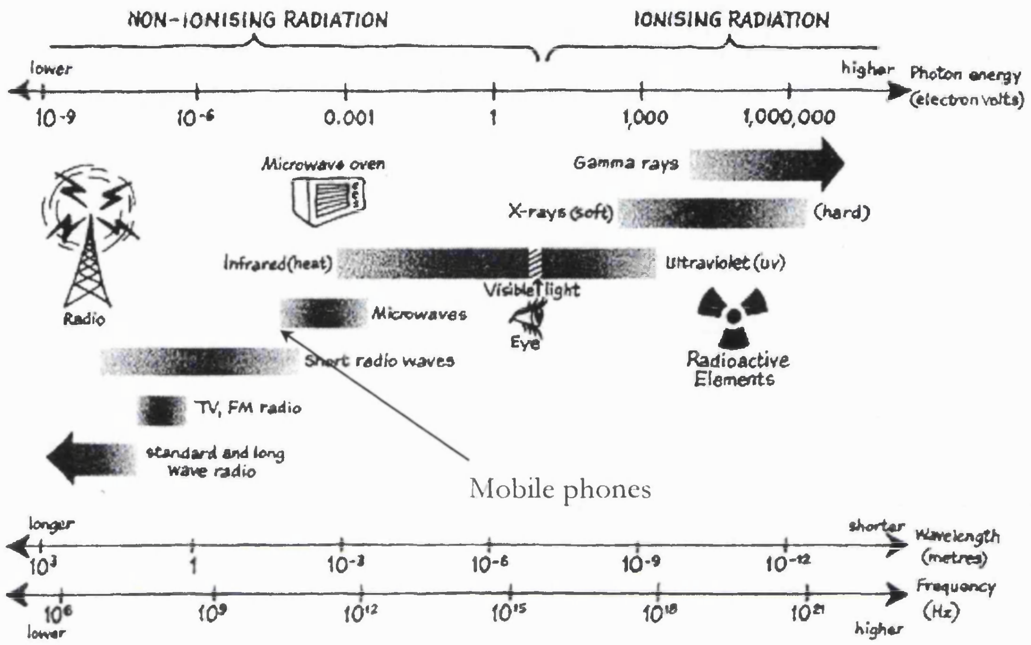
The phrase that provides the title of this section is a quote from John Stather,[2] the assistant director of the NRPB. It sums up the consensus over the known harmful effects of electromagnetic radiation such as that emitted by mobile phones (from heating body tissue) and an assurance that any uncertainties that remain in the science are not problematic. I will explain in this section how such reassurances come about as a public manifestation of a scientific, regulatory and industry consensus. But first, we must consider the origins of science's knowledge about mobile phone risks.

---

[2] The Money Programme, BBC2, 18 April 1999

Mobile phones and their base stations are generators of electromagnetic fields (EMFs) and emitters of electromagnetic radiation. This radiation is used to carry our phone calls, text messages and, increasingly, a range of other information.

Figure 1: The Electromagnetic Spectrum



Picture reproduced with permission from the National Physics Laboratory

In the UK, the two prevalent digital systems operate at frequencies in bands around 900 and 1800 megahertz (MHz).[3] These frequencies lie between analogue TV transmissions (at 470-854 MHz) and microwave ovens (at 2450 MHz) on the electromagnetic spectrum (see figure 1, above). Electromagnetic radiation at these frequencies is commonly referred to as microwaves or radiofrequency (RF) radiation (anything from 300MHz to 300,000MHz (3GHz). Radiation at these frequencies is considered fairly innocuous when compared with established radiation hazards such as X-rays ($>3 \times 10^{16}$ Hz) and gamma rays (anything above 10^{19} Hz). At this end of the electromagnetic spectrum, radiation has sufficient quantum energy (directly proportional to its frequency) to break molecular bonds. X-rays and gamma rays can

[3] BT and Vodafone, the older pair of network operators, use the 900MHz band (GSM900). Orange and One2One use 1800MHz (GSM1800). GSM originally stood for the 'Groupe Spécial Mobile', the working group set up to establish a European standard for mobile networks. However, that acronym has now been usurped in common usage. GSM is now understood to stand for 'Global System for Mobile Communications'.

therefore cause permanent tissue damage and DNA mutation, leading to cancer. Radiation in these frequencies is known as *ionising* radiation (anything more energetic than ultraviolet light). In terms of this known carcinogenic effect, low-powered microwaves are insufficiently energetic, and so impotent in damaging human tissue. Microwaves are therefore known as *non-ionising radiation.* One scientist I interviewed used an analogy to explain to me the vast differences between known radiation hazards and the dangers of microwaves:

> "If you transmit that to football, if you don't mind... David Beckham can... kick the ball about half the pitch. He's Mr Microwave. If he became Mr UV (Ultra-violet light), he could kick the football across the Atlantic Ocean. And if he became Mr X-ray or Mr Gamma Ray, he could kick the football out of the solar system. That's the difference in energy, at the atomic level, that we're dealing with" (Interview transcript, No. 21)

The point he is making is that an individual microwave simply does not have the energy to do anything dangerous. So, if classical physics denies microwaves (non-ionising radiation) the opportunity to directly damage DNA and cause cancer, what dangers do we need to be protected against from mobile phones?

Microwaves, which we can imagine as frequently oscillating electromagnetic fields, can, if enough energy is imparted within a period of time, vibrate charged molecules such as water, which leads to a heating effect in organic tissue. Knowledge of this effect and its mechanism, as we shall see, is at the centre of 'what science knows'[4] about the effects of microwaves. The effect allows for the heating of food in microwave ovens, which radiate at hundreds of watts. But exposure of living tissue to intense microwave heating would cause tissue damage which would lead to cataracts, cancers or foetal abnormalities in pregnant women (NRPB 1982). The heating effect of microwaves therefore needs to be prevented with mobile phones. The power of a mobile phone (less than one watt) is less than a thousandth of the power of microwave oven,[5] but mobile phones are usually held next to a headful of wet, sensitive tissue.

---

[4] This phrase is borrowed from Steven Epstein's (1996) description of the facticity of HIV causing AIDS. Epstein in turn borrows the phrase from Patton (1990).

[5] Mobile phones are not allowed to transmit at more than 2 watts for the 900MHz band or 1 watt for the 1800MHz band. However, a technique called Time Division Multiple Access (TDMA) is used to

For mobile phone technology, protection from known hazards includes not just the exposure of the human head to handset radiation, but also of the human body to the radiation transmitted from mobile phone base stations. In order to protect the human body from thermal harm, scientists and advisors must decide four things:

- What counts as a tolerable increase in temperature?
- What level of radiation absorption induces such a temperature rise?
- How best to calculate that absorption?
- How to numerically represent a maximum safe level?

The purpose of this chapter is to explain how such decisions involve uncertainties, controversies and political choices, and how these choices are represented in public when advisory scientists explain their rationale. The extent to which the contingencies of the underlying science are communicated in scientific advice highlights the prevailing expert construction not just of a scientific consensus, but also of the role of scientific advice and the degree to which scientific knowledge should be demarcated from political decision-making and public challenge. In the UK, the body responsible for sifting the science and deciding how best to advise on protecting the public is the National Radiological Protection Board, who are the main focus of this chapter.

The NRPB

The National Radiological Protection Board was established in 1970 by the Radiological Protection Act, which burdened it with the dual responsibility of conducting research into the health aspects of radiation and advising both the Government and the public. The NRPB originally concentrated on hazards from ionising radiation, responding to scientific and public concerns about nuclear power, X-rays and underground radon. But in the 1970s, concerns began to arise that non-ionising radiation might be harmful in ways that were not well understood by

---

maximise the available frequency channels by splitting each channel into eight slots. This means that a digital phone transmits at an average power no more than 0.25 watts or 0.125 watts (an eighth of the values above).

experts. In 1974, The NRPB's remit was extended to include non-ionising radiation.[6] At around the same time, global scientific and regulatory interest in the potential dangers of an increasing variety of electromagnetic technologies led to the creation of the International Non-Ionizing Radiation Committee (INIRC) (in 1977), as an offshoot of the International Radiation Protection Association (IRPA). In 1992, the International Commission on Non-Ionizing Radiation Protection (ICNIRP) was launched to continue the work of INIRC[7] and we will see later how the relationship between ICNIRP and NRPB, both responsible for setting guidelines, illustrates the dynamics of regulatory science. But first we should consider how the NRPB responded to the first concerns about the possible hazards of EMFs.

The Advisory Group on Non-Ionising Radiation

In 1990, a report leaked from the United States Environmental Protection Agency had classified Extremely Low-Frequency (ELF) EMFs as a 'possible human carcinogen', defined by the presence of some "limited evidence of carcinogenicity in animals in the absence of human data" (EPA 1986, p.20). At this time, the public EMF controversy that attracted the greatest scientific attention was over the possible hazards of ELF fields generated by overhead power lines (at 50-60Hz). Despite the extension of its remit some years before, the NRPB realised that it was not sufficiently able to respond to the growing concerns about non-ionising radiation so, in the same year, a group was formed to better advise the NRPB on the state of the science and directions for future research regarding the health effects of non-ionising radiation. The Advisory Group on Non-Ionising Radiation (AGNIR) was designed as an independent adjunct of the NRPB. It was chaired by the eminent epidemiologist Sir Richard Doll who, since convincing the world of the link between lung cancer and smoking in 1950, had turned his attention to carcinogenic exposures from radioactive sources such as underground Radon. Doll, who had advised the NRPB on ionising radiation issues for the previous decade, was asked to form a committee to look at the more uncertain science of EMFs and human health. (According to my interviews with AGNIR and NRPB scientists, the UK had not

---

[6] NRPB, Extension of functions order, 1974, Statutory instrument Nr 1230 (1974) HMSO. NRPB's non-ionising radiation division remains a lesser interest, using up a fifth of the NRPB's resources (Interview notes, No. 13).

[7] ICNIRP now reports to the World Health Organisation.

seriously considered the issue of health risks from EMFs before 1990). Experts needed to catch up with the public in order to authoritatively deal with any controversies that might arise. AGNIR comprised scientists from areas such as radiation physics and bioelectromagnetics, the fields most closely associated with the knowledge on which advice was based, as well as more generally respected epidemiologists and physiologists.

AGNIR, after two years and seven (private) meetings, produced a report which attempted an advisory summary of the state of the science that had considered the potential carcinogenicity of EMFs. By this time, the report from the EPA had still not been published, (and indeed would never be, despite its leaked conclusion) although other reports had since echoed its conclusions (e.g. NIEHS 1999). AGNIR, who were asked to advise whether the science suggested NRPB should change its existing advice, concluded:

> "Much of the evidence that has been cited is inconsistent, or derives from studies that have been inadequately controlled, and some is likely to have been distorted by bias against the reporting or publishing of negative results... In the absence of any unambiguous experimental evidence to suggest that exposure to these electromagnetic fields is likely to be carcinogenic, in the broadest sense of the term, the findings to date can be regarded only as sufficient to justify formulating a hypothesis for testing by further investigation." (NRPB 1992, Introduction)

This pattern of experts identifying uncertainties as justification for further research will become familiar as my thesis develops. The report went on to recommend some areas for future research, noting the "current paucity of fundamental knowledge on the biological effects of low level electromagnetic fields" (ibid., Report summary). One of the group's conclusions summarised their impression of the area of science that had produced most of the research reviewed in the report:

> "The Advisory Group suggests that more emphasis is needed on the consolidation of 'positive' findings and the formulation of the testable hypotheses necessary for the whole field to progress beyond the largely phenomenological position it currently occupies."[8] (NRPB 1992, Recommendations for Research)

---

[8] This conclusion has echoes of Kuhnian philosophy behind it, suggesting that the scientific paradigm needs to be strengthened for reliable, normal science to be carried out (see Kuhn, [1962] 1996).

As we shall see throughout this thesis, the perceived immaturity of the field that is the source of policy-relevant science in this area affects the credibility of claims that advice is strictly 'science-based'. However, it was considered that the established consensus around the known, thermal effects of EMFs was sufficiently robust to maintain the pattern of existing scientific advice reflecting certified scientific knowledge.

The NRPB's response to the work of AGNIR was a minor revision of the exposure guidelines for non-ionising radiation (NRPB 1993), which had existed since 1989 (NRPB 1989). This original advice, developed over seven years, represented the 'state of the science' at the time. Both this original advice and the 1993 revision contained two sets of guidelines – standards which, if compliance was demonstrated, would protect humans from harm. One set of guideline levels was designed to restrict *exposures* which might cause harm. The other set, called 'basic restrictions', enumerated the levels of *absorption* of radiation that might be considered safe. These basic restrictions, taking into account not only the physics of EMFs, but also the biology of human tissue were based on a derived quantity known as the Specific Absorption Rate (SAR). As the NRPB's SAR values represent the end-point for British scientific advice on the safety of EMFs, it is a good starting point for my investigation of what constituted the advisory discourse when the controversy over mobile phones first emerged.

Constructing SAR Guidelines

Guidelines for exposure to EMFs have existed globally since the late 1950s, although there was originally little agreement between countries as to what they should be (an issue to which we will return later in this chapter[9]). In the UK, the first scientific advice regarding exposure to EMFs was an exposure restriction addressed to workers in the Post Office (Home Office 1960), which, at the time, also managed radio transmissions. In 1989, the NRPB published the first UK guidelines on exposure to EMFs based on a thorough review of the available science. These guidelines, as well as suggesting restrictions on exposure based on field strengths, suggested basic restrictions based on SAR (which measures *absorption* rather than *exposure*), following the lead of American bodies, who had been the first to provide a

[9] A point made at a conference held to discuss international harmonisation, Zagreb, November 1998.

dosimetric set of guidelines (measuring the dose *absorbed* by the body) in the early 80s[10]. Previous guidelines, such as the 1960 Post Office advice, were based solely on exposure levels such as power densities and field strengths (Kuster and Balzano 1997, p. 13).[11]

The 1989 guidance was based on the (scientific) responses from two consultation documents (NRPB 1982 and NRPB 1986). The first of these noted that "the public has become increasingly aroused to the possibility of hazards to health from exposure to non-ionising electromagnetic sources of radiation such as microwave ovens, radar and radio equipment, lasers and overhead power lines" (NRPB 1982, Foreword). All of these technologies had been associated with greater or lesser degrees of health risk.

In 1989, the dosimetric techniques required to calculate the dose of radiation absorbed by the human body were relatively crude, so 'investigation levels' of power density and field strengths were provided. These investigation levels are still used to easily assess whether the radiation to which human tissue is exposed is likely to be safe in terms of absorption. Investigation levels give values for electric and magnetic field strengths which can be easily measured and expressed as a pair of quantities (in Volts per metre (Vm^{-1}) and Tesla (T), respectively).

For mobile phone technology, the investigation levels allow a relatively easy assessment of whether the 'far-field' radiation that is transmitted from a base station complies with guidelines. The relatively simple pattern of exposure of a whole human body in a measurable field makes it easy to calculate absorption. It is therefore assumed that, if none of the investigation levels are exceeded, the radiation absorbed will be less than the basic restriction level. However, when a mobile phone is pressed against the human head, tissue is exposed to the 'near-field' of the antenna.[12] The interaction of this 'near-field' with the human head is more complex,

---

[10] ANSI (American National Standards Institute) 1982 and NCRP (National Council on Radiation Protection) 1986

[11] ...although the restriction on field strength was derived, in a rudimentary fashion, from a suggestion to keep absorption below 1 watt per kilogram.

[12] In an effort to better understand the intricacies of near-fields and far fields, I posted a message to the Bioelectromagnetics mailing list asking for a typical near-field radius at 900 and 1800MHz. The responses demonstrated that even the most mundane of questions can be controversial. Calculations

so exposure levels are not useful for demonstrating safety or harm. Absorption in the head has to be calculated directly to get an accurate picture of the possibility of a hazard. This absorption of the radiation is expressed as the Specific Absorption Rate.

The Politics of SAR

> "There is a consensus that an important dosimetric measure of RF exposure is the specific absorption rate (SAR). This is the unit-mass, time-average rate of RF energy absorption specified in SI units of watts per kilogram (W/kg)... thus the SAR is the rate at which RF electromagnetic energy is imparted to unit mass of a biological body." (Michaelson and Elson 1996, p. 441)

SAR is seen by most scientists as the best available metric for the restriction of well-understood hazards from EMFs. Calculation of SAR and the construction of guidelines based on the restriction of SAR are therefore seen as scientific, and hopefully authoritative. However, SAR is not the only metric that can be used to indicate the level of non-ionising radiation absorbed in the head or the body. Over the course of the public controversy, SAR has acquired a political voice, being seen as the tactic of one constituent in a debate, rather than an unequivocal answer. To understand why, we need to unpack the assumptions that SAR embodies. The acronym alone tells us that SAR is a measure of *absorption*, a measure of the *rate* of absorption and it is *specific* (to the tissue that is absorbing the RF energy). In addition, as the excerpt above notes, it is 'time-averaged', which will be discussed in more detail below.

With base station exposure, the human body is normally exposed as a whole to the far field radiated by the antenna. For exposure from handsets, the human head is exposed to the near-field of the antenna, so the absorption only occurs in the head (and perhaps in the hand holding the phone). Calculations of SAR in either case require knowledge not just of the physics of the fields, but also of the dielectric

---

varied from one scientist to the next, with disagreements about the correct formula and the relevance of the near-field measurements. One list member asked why a sociologist would need to know such a thing. (See bioelectromagnetics mailing list archive, November 2003 http://groups.yahoo.com/group/bioelectromagnetics/messages/2093) properties (such as permittivity and conductivity) of the tissues that are absorbing the radiation.

SAR is calculated in complex dosimetric models whose success depends on the accuracy with which they can simulate the complexity of human tissues. [13] Calculations for SAR over the whole of the human body are relatively straightforward. However, in the case of handsets, because of the unpredictability of near-field radiation from antennae, the absorption cannot be predicted by the fields that emerge from the antenna. Normally over half of the radiation emitted from a handset antenna is radiated away from the head, carrying information to a base station. However, the remainder is absorbed by the tissues of the brain, the skull, the ear and possibly the eyes. The near-field is not directed, so it cannot easily be engineered to radiate away from the head. To establish the SAR of individual handsets in relation to the basic restriction levels therefore, each phone must be separately modelled, which raises problems of comparison and demonstrating the SAR in relation to the guideline levels (see Kuster 2001 for a review). Dosimetry is therefore driven by an unending need to more accurately model SAR as a standard quantity.

As we will see in this chapter, there is no single arrow from good science to good policy in the setting of standards. In setting SAR guidelines to protect people from mobile phone radiation, advisory bodies such as the NRPB use what they consider to be the most robust scientific knowledge currently available. But they must unavoidably make some decisions that are not easily defended in scientific terms. According to the NRPB, current scientific consensus is that an increase in body temperature of 1°C caused by radiofrequency radiation is tolerable, and the mechanism for it is well understood (this is less than would be experienced with a mild fever, or a period of exercise). The SAR which produces this increase is calculated at between 1 and 4 watts per kilogram,[14] and the SAR guidelines for

---

[13] SAR can be determined by one of two methods. Experimental dosimetric techniques use physical models (sometimes called 'phantom heads') filled with liquids which simulate brain tissue (e.g. Manning and Densley 2001), which can be effective for measuring the SAR of handsets in models of the human head. The alternative is numerical dosimetry, which uses computer models of the human body (or parts of it) based on MRI imaging data (e.g. Dimbylow and Mann 1994).

[14] There is "a consensus opinion… that reliable evidence of hazardous effects is associated with average whole body specific energy-absorption rates in excess of 4 W kg^{-1}"(NRPB 1986, p. 13)

exposure to the body as a whole in the UK are 0.4 watts per kilogram of tissue (averaged over 15 minutes of exposure).[15 16] This difference of up to a factor of ten is seen as a 'safety factor', a *conservative* estimate for exposure. This safety factor, as hinted at by the use of the word 'conservative' (albeit with a small 'c') is a source of some political disagreement between regulatory bodies, as we will see later.

The above paragraph reminds us that SAR levels are derived from a known hazard of overheating. So they assume a thermal effect and aim to protect against it. However, SAR has little to say about any other possible effects which might occur without a noticeable rise in temperature. For those people who claimed that potentially harmful effects could arise without heating ('non-thermal' effects, see below), SAR was seen as answering the wrong question. To those outside the scientific orthodoxy who felt that the public was not being adequately protected, the NRPB guidelines (just as with the ICNIRP guidelines abroad) came to represent an institutional defence of a safety consensus with questionable foundations. As the controversy developed in public, the assumptions behind guidelines based on SAR measurement were challenged – explicitly by interest groups, and implicitly by the growing areas of scientific uncertainty. Some claimed that the guidelines were set far too high, others that the guidelines should not be based on SAR at all.[17] To understand the types of criticism that were levelled at the scientific establishment, we must consider the assumptions that lie behind using SAR as the value for the determination of safety.

We have seen that SAR restrictions aim only to prevent a known danger of excessive heating. But SAR also only regulates a dose *rate*, rather than a total dose. It gives a value for the absorbed energy per unit time, rather than a value for the total energy absorbed by tissue over a period of time. A scientist explained to me the thinking behind the concept.

---

15 Minutes of evidence to the IEGMP from Alastair MacKinlay, NRPB, 8th October 1999

16 For exposure to the head, the SAR guidelines are set at 10 watts per kilogram, averaged over 10 grams of tissue, for six minutes (it takes about six minutes for the head to reach a thermal equilibrium, see below).

17 Questioning the assumption that thermal effects are the only effects has led some critics to suggest that SAR values might be so irrelevant that a lower SAR would not necessarily mean that a source of

> "...if you're considering thermal effects, then the thing that matters is the temperature rise. Now the temperature rise does depend of course on the SAR and the time for which the person is exposed to the microwaves. But then you reach an equilibrium position [after about six minutes], because of the cooling processes [of the human body]... As soon as the microwaves are turned off, you cool down and that's the end of it, there's no permanent effect carried over. The dose isn't cumulative in that sense." (Interview transcript, No. 31)

The six minutes to reach thermal equilibrium explain the rationale behind averaging exposure over six minutes, but SAR calculations do not take into account the length of time spent exposed to EMFs once this equilibrium temperature rise is experienced. The distinction between dose rate and cumulative dose becomes important when comparing the possible hazard of, for example, the absorption from ten six-minute calls with that of one hour-long call, or living close to a permanently-transmitting base-station. It follows that SAR indicates only the potential of an acute hazard rather than a chronic one.

SAR does not take into account the differences between continuous and pulsed (digital radiation). Digital radiation, such as that emitted by the current (GSM)[18] generation of mobile phones, is modulated so that it pulses at 217Hz. The results of some research has suggested that pulsed radiation might interact with the brain in a way that continuous-wave radiation does not.[19] Pulsed radiation also emits peak powers which might exceed SAR restrictions (albeit for a very short time), which were designed to limit absorption from more uniform exposures.

These assumptions, and others,[20] have emerged as targets of contestation in the scientific and public debate as we shall see in further chapters, but uncertainties also

---

radiation is any safer. Much of the discussion that occurs on email lists such as EMF-L centres on the irrelevance of SAR in determining safety.

18 'GSM' began as an abbreviation for the standard developed by the Groupe Spéciale Mobile. It is now understood as an abbreviation for the Global System for Mobile Communications, a European standard. The American standard uses different frequencies.

19 For example, a number of studies have demonstrated effects with microwave radiation pulsed at 16 Hz (e.g. Adey et al 1982).

20 The more a standard is picked apart, the more assumptions can be revealed. SAR levels, for example, are given for a range of frequencies. But any risk might occur only within narrow frequency bands – a so-called 'window effect' (see Postow and Swicord 1996)

exist about the ability of science to accurately determine the level of SAR and so demonstrate that a handset falls below the basic restriction.

Before 2000, there was no standard dosimetric technique so, for example, comparing two handsets evaluated in different laboratories was problematic. The recent gradual introduction of the European CENELEC standard has facilitated comparisons and requires the demonstration of compliance for a cross-section of users of different sizes and shapes and with different ways of holding their phones.[21] However, it remains impossible to know the power absorption in complex tissues. Uncertainty exists as to whether the level of heating in all parts of the body is being accurately calculated, or whether localised heating in certain pasts of the brain might still be occurring at worrying levels.

The assumptions that underlie the calculation of SAR and its selection as the relevant metric for setting guidelines, are based on a scientific consensus as to the known effects of non-ionising radiation. For those involved in research or scientific advice, SAR exists as an authoritative representation of certainty, of a known effect. At the same time, the accurate calculation of SAR is constructed by those with an interest in radiation protection dosimetry as the most problematic area of uncertainty, with adjustments in levels prompted (occasionally) by improved dosimetric modelling. However, for critics, SAR guidelines came to embody their underlying assumptions, providing a target for criticism of the consensus that supports them. SAR-based guidelines therefore became the battleground for challenges that are not easily categorised as scientific or political, justified or unjustified. To consider how the NRPB, industry and others constructed the authority of the guidelines and the consensus behind them, we can look towards the challenges to the status quo that emerged from the public context of what had previously been expert negotiation of policy and uncertainties.

---

[21] CENELEC European Specification ES59005 October 1998 – "Considerations for the evaluation of human exposure to Electromagnetic Fields (EMFs) from Mobile Telecommunication Equipment (MTE) in the frequency range 30 MHz - 6 GHz." CENELEC (European Committee for Electrotechnical Standardization) are an organisation representing 22 European countries (including the UK) working together develop international standards.

The Beginnings of Public Uncertainty

I have already described how the mobile phone controversy began in as much detail as my study allows (see Introduction). However, before continuing to consider the advisory response to health concerns about mobile phones, we should draw out some of the key factors that contributed to the growth of the controversy (I refer the reader again to Burgess (2004) for a broader analysis). I will address three major factors, though I stress that none of these can be seen in isolation from any other. Firstly, the growth in mobile phone usage, transforming mobile phones from a minority technology to a necessary one. Secondly, the publicity given to scientific studies reporting effects not easily-explained with a thermal mechanism. And thirdly, reports of symptoms experienced by users attributed to mobile phones, and the publicity granted to these reports by media.

Expansion of Networks and Markets

In the 1990s, a combination of technological innovation, industrial expansion and public demand contributed to the rapid growth of mobile phone technology.[22] As mobile phones emerged as a 'risky' technology, in the late-1990s, ownership in the UK expanded from 9.1 million (1997/8) to 30.5 million (June 2000) (and would continue upwards to over 50 million by the time of writing).[23]

As the number of mobile phones increased in the UK, along with both promises of, and demand for, universal coverage, mobile phone networks needed to accommodate an increasing volume of mobile phone traffic. Busy areas such as cities required smaller cells (areas served by one base station) so that the limited number of channels each base station provides could be used again in a nearby area. As the number of users increases, base stations need to be closer together and increase in number. The rapid growth in mobile phone uptake therefore required a rapid expansion of the number of base stations required for reliable networks. But the effect this had on the radiation to which mobile phone users would be exposed is not straightforward.

---

[22] It is beyond this thesis to offer a causal explanation of the growth in demand for mobile phones relative to innovation.

[23] Statistics of mobile phone ownership from the web site of the Mobile Operators' Association, http://www.mobilemastinfo.com/information/history.htm, accessed 27th October 2003

Mobile phone handsets, using a technique called 'adaptive power control', adjust their radiated transmission power. This allows the handsets to use the minimum power necessary to maintain effective communication with a base station. This means firstly that batteries can be smaller, cheaper and longer-lasting, a feature which contributed greatly to the ubiquity of mobile phones. Secondly, it means that there is less chance of interference. Finally, adaptive power control means that radiation emitted will be reduced when the signal is strong (generally when the caller is close to a base station). So as the number of base stations increases, both handsets and base stations become less powerful. Mobile phone handsets, when they were first produced, exceeded the guideline levels that existed at the time. Inefficiencies in their design and the small number of base stations necessitated their operation at higher powers (Around 0.6 watts). In many countries, guidelines created for other non-ionising radiation technologies formed binding regulations, so handsets needed to be excluded, usually with the rationale that users took a positive choice in their exposure (Kuster and Balzano 1997, p. 17). Mobile phones now are more efficient, and can operate at low power because of the large number of available base stations. The general pattern therefore, has been a downward pressure on radiated power from the economic incentives of increased efficiency and increased usage. For the modern generation of mobile phones, compliance with guidelines such as those of the NRPB and ICNIRP is no longer a problem, although low power is a consideration in the design process.

As mobile phone networks expanded, people began to notice base stations being erected, many of them exempt from requiring full planning permission. Previously invisible networks, supported by a small number of roadside masts, began to affect the well-being of those who lived or worked nearby, many of whom felt powerless to object. Reports of masts being erected in the mid-late-90s, at the time of most rapid network expansion, were picked up by news media (especially local news) eager to report the injustice, particularly where schools or council properties were involved. Network operators began to develop reputations based upon their attitudes to public consultation or sensitive siting of masts. The most heavily criticised was Orange, one of the newer pair of networks, who were driven by a need to build a network from scratch once their licence was awarded in 1994.

Alongside this expansion, aided by publicity from interest groups and dissenting scientists, scientific research emerged in the 1990s that suggested effects which seemingly weren't considered in the available scientific advice.

"Nothing that you would want to lose sleep over" – The science of non-thermal uncertainty

This chapter has so far largely discussed the consensual knowledge that science has about the effects of microwave radiation and the implications of this knowledge for setting guidelines. However, a crucial contested feature of the public controversy over mobile phones has been the relevance of effects which have been detected by some researchers at levels that the guidelines would suggest were safe. As the public scale of the controversy grew, news media began to pick up on any scientific publication that reported some effect on humans, animals or cells.[24] There was no particular novelty in the reporting of biological effects of non-ionising radiation at low-levels. Such 'non-thermal effects', detected at exposures below those suggested by guideline levels, had been reported for almost a decade:

> "Oh, there've always been hints of things. In '91 and '92 we did reviews of biology, so it was at least ten years ago… but nothing conclusive. Nothing that you would want to lose sleep over… but it's just, there's always been hints." (Interview transcript, No. 32)

One scientist went even further, claiming:

> "there have been reports on non-thermal effects ever since I've been working in the field, which is 50 years now. This is nothing new." (Interview transcript, No. 31).

As mentioned in the introduction, almost any technology generating electromagnetic radiation has been associated with a health controversy, accompanied by scientific reports of unexplained health effects (e.g. power lines, radar, microwave ovens and

[24] There was also misguided reporting of the known, but less significant heating effect: e.g. "This is how a mobile heats your brain" (Front page of the Sunday Mirror, 7th March 1999). Metaphors of cooking, scrambling and microwave ovens have abounded in the UK's tabloid newspapers (see Stilgoe 2001b).

TV and radio transmission towers)[25]. Mobile phones, being the most recent popular EMF technology, have attracted the most recent concern, both scientific and public.

Much of the first research that looked specifically at mobile phones in the 1980s had been sponsored by mobile phone manufacturers. The industry calls this "due diligence research",[26] fuelled by concern that some potentially threatening gaps in scientific knowledge had been opened up by research in previous decades.

In the 1990s, however, the public concern around mobile phone safety, coupled with the massive growth in their uptake, prompted more scientists to work with the possible health effects of mobile phones (Interview notes, No. 31). Scientists from oncology, epidemiology, cell biology and neuroscience began to research around the question of whether mobile phone radiation might be harmful. Established scientists who would place themselves in the more specialised field of bioelectromagnetics (the science of interactions between living bodies and EMFs) continued to present novel findings.

The studies that attracted media attention in the mid-late-1990s reported effects for *in vivo* (live animal) and *in vitro* (cell culture) models, as well as increased relative risk in epidemiology. There had been myriad studies reporting effects, and it would be pointless to offer a review here, but some studies particular attention. I outline some of these to illustrate how their results might challenge the existing scientific/regulatory regime and shape public concern. The following studies are those that were namechecked most often in the news in the late 1990s, and continue to be cited today by non-experts highlighting uncertainties. They are provided as examples of the kind of research that was being done, and reported on, at the height of public concern about mobile phone risks.

Perhaps the most regularly cited research, by both scientists and the news media, claimed that pulsed, low-level, non-ionising radiation caused detectable breaks in the DNA of rat brains (Lai and Singh 1995, 1996). Henry Lai and colleagues had previously conducted work and published a huge number of well-respected papers with radiation at radar/microwave oven frequencies (2.45 GHz). Much of this

---

25 Paul Brodeur, a key contributor to these early controversies, provides a good popular summary (see Brodeur 1989). For a scientistic response, see Park (2000, chapter seven).

26 Quirino Balzano, Motorola, Evidence to the HCSTSC, 16 June 1999

previous work had been behavioural (looking at how an animal's ability to perform tasks is affected by microwave exposure). The experiments that attracted so much attention for their implications for mobile phone risk used 2.45 GHz pulsed and continuous radiation, and the SAR was calculated at 0.6-1.2 watts per kilogram, comparable to that from a mobile phone handset and below the NRPB's guideline levels. Although the frequency of radiation was higher than that used by European mobile phones (0.9 and 1.8 GHz), this research persuasively suggested that DNA can be damaged by low-level non-ionising radiation.

In 1997, a study conducted at the Royal Adelaide Hospital was reported in a paper which claimed that exposure to GSM-type radiation (900 MHz, pulsed at 217 Hz) caused a doubling in the incidence of lymphoma in transgenic mice (carrying a lymphomagenic oncogene, and so predisposed to this type of cancer). This study (Repacholi et al 1997) was headed by Mike Repacholi, who would later appear (in an advisory capacity) both in the Stewart Group (see chapter five) and at the World Health Organisation (WHO), where he ran their International EMF Programme. Again, the absorbed dose rate in this study was calculated to be not more than would be received from a mobile phone, although the animals in both studies were obviously much smaller than the humans for whom the risk was being assessed.

In the UK, one study that attracted a great deal of attention while the news media were at their most excitable looked at changes in reaction time being affected by exposure to 900 MHz analogue and GSM-type pulsed radiation (Preece et al 1999). Alan Preece's study was the first to look at the direct effects of mobile phone emissions on the performance of the human brain, and claimed that reaction time was slightly reduced after exposure, i.e. reactions improved.[27] While this could be seen as a beneficial impact of mobile phone radiation, the study implied that mobile phones could affect cognition, suggesting shortcomings in current regulation and the established consensus of safety.

Another UK study (de Pomerai *et al 1999*) looked at the effects of mobile phone radiation on transgenic nematode worms, modified to indicate when they are producing heat-shock proteins. The results implied that the worms were becoming

---

[27] ...although some news reports of the then-unpublished study claimed it had demonstrated the opposite effect ("Mobile phones slow the brain in new tests", Sunday Times, September 20, 1998)

stressed, as if by a heat source, when they were exposed to mobile phone radiation, but that a temperature rise could not be detected. As with the response to radiation in the Preece study, the effect could be seen as beneficial, with heat-shock proteins providing a degree of protection. However, as with the Preece study, the study was represented in the news as demonstrating possible harm. David de Pomerai's study also observed the animal model over a longer period of exposure (seven hours) than many others had.

These studies, and the many others that had produced similar results over the past decades, provided positive evidence of the presence of non-thermal effects. However, as well as questioning the existing boundaries of scientific consensus, they implicitly (and sometimes explicitly) questioned the assumptions behind the existing guidelines. Many of the relevant studies used animal models that some argued were insufficient to demonstrate a risk. But, in public, the results demonstrated that effects were occurring beneath the guidelines, which might be worrisome, or that the basis of current regulation on SAR levels (with assumptions of acute, thermal danger) was inappropriate.

These non-thermal effects are often referred to as "subtle biological effects", to distinguish them from the more cumbersome and easily-explained mechanism that classical physics provides for microwaves heating and harming tissue. The 'subtlety' of these effects and the absence of both an established mechanism for their occurrence and a link to any effect on *health* reduces their power to question the regulatory consensus. For those scientists who feel that the only possible effects of non-ionising radiation are from heating, claims of non-thermal effects are seen as at best interesting and, at worst, artefacts of bad experimental design. Ross Adey, an established scientist who was among the first to report on non-thermal effects, offered his impression of this attitude in a review of research in the area:

> "In an historical perspective, growth of knowledge in bioelectromagnetics has occurred in the face of pre-emptive views on the part of some physical scientists that athermal [non-thermal] effects are in the realms of spurious or pathological science, because observed sensitivities relate to fields that lack sufficient energy to break chemical bonds." (Adey 1997 p. 101)

This quote comes from a scientific review which necessarily probes uncertainties and current areas of concern and research rather than reminding readers of the existing consensus.[28] Studies providing positive results are thus emphasised although they are not supported by a weight of evidence and theory. However, for any public health issue, the importance of a few positive results, even if contradicted by a barrage of negative studies, cannot be underestimated. The *suggestion* of effects which *might* be harmful is often enough to sway opinion. (This quote also illustrates an important tension between the known bedrock of physics and the experimental results of biology. I will problematise this distinction in the next chapter).

Non-thermal effects became less and less controversial in the 1990s. It began to be accepted among scientists that effects probably did occur that could not be explained as a product of heating. One NRPB scientist described to me the general pattern:

> Q: Had I asked [about] the scientific consensus five years ago, would they have agreed that there were biological effects [from low-level microwaves]?
>
> A: Probably not. Difficult to answer that... I think they knew the biological effects were there but their potential importance is now recognised. But the example I give of the biological effects and the scepticism is that you can, if you have things in a Petri dish, and what people do is they have some cells and they stick a mobile phone there, or simulate it, you can see some effects. Put a grain of salt in, they all bloody die. Put Coca-Cola in it? Fries them. Hostile gases? Absolutely knackers the cells. Caffeine, bit of instant coffee? completely knackers the cells... do you see what I mean? Just because there's a biological effect observable [it doesn't mean there's a health effect]. You cannot make that jump... If there could be cellular changes going on in the brain, who knows what the results could be?... the scientific consensus is we've certainly got to get to the bottom of these biological effects." (Interview transcript, No. 21)

We can see uncertainty being constructed at an expert level. Effects are identified as requiring further expert attention. But there is a strong distinction made between

---

[28] This quote illustrates a tension between the known bedrock of physics and the experimental results of biology. This tension, and the conflicts of understanding that arise between scientists who might refer to themselves as physicists or biologists was often expressed to me in interviews with researchers.

biological effects and health effects. It would require another level of uncertainty to suggest that the non-thermal effects detected might be harmful. So, even though they might exist, non-thermal effects are, to paraphrase the scientist quoted above "nothing to lose sleep over."

And yet there were suggestions of harm from mobile phones, suggestions that were seized upon by those trying to convince bodies such as the NRPB to appreciate more serious gaps in expert knowledge. Some evidence of harm was scientific, but most was from members of the public reporting symptoms that they ascribed to their mobile phones or their proximity to a base station.

One epidemiology study, conducted in Sweden and Norway, had provided a suggestion of an increased risk of brain tumours from mobile phones (Hardell et al 1999). This study has been much criticised. In public, it was deemed by some scientists that the results were not statistically significant and that the pre-release of findings was irresponsible (see Microwave News, Sept/Oct, 2001). The study was based on a survey in which sufferers ('cases') and controls reported on their mobile phone use, raising the question of reporting bias. In my interviews, it emerged that other epidemiologists considered that Lennart Hardell got the result he was looking for from the outset. One interviewee referred to him as an "epidemiological twister."[29] But this study investigated comparisons between heavy and light mobile phone use, asking questions that others had not previously considered. The results of this epidemiology study were publicised by an edition of the BBC news documentary programme 'Panorama' in May 1999, which compiled testimony from some of the other most prominent scientists in the area to further doubts about mobile phone safety. This documentary, which aimed to illustrate uncertain scientists, an industry cover-up and injured members of the public, began with the testimony of a man suffering from a 'mystery illness' caused by his excessive mobile phone use.[30]

---

[29] As an aside, in the 1970s, Lennart Hardell had controversially produced epidemiological evidence that the herbicides 2,4-D and 2,4,5-T (the constituents of Agent Orange) were carcinogenic. Richard Doll, who was to go on to become the chairman of AGNIR (see above), criticised Hardell by saying that the opportunities for bias in his study meant that "his work should no longer be cited as scientific evidence." (See Hardell et al 1998 for his own account of these arguments).

[30] BBC TV, Panorama, 'Mobile Mystery', 25th May 1999

Reported symptoms and lay construction of uncertainty

Most of the evidence of the harmful effects of mobile phones arose from members of the public reporting their symptoms (often referred to as 'anecdotal evidence', the focus of chapter six). These reports were used by newspapers to further claims that mobile phones induced a range of symptoms from headaches and sleep-loss (I refer you back to the main title of this section – 'Nothing to lose sleep over') to fatal brain tumours. Adding weight to these claims of harm was a small body of dissenting 'experts', often contacted as part of a media desire for balanced reporting. People such as Gerard Hyland (a Biophysicist from the University of Warwick), Roger Coghill (an independent researcher and campaigner) and Alasdair Philips (an activist and the face of 'Powerwatch') provided alternative views of the scientific evidence to back up the reported symptoms that most often formed the backbone of news stories.[31] Often, the public, with the help both of newspapers and some dissident expertise provided evidence of clusters of symptoms ranging from cancer to sleep-loss (just as had been done with the power lines controversy a decade and a half before).

Building concern

Claims such as those above, with varying degrees of expert accreditation, constituted challenges to a scientific consensus and a regulatory philosophy that, it was felt, was not adequately protecting the public. As more attention was paid to scientific uncertainty (see chapter five), accumulating such claims contributed to a consistent picture either of established harm or of worrying uncertainty. In public, the weight of evidence behind the existing consensus meant little, because these claims represented a challenge to just this consensus. Positive non-thermal studies therefore provide a much more persuasive claim than negative ones.

Uncertainty, defined here as the suggestion of non-thermal effects, has always existed about the possible health effects of mobile phones. However, as we have

[31] The news media, after the crisis of authority and trust caused by the mismanagement over BSE (involving the dissident expert Richard Lacey) were no doubt eager to represent the views of an expert who might turn out to have been correct from the start. However, it should be made clear that the experts called upon to discuss mobile phones were by no means as well-certified as Richard Lacey was.

seen, this uncertainty became a public resource rather than simply an expert one (a theme developed more in the next chapter). One can easily understand why experts, who aim to balance all the scientific evidence to authoritatively declare 'the current state of scientific knowledge', might feel the need to retain close control of scientific uncertainty. Once uncertainty is a public, political resource, positive effects become a method of indelibly defining a controversy.

The gaze of opposing scientists or other outsiders can expose the foundations of a consensus as weak. One Australian activist, Don Maisch, writing about the state of the science of mobile phones and health, echoed the views of many critics:

> "The decision to choose tissue heating as the key exposure parameter was based more on a lack of scientific data than for positive reasons, however it quickly gained favour with both the military and industry as it created something that could be claimed as a safety standard, and avoided (without openly dismissing) the possibility that low-level, non thermal health effects could exist without tissue heating.
>
> The "thermal school of thought" quickly became the accepted norm with Western standard-setting organizations and as a result the vast majority of "science based" research was directed at short term, high level exposures. Research into prolonged environmental level exposures that did not cause tissue heating was not encouraged, simply because it was perceived as a possible threat for technological development." (Maisch 2001, p. 4)

Maisch identifies a consensus that he sees as preventing productive research from being funded. The consensus, he and many others suggest, was built up because of the comfort of existing knowledge rather than as the likely source of greatest harm. He notes that current science and guidelines concentrate on acute, rather than chronic exposures (one of the assumptions behind SAR described above), and suggests that the consensus served political ends for both scientists and industry. He points out that a consensus can exist that obscures unsightly results and constrains research which might be in the public interest.

The edition of Panorama referred to above adeptly constructed an image of experts in complete ignorance of the dangers of mobile phones. Among their sources were the scientist Ross Adey (mentioned above) and Louis Slesin, editor of Microwave News, an influential newsletter. Discussing a study that Adey had conducted on behalf of Motorola, which had suggested a beneficial effect on tumours in rats, the programme's transcription quotes them as saying:

> Ross Adey: Going up or down, with respect to tumour numbers, is less significant than that there is an effect.
>
> Louis Slesin: Because once you open that Pandora's Box and allow that microwaves at these levels can have these effects, then you have to ask the next question, what else can it do?[32]

Uncertainty, once opened up, can be reasonably expanded in any number of possible directions.[33] Once non-thermal effects are seen, observers might ask how anyone can be sure that these will not lead to harm? How can we be sure that these effects are not cumulative, causing more and more damage with chronic exposures? How can we know that the older studies behind the consensus on thermal effects apply to mobile phone frequencies as well as the frequencies at which they were conducted? A consistent picture can be built up that very little is known about mobile phone health effects.

So how did the gatekeepers of the scientific and regulatory consensus react to such challenges to their authority? The defensive rhetoric that emerged from the debate at this stage illuminates the attempts made to maintain the authority of both science and scientific advice.

A Discourse of Compliance

One scientist I interviewed was certain that mobile phones presented no risk, and that non-experts (and some experts) did not appreciate the weight of the science that established their safety:

---

32 BBC 'Panorama' transcript, 24th May 1999

33 The expansion of uncertainty has led to news stories placing the blame on mobile phones for a number of phenomena. As well as the huge collection of symptoms that have appeared, mobile phones and their networks have been blamed for the disappearance of Britain's sparrows (The Observer, January 12th 2003) and, more satisfyingly, the falling number of appearances by ghosts (Sunday Express, October 14th 2001).

> "So, when you get a scientific consensus, it's something that the public doesn't understand because they're not part of and nobody tells them, but it's a very, very important thing to have, and it's a very powerful thing." (Interview transcript, No. 27)

The point made by this interviewee is interesting in the light of the previous discussion, and prompts us to think how the features of a scientific consensus and its underlying uncertainties do get communicated to non-experts in a controversy. I argue in this section that there is an overriding 'discourse of compliance' operating at the boundary between those who police the science (experts) and those who scientific advice is designed to protect (the public). This discourse, which emphasises compliance with standards as the endpoint for discussions of safety, both relies upon, and aims to strengthen, the scientific consensus. But as the consensus is communicated and formalised in scientific advice, the contingencies and modalities that form an integral part of any science are erased. At the same time, the political boundaries of participation are sharply defined.

The supporters of the discourse of compliance are the two groups who have held stakes in the authority of scientific advice from the outset, regulatory authorities and industry (phone manufacturers and network operators).

The position of the NRPB is that it can only provide advice based on the available scientific evidence (NRPB 1993). The SAR levels (and their accompanying investigation levels) that emerge as the practical prescriptions of this advice are therefore viewed as a representation of robust scientific certainty, providing protection against a *known* hazard, that of the heating effect of high-powered microwave radiation. This is from an interview with an AGNIR member:

> "Risks might arise from that part of the electromagnetic spectrum [microwaves] through molecular agitation, ionic agitation and microwave heating. There's a very well-established basis for that. Radio-frequencies do cause heating, and if it's sufficiently severe, it can cause medical problems. There's a vast literature on the effects of heating. And there are I think… rationally-based, well-worked-out regulations from NRPB and a number of other of other agencies, all based on that." (Interview transcript, No. 6)

The scientific consensus, enshrined in guidelines and the research practices of many large programmes and bodies is that, while uncertainties exist, the established effects of RF radiation occur from heating. Such effects are well-established, and evidence to suggest other effects is seen as weak. NRPB standards are 'science-based', which makes them apparently easy to defend on grounds of 'rationality'. They are also constructed independently of the technologies to which they might apply. It does not matter that base station exposures are a minute fraction of those from handsets (typically thousands of times lower), both comply with the guidelines, and so both are seen as safe. It will become apparent in this section that such a discourse serves political ends for scientists, policy-makers and industry, setting the public agenda for reasonable debate.

As an introduction to the discourse of compliance, I will illustrate how a suggested policy initiative (the labelling of mobile phones' SAR values) forced a defensive reaction from industry and the NRPB.

Compliance and labelling

The public context of mobile phone health concerns has prompted policy debate in a number of areas. One of these is the question of what information should be made available to the public. Researchers for the episode of 'Panorama' mentioned above had revealed large differences in SAR levels between different models of handsets (all of which were below the basic restriction level). It was therefore suggested that consumers might appreciate the choice of a handset based on a lower-than-average SAR. A month later the House of Commons Select Committee on Science and Technology held meetings to discuss the issue of mobile phones as part of a parliamentary review of the provision of scientific advice. As part of the industry's evidence to the committee, David Brown, the Chairman of Motorola in the UK, responded to the question of informing the public about SAR values on phones:

> "SARs, specific absorption rates, as the Committee may already know, are the units by which the standards are set and with which we are all complying. I am an engineer not a scientist but I am given to understand that once you are below the standard set for SARs then all phones are equally safe; there is a scientific effect called the cliff effect... once you are below that level, then you cannot use SARs to discriminate between phones. It is not meaningful, it is not understandable. That is the reason why we do not. That is the reason why we say the important thing is to be able to state categorically that the mobile phones meet the SAR standards set by the NRPB and not then go on to risk confusing the public about what relative SARs below the safe

> level really mean... we think it is entirely inappropriate to put precautionary health labels on products for which there is no evidence to suggest they are unhealthy. That would be to deepen the mis-characterisation issue of the underlying science. Mobile phones are safe and that is that... They are all safe."[34]

This long quote reveals a number of discursive features. The argument follows these lines: All phones comply with standards (which are only properly understood by the scientists who set them). Once a phone is classed as safe, there is no point in discussing the issue further. The director of the NRPB, giving evidence to the same committee, gave a similar impression, albeit less forcefully:

> "From a radiological point of view I believe that all marketed telephones meet our exposure guidelines and as such there is no need for any further consideration."[35]

I asked an industry representative about labelling phones with their SAR values:

> "We don't believe that's the right way to go, because that undermines the existing regulatory regime. We already have in Europe a CE mark. Putting a CE mark on a product means that it complies with all European directives, including safety. So by putting the CE mark on there, we're actually certifying that it meets the European recommendations, so we don't see the need for any additional labels, it's confusing to consumers. There's no rationale to say that a phone with a SAR level of 1 rather than a SAR level of 2 [watts per kilogram], there's no relative safety, because SAR was always a pass/fail test... The important thing for us is that, whatever we do, it's fact-based." (Interview transcript, No. 25)

This comment, that SAR guidelines represent a pass/fail test, typifies the industry and regulatory view that the *only* relevant level of absorption is the one at which known (thermal) effects might begin to pose a problem. Below this level, because reproducible effects with known health implications cannot be detected, any differences in SAR are irrelevant. Public concerns, or demands for a phone with a low SAR value (at, for example, a tenth of the guideline value) are seen as unwarranted, based on erroneous assumptions. Labelling phones therefore challenges the scientific consensus without good (scientific) reason. A challenge to a compliance-only attitude is a challenge to the well-established consensus behind

---

[34] Minutes of verbal evidence to the HCSTSC, 16th June 1999

[35] Minutes of verbal evidence to the HCSTSC, 9th June 1999

thermal-based guidelines. Non-experts, it is assumed, are not cognitively-equipped to make such a challenge, so the discourse acts as a barrier to challenge from outside.

These quotes illustrate the prevailing discourse, and the space that is allocated for non-expert debate. The whole field of measurement and SAR is cordoned off by expertise, with the non-experts left only with the question, "does it comply?" According to this discourse, the introduction of labels is therefore inviting non-experts to make decisions that, firstly, do not need to be made and, secondly, they are not qualified to make.

The 'science-based' guidelines and the discourse of compliance around them allow industry to largely ignore health issues. Industry does not form a part of this consensus, indeed it strives to prove its independence from the science which exonerates its technologies, much of which it funded in the 1980s. Tom-Wills Sanford, from the Federation of the Electronics Industry, was asked by the Select Committee for his attitude to the NRPB's guidelines:

> (Michael Clarke, [Committee chair]) "The National Radiological Protection Board (NRPB) has a set of guidelines which are generally looking at the thermal effects from mobile phones. Do you think these standards should be revisited? Are they something which should be updated in light of the large use now of mobile phones and the proliferation of them?"
>
> (Mr Wills-Sandford) "We are very happy to leave that judgement to the NRPB. We have great faith in them and, as our memorandum says, we believe there is a large number of highly respected international scientists in the NRPB, so we are very happy to leave that judgement to the NRPB."
>
> (Michael Clarke) "You are just interested in selling phones and you do not really care about the standards to which they are sold. You leave that to somebody else."
>
> (Mr Wills-Sandford) "Not at all. We are interested in selling phones and services and so on which comply with internationally agreed guidelines."[36]

Compliance with ICNIRP (international) guidelines is taken into account at the early stages of the engineering process (Interview notes, No. 25). The handsets are usually sold across many countries and many regulatory regimes, so they must comply with

[36] Minutes of verbal evidence to the HCSTSC, 16th June 1999

the vast majority of countries' guidelines. However, as I described above when discussing expanding networks, the major incentive to keep SAR down is efficiency. Radiation into the head is a waste of energy that could be used to communicate with base stations at a lower power and so extend battery life. As one representative of a mobile phone manufacturer told me, "We're not in the business of heating heads" (Interview transcript, No. 25). Within the discourse of compliance, a reduction in the SAR value of a handset is defended on the grounds that it makes for a more efficient phone. The relative safety of the technology does not need to be addressed. Industry, once it has demonstrated compliance (and only poorly-engineered technology would approach the guidelines) does not need to consider the science.

An aside on mobile phone shields and hands-free kits

The explanation above can also account for opinions on the growing variety of devices designed to reduce SAR from mobile phones. Such devices include shields which surround the phones, clips for phone antennae, adhesive buttons, filters, crystals and, from Levi's, 'Icon S-Fit' jeans, with built-in radiation-protected pockets (BBC news, 13th September 2002). These gadgets vary in the scientific credibility of their claims, often employing streams of abused scientific terms.[37] But all are sold as individual precautionary measures to dampen concerns (such concerns are often reinforced in their publicity material).

Despite stories of mobile phone manufacturers manufacturing their own shields (which contributed to allegations of a cover-up), the UK industry's view of mobile phone shields follows the same lines as their view of labels. In their evidence to the House of Commons Science and Technology Select Committee, the industry described shields as unnecessary because all phones complied with guidelines. But it also argued that, if any devices were effective in reducing radiation, this would be counter-productive. Either the phone would compensate (using 'adaptive power control' – see above) and reduce its power, or it would operate less effectively.[38]

---

[37] An example: "The Mobile Phone Ray Filter does not remove the microwave radiation, but is raising its vibration, bringing a deep harmony and light-substance into the electromagnetic field, making it less harmful." (From 'Healing Tools & Radiation Protection' http://www.cleanbeauty.org/ukudviklinghealingsredskaber.htm, accessed 29th October 2003).

[38] Minutes of verbal evidence to the HCSTSC , 16th June 1999

It is understandable that industry should be wary of such devices. Their presence implies that a danger exists.[39] But shields also allow non-experts to take a decision that the discourse of compliance decrees that they should not be taking – to attempt to reduce their exposure, despite no change in the official level of 'safety'.

Mobile phone manufacturers have been able to side-step this issue by supporting the use of 'hands-free' kits that, though shown to significantly reduce exposure,[40] can be marketed solely as tools for greater mobility.

The only acceptable grounds for non-expert debate is whether a technology complies with the guidelines set. The problem therefore is framed as one of measurement. Although this raises myriad uncertainties connected with dosimetry, these uncertainties are policed by expertise. Indeed, resolving these uncertainties and accurately determining compliance is vital for the preservation of authority. It falls to the NRPB to police these more problematic aspects of the discourse of compliance.

We have seen how the discourse of compliance represents what is known about mobile phone risks, and what the acceptable form of debate is about these risks. In this sense, it is a way of obscuring uncertainties that might prove problematic (with public (mis-)interpretation) in the provision of authoritative advice or the selling of mobile phones. However, we have also seen how uncertainties are explicitly addressed as a need for further research. When the NRPB's director, Roger Clarke claimed to the House of Commons Select Committee that "all marketed telephones meet our exposure guidelines and as such *there is no need for any further consideration*", he means that there is no need for consideration by anybody outside the citadels of expertise. The uncertainties that exist should not affect the policy implications of the guidelines in any way.

Guidelines and Uncertainty

As noted in chapter two, constructivist studies of science demonstrate that uncertainty is not problematic in itself. It becomes a problem when someone decides

[39] A point made by Alasdair Philips in his evidence to the IEGMP, 21st January 2000

[40] A study by the consumers' association claimed that some hands-free kits increased SAR threefold (Consumers' association 2000). The Department of Trade and Industry responded with their own, less widely-reported study, claiming that this increase was an artefact of bad experimental technique (DTI 2000).

that it has problematic implications (Wynne 1987, p. 95). Scientists, when dealing with the demands of policy are likely to represent uncertainties as manageable and soluble through further research (Shackley and Wynne 1997) in an attempt to maintain social control. Uncertainty can never be eradicated, so it must be presented as an ongoing struggle. A letter from the NRPB to an activist who is concerned about base stations is typical:

> "The current position of the NRPB is that compliance with the recommended basic restrictions will prevent any adverse effect on human health due to exposure to electromagnetic fields. I cannot however give you an absolute categorical statement that non-thermal effects pose absolutely no health risks to nearby residents."[41]

Such a statement, implies that small uncertainties will always exist, and are essentially insoluble, and is common of public summaries of the state of the science. A similar sentiment is expressed by David Brown, representing Motorola at the Science and Technology Select Committee:

> "I would of course always fall back to the basic scientific fact that you cannot prove a negative and therefore it is always never going to be a perfect understanding, but beyond that point, no, any reasonable scientist would conclude that the science is essentially complete."[42]

What contingencies are noted are often considered unimportant and essentially insoluble, as above, with an emphasis that 'science can't prove a negative'. The consensus is however represented by orthodox science as strong, supported by a weight of well-understood scientific evidence.

The uncertainties that are described by experts, whether they are constructed as the filling-in of details or the unachievable horizon on the journey to knowledge, might be reasonably seen as having a bearing on the guideline levels for safety. I asked one interviewee, a member of AGNIR, to consider the implications of uncertainties that have always existed about the (non-thermal) effects of EMF radiation. He explained why the setting of a definitive level needs to be based on certified knowledge:

---

[41] Letter from John Stather, deputy director of the NRPB, to Margaret Dean, Northern Ireland Families Against Telecommunications Transmitters, 2nd September 1998

[42] Minutes of verbal evidence to the HCSTSC, 16th June1999

> "Above this level is unsafe, below this level is safe. How can you do that on the basis of the literature that exists for non-thermal effects? And secondly, what is the evidence that any of these non-thermal effects might pose a risk to health? The answer is virtually none. So they [the NRPB] knew about them, but they didn't fit into the model to determine finite, numerical standards. Particularly since they didn't seem to have any risk to health." (Interview transcript, No. 6)

Uncertainty about the possibility of non-thermal dangers, though appreciated, is not taken into account when guidelines are established (indeed it does not 'fit into the model' used by the NRPB). However, as I noted above, the NRPB plays a dual advisory and research role.

The NRPB contains some of the leading bioelectromagnetics scientists in the world, carrying out lab-based animal studies, cellular studies and dosimetric work. These scientists maintain a proximity to the research face that forces appreciation of the uncertainties of such work, as we would expect from the insights of some of the authors discussed in chapter two (e.g. MacKenzie 1990, Collins 1987). One NRPB research scientist embraced the challenges of scientific uncertainty in his research:

> Q: "What don't we know at the moment?"
>
> A: "So much it's unbelievable. You say, 'if you don't know so much how can you be certain about anything?' We can't, life is never certain... But there are so many possibilities that could be explored. I mean, in terms of biological effects we haven't even started. Is there a cumulative dose, we say,... what is the exposure metric?... Haven't got the faintest idea. What else? Is it that it's exposure above a certain threshold? Well, we don't know. Is it some sort of transient exposure? Don't know. Is there a critical time in the day, in a year, in a lifetime? Don't know. Are men more sensitive, more women sensitive? Don't know. Are children more sensitive?...Do we have evidence that suggests that? Well, I don't think we do, I don't know of many studies, or any studies, using children." (Interview transcript, No. 32)

(The mention of children was prompted by the identification of the potential vulnerability of children by the Stewart report (illuminated in the next chapter)). This scientist appreciates the messiness of establishing scientific relationships, but the uncertainty is couched entirely in expert terms, as a need for research rather than as a prompt to policies which might explicitly acknowledge it.. Wearing the NRPB's other hat of expert reviewers of scientific evidence, his (qualified) certainty about the overall safety of RF radiation is clear. Having discussed uncertainty, I asked what we *did* know about mobile phone health effects:

> A: What do we know? They're unlikely to cause... anything really damaging to health, I think, because... all the previous work that's been done with radars and microwave oven-type signals, suggests that heating is the only phenomenon, the only interaction mechanism. If we get heating, we get effects. With base stations, we get no heating, there's just no energy coming through. With handsets we get minor heating... Looking through the other expert reviews, that seems to be generally a consensus. We can't rule out some probabilities. We can't rule out slightly... increased rates of cancer, but we can't for so many things. But at the moment, I don't think it seems likely... I don't see where there's a consistent story coming through. (Interview transcript, No. 32)

This view seems to reflect the literature that emerges from the NRPB as scientific advice, although the interviewee expresses himself less prosaically. There are myriad uncertainties in the science (which the NRPB considers itself best placed to assess) but these uncertainties are not sufficiently worrisome (at the moment) to change the 'safe' levels for RF radiation exposure. When placed against the weight of evidence suggesting safety, uncertainties become less significant. The uncertainties identified as most salient by NRPB scientists are around the quality of measuring exposure. So while uncertainties are made explicit in scientific advice, the overall message again is "if it doesn't heat you, it doesn't harm you". Uncertainty and ignorance get lost in translation to robust advice because they do not pass the test of relevance.

The gulf between the constructions of uncertainty above and seeing areas of ignorance as policy-critical is vast. The treatment of uncertainty in attempts to construct robust, authoritative guidelines reveals the potential pitfalls of picking apart science and politics in regulation. Uncertainty at anything other than a laboratory level is constructed as 'political', because it does not fit into the scientific discourse of 'compliance means safety'. Two examples below, in which NRPB guidelines face up to foreign scientific advice, illustrate both the unavoidably political nature of scientific advice and the regulatory bodies' attempts to maintain scientific authority by demarcating areas of politics.

Safety Factors in the Provision of Neutral Advice

I mentioned above that the NRPB SAR restrictions include a safety factor of ten, which is seen as conservative (but certainly not *precautionary*). However, an important divergence between the NRPB and other bodies who set guidelines has been the use of *additional* safety factors. The ICNIRP guidelines markedly differ from the NRPB (1993) guidelines in one aspect. ICNIRP recommend an additional safety factor of five for exposure to the general public (rather than those who are occupationally exposed), allowing for the possibility (and implicit uncertainty) of more sensitive subgroups of the population (there is more on this in chapters 5 and 6). The ICNIRP guideline SAR level for whole-body exposure is therefore 0.08 watts per kilogram rather than the NRPB's 0.4[43] An AGNIR member explained:

> "ICNIRP said, and I think quite reasonably, and keeping in mind the health risk, said OK, members of the general public include groups that might be particularly at risk from the same hazards as occupational workers, but they might be less physiologically able to deal with them, namely the elderly, the very young, the infirm, those are the principal categories, interestingly without any scientific evidence for how to arrive at an appropriate quantitative measure of that extra risk, so what they did was introduce an arbitrary difference. They simply said "OK, since the general public includes these extra groups that might be more at risk, let's, say, reduce the limit by a factor of 5," which they did... NRPB didn't like that kind of approach... NRPB's style was to say "look, we're in the business of setting limits that can be defended rationally." The numbers can actually be linked to some scientific evidence... the NRPB has published a different limit for situations in which children are exposed, do you know about this? It's about a factor of two. But it's based not just on hand-waving, on saying 'oh yes, well children are probably more at risk, let's just make it a factor of two.' It's actually based on firm modelling and evidence." (Interview transcript, No. 6)[44]

---

[43] The Institute of Electrical and Electronics Engineers in the USA proposed to remove a similar five-fold safety factor because they could not justify its existence on "socio-political considerations" (Eleanor Adair, quoted in Microwave News, Sept/Oct, 2001).

[44] NRPB 1993 does have a distinction between adults and 'children present' exposure (relates to the resonance frequencies of small people), but none for public/occupational. NRPB document is a guide, but conformity to it will satisfy the normal requirements of the 'health and safety at work' legislation.

Debates over safety factors shed light on the political facet to the NRPB's actions in setting their guidelines. The NRPB feel their authority is best maintained with a clear separation, which recurs in most of the evidence submitted by the NRPB to various committees, between science-based and political decisions (which include any kind of precautionary measures). While rational decisions can still be 'conservative' in their degree of protection, to take account of uncertainties in the science is seen as unscientific. Attempts to maintain the false dichotomy between science and politics, as we shall see in this chapter's conclusion, are at the root of many of the troubles experienced by the NRPB. It is indicative of an identity crisis which similar organisations must experience when their responsibility becomes a public controversy. Part of this identity crisis is a confusion about the role of an advisory body contributing to decision-making.

The NRPB are responsible to the Department of Health, who they see as the relevant decision-making body. A document published by the board of the NRPB, to which the NRPB's director Roger Clarke referred in evidence given to the House of Commons committee, is illustrative:

> "For members of the public, ICNIRP has generally included a reduction factor of up to five in setting basic restrictions across the frequency range to 300GHz. There is, however, a lack of scientific evidence to support the introduction of these additional reduction factors. The Board believes that the existing UK advice by NRPB on limiting exposures for the general public already provides sufficient protection from direct and indirect effects and that any health benefits to be obtained from further reductions in exposure have not been demonstrated. It sees no scientific justification, therefore, for altering the advice previously given by NRPB on exposure guidelines for members of the public. *It does, however, accept that other factors may need to be taken into account by government in establishing generally accepted exposure guidelines for the public.*" (NRPB 1999, my emphasis)

The last sentence indicates that the NRPB sees its role as scientific. An NRPB report on an EMF survey reveals their position: "The guidelines represent scientific advice, however it is for policy makers to determine whether and how any particular set of guidelines should be adopted."[45]. The political responsibility for guidelines is passed

---

45 Radiofrequency Electromagnetic Fields in the Cookridge Area of Leeds, NRPB-W23, September 2002

away, to protect the integrity of the scientific consensus. The guidelines are not regulatory as they emerge from the NRPB. The NRPB's message, in effect, stops at 'compliance means safety'. Framing the role of the NRPB in terms of risk, Roger Clarke, explained that…

> "… the Government has made it clear that it expects advice to be based on scientific evidence and that the decision on whether to take other factors into account rests with Government. This effectively restricts advice on non-ionising radiation to risk assessment and excludes risk management. This is not the case with ionising radiation, where NRPB has been actively involved with risk management. Here the evidence on health effects and dose-response relationships is clearer."[46]

The problems of policy decisions (risk management) are tied in this comment to the uncertainty or novelty of non-ionising radiation risk assessment. The uncertainties of the science of EMF health effects mean that only some aspects of science can be represented as guidelines that can be defended as science-based. We begin to see how, as predicted by a host of authors (e.g. Jasanoff 1990, Irwin et al 1997), scientific advice institutions attempt to separate the domains of science and politics, and often come unstuck. The assumption on the part of regulatory or advisory agencies that 'science-based' means apolitical is easily exposed as false. In the next section, I will expand on this point through a discussion of the dynamics of attempts to universalise standards.

International Harmonisation

An important feature of any regulatory consensus that falls under the public gaze is variation between countries. Guidelines and scientific knowledge are mutually supportive and representative. So local variation between guidelines will therefore act to reduce the authority of scientific advice that is claimed to rest on universal, certified knowledge. Seminars, conferences and ongoing programmes, often overseen by the World Health Organisation, have been devoted to the harmonisation of standards across the world, which means, in practice, the adoption of ICNIRP guidelines. In the light of the challenges to the authority of guidelines described above, advisory bodies and industry see harmonised standards as an

[46] Summary of oral evidence to IEGMP, Roger Clarke, Director of the NRPB

important representation of a general scientific consensus. David Brown from Motorola demonstrated this in his evidence to the House of Commons:

> "...the NRPB, ICNIRP, the International Commission on Non-Ionising Radiation Protection, the European Commission Expert Group, WTR in the US, the Royal Society of Canada, have all reviewed all the evidence and concluded that there are no grounds for concern. Perhaps the Committee would be interested to know that all of the bodies, all around the world, who have established these standards, have arrived at what I consider for all the cultural diversity, an amazingly consistent conclusion. All those standards for whole body exposure, for workers are all set at 0.4 W/kg. That agreement suggests a massive consensus worldwide."[47]

One scientist, offering a response to the findings of this committee, said that such similarities were not an indicator of a firm consensus. Inversely, the pattern of similarities that appeared was the direct product of attempts to move countries into line:

> "Members of the committee expressed their amazement about the unusual unanimity of many scientific boards all over the world concerning their limit values for high frequency electromagnetic fields. The reason for this uniformity lies in the activities of IRPA [International Radiation Protection Association] and ICNIRP to harmonise internationally all guidelines concerning EMF exposures and it does not reflect unanimity concerning scientific evidence."[48]

Indeed, for many years the countries of Eastern Europe and the former Soviet Union had suggested very different standards. An article by Don Maisch, an Australian activist, describes a meeting in Moscow in 1999. The Russian National Committee on Non-Ionizing Radiation Protection met with ICNIRP to discuss 'harmonisation' of standards. The Russian body made it clear that non-thermal effects, subjective symptoms experienced by users (see chapters 5 and 6) and possible cumulative effects should be taken into account, while ICNIRP insisted that the only effects from which we could draw conclusions were thermal. The difference in perspective led Russia and other Eastern European countries to set guidelines for

---

[47] David Brown, Minutes of evidence to the HCSTSC, 16th June 1999

[48] Professor M Kundi – response statement, House of Commons Science and Technology Select Committee Report, Scientific Advisory System, Mobile Phones and Health, submitted as evidence to the IEGMP by Alan Meyer

some RF exposures up to 100 times lower than the UK (Microwave News, Nov/Dec, 1999). Many of the influential studies of non-thermal effects originated in Eastern Europe (e.g. Szmigielski et al 1982) and, as some interviewees told me, possible harm from non-thermal effects has always been treated more seriously than in the West.

Disagreements between countries are useful demonstrations of the insight that regulatory standards can never just represent science (Gillespie et al 1979). There is no one true nexus from scientific evidence to robust guidelines.[49] Rather, guidelines are a construction of regulatory philosophy and a body of research that is considered 'scientific'. Although few advisory bodies would deny the existence of a strong consensus behind the known thermal effects of non-ionising radiation, there is global *political* uncertainty as to the scale and relevance of the *scientific* uncertainty revealed by reports of non-thermal effects. (In international terms, however, there is the added disagreement about the quality of evidence from other countries. EMF research during the Cold War was entangled in its possible military applications, so many scientific results did not escape their countries of origin for decades[50]).

Despite these implicit challenges to the political authority of advice that is seen as purely a representation of science, the NRPB maintained its 'discourse of compliance', aiming to control both scientific uncertainties and the shape of public engagement. The discourse protects the consensus around thermal effects by focussing on what is known. Known effects are incorporated into guidelines, which can be defended on the grounds that they are 'science-based' and therefore apolitical. Uncertainties are acknowledged at a research level but removed from a quantified form of scientific advice. The discourse of compliance gives the impression that science suggests a single, correct level of safety. Uncertainties about the correctness or relevance of this level do not, as one scientist quoted above said,

---

[49] It does not take a sociologist to make this point. An editorial in Microwave News explained, when reviewing various negotiations of standards: "Setting standards is as much a social as a scientific exercise. It is about dealing with uncertainty and deciding on acceptable levels of risk." (MWN, Sept/Oct 2001, p.19)

[50] On the Internet, the military applications of EMF technologies have been greeted with fevered interest. Stories of electromagnetic danger are often contextualised with reference to the bombardment of the American Embassy in Moscow with microwaves in the 1960s (Brodeur 1989). Other Internet activists have concerned themselves with Governmental 'mind control'.

'fit into the model'. They are therefore constructed as political, the stuff of decisions best taken by others.

The discourse of compliance (as with any discourse in the Foucauldian sense[51]) has an agenda for reasonable debate set by those in authority, and groups who wish to contribute are controlled by this agenda. The public are disenfranchised from not only questioning the guidelines, but also from suggesting the appearance of effects from technologies that easily comply with these guidelines. The decision as to public acceptability is taken on behalf of the public. Public enquiries as to the safety of a base station or a handset are reframed as a request for certification as to compliance. The enquiry is therefore cast in expert terms. The discourse of compliance allows the NRPB to manage the issue as a scientific one, with a single answer. Problems arise when groups outside the scientific orthodoxy begin to unravel the politics inside the discourse as well as outside. This is a feature of the chapters 5 and 6.

I was reminded by one interviewee that a discourse of compliance does not necessarily emerge as a regulatory offshoot from a conviction of the safety of mobile phones. This scientist believed mobile phones were utterly safe, but that the NRPB's tactics were not constructive:

> A: Yeah, well this is the Government, the industry and the NRPB's agreed way to deal with the health issue. It's just to talk about the limits. And if you say 'gee, I think I'm having epileptic seizures… is it that bloody radio-antenna?', they'll say, 'you know, my dear, we're operating below the guideline limits, here's all the information, here, here, here and here,'…
>
> Q: So how do you address their concerns without undermining your own guidelines, which you seemingly said are sufficient as they stand?
>
> A: Well, that's a very good question, but you have to take into your head that in the last ten years, most of the research has been on the guideline limits, and the industry

---

[51] Philip (1990), summarising the key contributions of Foucault, states that 'discourse' in the Foucauldian sense, is "…best understood as a system for the possibility of knowledge." (p. 68) A discourse is based on rules, but these are not consciously followed. They operate "'behind the backs' of the speakers of a discourse" (ibid.). For my purposes, the word 'discourse' is a useful way of encapsulating a social trend of behaviour and rhetoric that illuminates some important underlying features. For Foucault's original thoughts, see Foucault (1972) Chapter 1, http://www.marxists.org/reference/subject/philosophy/works/fr/foucault.htm

> and government are paying for this research and they're not discussing it with the public… as a result, the people like the activists who come in and tell them "yes, we know this is a worry and we're going to tell you you're gonna get cancer problems and cognitive effects and all this kind of thing."… The NRPB and the industry have all agreed not to talk about it, so the people are left with a vacuum… And then you go and talk to somebody who you think might know a little more than you do but isn't quite the established source, and they tell you all the other things, you begin to believe them, and that's actually what happened with the Stewart group. (Interview transcript, No. 27)

This scientist feels that discussing the issue as one of compliance leads people towards alternative explanations. The science behind the guidelines, rather than being shrouded in this discourse, should be allowed to speak for itself. The weight of evidence should be allowed to tell its story, which is that there has been no evidence of harm from mobile phones that passes the normal tests of scientific certification.

In most cases, the evidence is not able to tell its own story. A degree of expert digestion is required. But the points made by this interviewee are interesting. By subscribing to a discourse based solely on assessing compliance, those in authority are not answering the questions they are being asked. Engaged members of the public are therefore likely to look elsewhere for their advice.

With the onset of public controversy, the discourse of compliance is exposed as an arrogant, unwieldy and ultimately unsustainable style of public communication. Not only can the politics of its operation be laid bare, but the credibility of the weighty scientific consensus that forms its basis can be dramatically reduced. As predicted by constructivist studies of science-in-public, the rhetorical removal of political elements of a discourse will only create new sites for dispute (see Barry 2001, ch. 9). As Shackley and Wynne (1996) have noted, reflecting on scientific uncertainty can give science and policy (and the public) a common discourse. However, the discourse of compliance obscures uncertainty, preventing engagement on anything other than expert terms.

Although I may seem to be placing the blame for subsequent credibility troubles at the door of the NRPB, who tended to support the discourse of compliance, it is illustrative to consider one passage from the minutes on a meeting, held to discuss international harmonisation:

> "Dr McKinlay pointed out that it was important for standards to [say] clearly about what health effects they provide protection [from]. All too often authorities hide behind standards to avoid answering the real concerns held by people."[52]

The man who made this point was Alastair MacKinlay, head of the non-ionising radiation division at the NRPB and chairman of ICNIRP. It indicates that agencies such as the NRPB do appreciate that compliance with guidelines should not be seen as the beginning and end of discussion about the possible health effects of mobile phones. However, they are caught in a situation that they are not well-equipped to control.

Conclusion – A Crisis of Authority

> "Required to implement the endlessly conflicting dictats of the first three branches of government – the legislature, the executive and the judiciary – and encircled yet further by competing interest groups bristling with lawyers and scientific experts, these… agencies writhe in a chronic crisis of authority" (Wynne 1992b, p. 745)

The quote above is taken from Brian Wynne's review of the most thorough constructivist study of scientific advice, Sheila Jasanoff's 'The Fifth Branch'. Jasanoff discusses the "crisis of authority" experienced by US agencies such as the Environmental Protection Agency, operating in a regime which traditionally 'deconstructs' regulatory science in public to a greater degree than in Europe. However, the picture that emerges from my research into a UK experience is similar.

We have seen in this chapter how a consensus, based on a well-established heating effect, can be constructed and formalised in a set of guidelines. These guidelines, set by the NPRB in the UK, are considered to be scientific advice, rather than regulatory standards. The political responsibility is passed to government. The NRPB, through its guidelines and advice is claiming only what conditions are considered safe by the weight of scientific evidence. But the guidelines and the consensus behind them become mutually representative and reinforcing. The

---

[52] Minutes of International EMF Project: Standards Harmonization Meeting, Ettore Majorana Centre, Erice, Sicily, Italy, 27th November 1999

balance of scientific evidence is obscured from view in a representation of what is *known*, and the guidelines act as an 'anchor' (Van der Sluijs et al 1998), one stage removed, preventing undue movements in the representation of scientific uncertainty.

Challenges to these guidelines or this consensus produce a defensive reaction which illustrates a prevailing attitude amongst scientific and regulatory orthodoxy that compliance with thermally-based guidelines promises safety. This discourse of compliance sets the bounds for reasonable debate, preventing non-scientific bodies (including industry) from questioning the framing of the guidelines. Uncertainties remain an expert resource, with the implication that absence of (robust) evidence is equivalent to evidence of absence of harm.

However, we have begun to see in this chapter how a discourse based on 'compliance means safety', can be challenged in a way that is simultaneously scientific and political, questioning both the scientific consensus and the correct way to extrapolate states of knowledge to guidance about dose levels. In the public arena, boxes containing framing assumptions are re-opened, new questions are asked: What technologies were the guidelines intended to cover? What are the grounds for scientific consensus? And what direction should research take? The door between risk assessment and risk management, which the public had previously seen from the outside in the form of SAR guidelines, is prized open through broader engagement around issues of policy and scientific uncertainty. International disagreement on the state of the science and the 'correct' restriction levels of exposure highlights the limits of the applicability of 'universal' knowledge and the pressure which 'neutral' knowledge is placed under when demands are made for 'correct' policies. Claiming to be '*science-based*' is not sufficient to preserve authority when external observers are asking '*what science?*'

As explained in this chapter, uncertainty has always existed around the consensus that thermal effects are the only harmful effects. At a laboratory level, such uncertainties have always been understood (indeed they are the lifeblood of productive research). In the reviews of the scientific evidence, such as that done by AGNIR, experts have taken care to acknowledge gaps in our knowledge and the incompleteness of *current* scientific knowledge. But such contingencies get lost within the advisory system. It is assumed that they are meaningless outside an expert context. This perception that uncertainty is solely an expert consideration is a part of, and part of the downfall of, the discourse of compliance.

We have seen how scientific uncertainty expands from, and with, public concern about a rapidly-growing technology. To return to the review quoted above, Wynne reiterates the point (borne from constructivist studies of science) that scientific uncertainty should be seen as a product of the deconstruction of public science that comes with public examination, rather than as a cause of the initial social unease (Wynne 1992b). The narrative of this chapter has extended this view of uncertainty. Uncertainties which might concern experts, such as the ability to accurately measure doses, or the presence of niggling non-thermal effects, can be expanded and picked apart until advisory guidelines and a scientific consensus appear unstable. The process of picking apart uncertainty as a controversy moves from an expert to a public context, forces those being challenged to "retreat to stronger bastions of claimed social 'agreement' or 'black-boxing'" (ibid., p. 751). In the case of the discourse of compliance, challenges to the scientific consensus tended to elicit the response that uncertainties would always remain, that 'science can't prove a negative' and that, essentially, there was nothing to worry about.

The more a consensus or a regulatory philosophy is challenged politically, the greater the temptation for regulatory science to harden it as an apolitical representation of a science-based consensus. However, the authority that should come from being 'science-based' is fragile. Organisations such as the NRPB, who consider that their role is solely to reflect the best available science, find themselves unwittingly representing a political position. People who approached the NRPB and similar bodies with worries about the protection afforded them by current safety guidelines were greeted with the discourse of compliance, which failed to answer, or even acknowledge, the questions they were asking. The NRPB and the standards themselves, originally developed as a workable representation of the only established effects of microwave radiation, became an inevitable target for public challenge.

Assumptions behind the guidelines were exposed and became the source of public disenchantment with the protection offered by the guidelines. To repeat one example, SAR provides a metric for the assessment of hazard from an acute, thermal exposure. After the six minutes required to reach a thermal equilibrium, the time exposed to radiation is not considered important. Many of the claims of harm came from members of the public living near base stations. The highest base station exposure is typically thousands of times beneath the NRPB's investigation levels and it would never induce a heating effect. But many of the concerned public considered that long-term (chronic) exposure was causing them harm, and would not be reassured by assurances of compliance with a metric based on acute exposures. For these people, the discourse of compliance was not answering their questions, prompting them to engage with alternative networks (interest groups, unorthodox scientists and news media).

In the vocal battles between engaged members of the public and the Government, the NRPB was the usual first call for advice. Subsequently, however, the NRPB came to embody the lack of protection people felt they were being given from mobile phones and base stations. The NRPB was perceived as aloof, expert and unhelpful. But, as I hope has come across in this chapter, the blame should not just be placed at their door and forgotten. There is a deeper explanation, provided with a brief summary of the narrative from this chapter.

When mobile phones were first introduced to an unworried public in the 1980s, only two groups considered the issue of protection from health effects – industry and regulatory agencies. Both groups had conducted or contracted significant bioelectromagnetics research to build knowledge of the possibility of harm. Guideline levels were drawn up based on known effects of the kind of radiation emitted by mobile phones, independent of mobile phone technology. Over time, efficiencies in engineering assured that mobile phones and base stations could easily comply with these guidelines. Industry was therefore free to declare its independence and not consider the issue further. As public concern began to grow, the NRPB, reassuring members of the public that compliance determined safety, became the only possible target for antipathy. The NRPB, as the provider of convenient regulatory advice, was seen as representing industrial interests. However, NRPB was anchored to its advisory guidelines by its insistence on science-based advice (and let's not forget that this advice also applied to other, less controversial EMF technologies). The NPRB's intention was never to make good (credible) policy. Its only option was to ride on the coat-tails of the more independent authority of an even more 'independent' group (AGNIR). An NRPB representative explained this conflict:

> "We're a public body and we've got to expect a bit of criticism... We're an independent statutory body, but if an organisation says one thing one year, it is hard for it to change its mind...We have these expert groups [such as AGNIR]... it assures the public, or should do, and politicians, that we are impartial... on difficult questions, we get people who are separate from us." (Interview transcript, No.21)

The contribution of AGNIR allowed the NRPB to better assess the available scientific evidence. And it was decided that evidence for thermal effects remained the only evidence robust enough with which to build guidelines. However, as we have seen, the nexus from good science to good policy is questioned by international disagreement on guidelines. The NRPB's 'science-based' attitude included a narrower perspective on what counts as evidence than other agencies. This attitude facilitated authoritative defence of NRPB guidelines at a time of largely expert debate, but it was not credible when broader questions were asked through public engagement.

The credibility of public science is tested by the questions that are asked of it, often by non-experts. As the context surrounding mobile phone regulation changed, the NRPB's advice no longer fitted the types of questions that were being asked. Originally expected by Government to be a source of authority, and of high quality science, the NRPB became a target for criticism, unsure of its role in a broader debate. The NRPB's natural style of advice was *harm-based*, working from the known dangers of ionising radiation or intense microwave heating. It was less well-equipped to deal with the *risk-based* style of regulation (considering the *possibility* of harm) demanded by the mobile phones controversy (see Jasanoff (1995, p. 72) for more on this distinction).

This harm-based, compliance-based style of doing public science was the target of expert and non-expert challenge with the onset of broad public controversy. Faced with new types of enquiry, the NRPB was shown as interested only in certain types of response. The loss of credibility in the NRPB's guidelines, and the science used to defend them, forced a governmental response. In 1999, Tessa Jowell, the minister for public health, formed an independent expert group to reconsider the issue, independent of the furrowed brows of the NRPB. This group, and their approach to the public science of deciding about mobile phone risks, is the subject of the next chapter.

5 'Meeting the Public Halfway'[1] – The Stewart Report

In the previous chapter, we saw how a controversy, once public, can be expanded beyond the control of a previously authoritative advisory body such as the NRPB. By 1999, after front-page news coverage, parliamentary questions and increasing local action against networks' attempts to erect masts, mobile phones had come to be defined by many of the public as 'risky'[2]. Action was required to regain some of the credibility of scientific knowledge and advice about mobile phone radiation. In late summer 1999[3] the Independent Expert Group on Mobile Phones (IEGMP) was formed under the chairmanship of Professor Sir William Stewart, formerly the Government's chief scientific adviser. Their report ('The Stewart Report') redefined the shape of scientific advice on the health effects of mobile phones. This chapter describes the context, operation and conclusions of the IEGMP that led to a broader consideration of the science and tighter social control of the uncertainties behind mobile phone risk.

In 1999, as part of a review of the Governmental scientific advisory system, the House of Commons Select Committee on Science and Technology had published a report on "Mobile Phones and Health", based on testimony from Industry, the NRPB and representatives from the department of health. Though not an expert review in itself, relying on existing constructions of the relevant science, the report set the tone for much of the policy debate that the IEGMP would follow up.[4] Some of the recommendations of the select committee would be directly echoed a year later in the Stewart Report.[5]

---

[1] This phrase is courtesy of Jerry Ravetz.

[2] A survey in June 1999, funded by Techno AO (manufacturers of a device that sticks to the back of a mobile phone and claims to reduce radiation), found that 43% of daily users were particularly concerned about the potential risks from mobile phones (MORI 1999).

[3] One ad hoc meeting on the 26th July, 1st full meeting on the 10th Sept 99

[4] Of the previous international expert reviews which had been prompted by growing public concern, the report from the Royal Society of Canada in March 1999 (RSC 1999) was the most influential, reviewing the evidence for non-thermal effects and recommending further research to clear up uncertainties.

[5] Uncertainties in the science and opposition to the current policies of siting mobile phone base stations had been publicly highlighted in a House of Commons meeting on the 15th June 1999. At

The need for the IEGMP had already been established by the time of the Select Committee's meetings. Tessa Jowell MP, then minister for public health, concerned at the increasing number of letters received by Members of Parliament, asked the chairman of the NRPB's board, Sir Walter Bodmer, about the formation of a group independent from the NRPB. Sir Walter Bodmer had recommended Sir William Stewart, who began to assemble the group on the provisos that he was allowed access to the Government without external involvement and that he could choose the membership. The members of the Independent Expert Group had largely been decided by July 1999. The group's remit was:

> "To consider present concerns about the possible health effects from the use of mobile phones, base stations and transmitters, to conduct a rigorous assessment of existing research and to give advice based on the present state of knowledge. To make recommendations of further work that should be carried out to improve the basis for sound advice" (IEGMP 2000, p. 11)

There are two clear distinctions between these terms of reference and the remit of the NRPB, previously given sole responsibility in this area. First, the IEGMP was told only to consider mobile phones, setting aside other technologies which were regulated under the weight of the same scientific knowledge. Secondly, the IEGMP was given the prescription to 'consider present concerns'. Whereas the NRPB had perceived that its stability in the face of public concern would benefit the authority of its advice, the IEGMP, through the augmented remit, and its subsequent interpretation, was placed at the heart of the debate.

Independent from Whom, Expert at What?

The emphasis in the membership of the independent expert group was on *independence.* It was deemed that independence would breed authority and go some way to solving the problems (described in the previous chapter) that had been encountered by the NPRB. The committee therefore distanced itself from the

---

this meeting, initiated by Phil Willis MP, a pressure group, Northern Ireland Families Against Telecoms Towers (NIFATT) had spoken about the need to properly regulate new base stations, and the NRPB, represented by Zenon Sienkiewicz and Sir Richard Doll, had responded with their take on the state of the science, saying that claims of harm were unsupported by evidence, but that further research needed to be done. (See "High anxiety", The Guardian, October 20th, 1999)

NRPB, from industry and from the Government. During the IEGMP's period of work, the Government was in the process of auctioning five new licences to use frequencies for the operation of third generation (3G) mobile phone networks. Both industry and Government (via the Department of Trade and Industry) had clear vested interests in an expert report suggesting that the mobile phone health scare was unwarranted.

For the reasons described in the previous chapter, the NRPB was perceived by some critics as supportive of a status quo and of an unfounded set of guidelines which had industrial backing. Some commentators had also linked the NRPB's interests to the auction of third generation licences, arguing that a share of the Government's windfall would be given to the NRPB to boost its research presence.[6] Although the NRPB would contribute the secretariat to the group, it was deemed that references to the NRPB or to Sir Walter Bodmer should be removed from public material emerging from the group.[7] Professor Alan Baddeley, an NRPB board-member leading a subgroup looking at how the NRPB should communicate with the public, described the malaise to Sir William Stewart:

> "One issue that keeps cropping up is the extent to which the public perceives NRPB as being independent... In actual fact I think they do an excellent job, and are not driven by a desire to please government, but it is clear from some of the questions raised by the public, that there are some doubts. I think the creating of the independent expert group reflects this feeling. Its creation does, however, call into question the role of the NRPB in advising the Government and the general public."[8]

At the public meetings, as one IEGMP member told me, "the word 'independent' kept being repeated." (Interview transcript, No. 19). Although the IEGMP was set up by the NRPB, the expert group set out to distance its work from that of the NRPB, so that the report could be seen as an unburdened attempt to consider the issue from a credible perspective. The IEGMP recognised, however, that two of the groups they claimed independence from (the NRPB and the mobile phone industry) were repositories of much of the research and expertise that was required to present

[6] "Mobiles to carry health alert", Kamal Ahmed, The Observer, Sunday April 30, 2000

[7] Letter from Sir William Stewart to John Stather, Assistant director of the NRPB (PRO HP4, file 7)

[8] Letter from Professor AD Baddeley FRS to Sir William Stewart, 10th February 2000 (PRO HP4, file 14)

a scientific review. Not wishing to undermine the *scientific* work of the NRPB, an NRPB secretariat was employed to help survey the science (although this secretariat was dismissed when the group began to consider the NRPB's role in the growing public disenchantment with scientific advice).

The IEGMP contained two members of AGNIR, Anthony Swerdlow, an epidemiologist, and Colin Blakemore, an esteemed physiologist. These members had significant experience of the issue but were considered sufficiently independent from the workings of the NRPB to have no vested interests (indeed, AGNIR was considered an *independent* group reporting to an *independent* body). Two other members of the group had worked directly in the area of mobile phone research: Michael Repacholi, the lead researcher in the rat brain cancer study mentioned in the previous chapter, and Les Barclay, a radio-communications engineer. Most of the remaining members, plucked from the advisory great and good, arrived at the issue with experience only of scientific reviews and advice in other public health issues. Two lay members were brought in (in accordance with a recommendation from the House of Commons Select Committee), to represent not only non-experts but also women and young people. Despite early discussions about including non-scientific experts such as social scientists, philosophers or lawyers in the group, the experts in the final committee were all scientific. Sally Macintyre, a medical sociologist who the chairman was initially keen to have on the committee, withdrew before the first meeting of the group.

The work of the IEGMP was based around 10 meetings, 5 of which were adjoined to public meetings (in Edinburgh, Liverpool, Cardiff, London and Belfast). The structure of the report was settled early on by the 4th meeting (the start of 2000), with sections allocated to members of the committee. The lay members were given responsibility for issues relating to 'public perceptions and concerns', the evidence for which had been gathered from the slew of media reports in the previous year,[9] written evidence submitted to the committee, verbal evidence from invited interest group representatives and evidence from the public meetings.

Of the public meetings, the Belfast meeting was the best-attended, courtesy of NIFATT (Northern Ireland Families Against Telecoms Towers), then the most

[9] 641 press cuttings, 76 TV and radio broadcasts (IEGMP 2000, p. 21)

powerful anti-base station pressure group, who had advertised the meeting more widely than the NRPB had done at the other locations. At the Belfast meeting, members of the public complained about the ease with which the phone networks could put up masts despite strong public objection. At the meeting in Cardiff, it was explicitly suggested that the NRPB had lost credibility and that this was in part because of industry and planning authorities 'hiding behind' the guidelines (see previous chapter). Uncertainties were expanded into a worrying sign of ignorance, or evidence of a cover-up, and suggestive studies were put forward as evidence of danger. At each public meeting, Sir William Stewart made clear that the group were interested in hearing objections to the current situation and reports of health problems (which came to be understood as 'anecdotal evidence' – see chapter six).

Although the attendees of the public meetings were in no way representative of a general public unease (a point reiterated by some group members who were eager not to pander to non-expert demands), the meetings became the IEGMP's conduit to 'public concern'. Some of the group's experts were looking at this particular science for the first time. Their investigation, originally prompted by 'public concern', was thus informed by a number of public issues that were not easily separated from the uncertainties that had been previously been constructed within the domain of expertise.

Uncertainty in the Cold Light of Public Controversy

> "It was simply when we started analysing the results that the uncertainty became more apparent." (Interview transcript, No. 30) – An IEGMP member

The IEGMP, looking afresh at the available scientific (and some non-scientific) evidence, saw a body of knowledge in the light of public concern and distrust. The construction of uncertainty that emerged from their analysis was flavoured by their broad remit and an appreciation of the political stakes of scientific advice. This section considers how a flexible feature such as scientific uncertainty can be reframed or rebuilt to present a subtly different picture of the adequacy of current knowledge and policy in dealing with risk. Previously, proponents of the sufficiency of a thermally-based set of guidelines and concurrent reassurances had argued that there was a wealth of knowledge which largely exonerated mobile phones (I was told by some scientists of the thousands of scientific papers which considered the biological effects of radiofrequency EMFs, and the hundreds more that had looked at ELF effects). Other scientists, who were wary of attempts to downplay scientific uncertainties, had suggested that the papers of *direct* relevance numbered in the hundreds and that there were still myriad unanswered questions.

In trying to analyse the degree of uncertainty about such a complicated public health issue, the only conclusion that can safely be drawn is that one person's conviction about safety is another's uncertainty, and this uncertainty might be, to another observer, consistent evidence of danger. Those who had reason to believe that mobile phones carried significant risks, when asked about uncertainty, tended to reply that, rather than the gaps in our knowledge being worrisome, the danger is evidenced by the existing scientific evidence. (Such arguments were often accompanied by a suspicion that scientists detecting non-thermal effects had come under pressure from industry not to disclose their findings). I asked one scientist whether everybody would agree that there is as little relevant science as some have suggested about the health effects of mobile phones:

> "No, there's an abundance of evidence... Even since the advent of mobile telephony, there's been a vast amount of research which has been done, often financed by the phone companies. On the other hand they don't always want to believe what's found and they persuade people to change their results. Have you come across that?" (Interview transcript, No. 9)

Scientific critics and activists unearthed 'definitive' studies (often from Eastern Europe or Russia, where regulatory disagreement was most marked (see chapter four)), which demonstrated harm from EMF radiation and presented them to groups such as the IEGMP. Such evidence was used to support the case for danger, but also the case for uncertainty or ignorance – highlighting how little the UK establishment knew, or what they were trying to hide.

The IEGMP, approaching the science largely as outsiders, reviewed 430 scientific papers and concluded that there were deficiencies both in scientific knowledge and in the understanding of the relevance of research in setting standards. One group member described to me the feelings of the group while they were considering the issue during meetings:

> "I certainly saw some committee members sort of changing their mind, you know, several times during the thing. And the thing that we all kept repeating is 'we haven't had enough time, there isn't enough evidence,'... people were saying 'we just haven't got enough time, we need more research, we need, you know, in 5, 10, whatever years time,' so people certainly never thought that this is it, done it, tick the box and that's it. In fact, if anything, all of us, after each meeting, realised the scale of the need for the research." (Interview transcript, No. 19)

So how did the IEGMP unearth sufficient scientific uncertainty to justify a precautionary stance that was markedly different from the approach of the NRPB, when previously the body of scientific knowledge had been considered adequate? Partly, there was the evidence that had arisen from the recent studies reported in the previous chapter. These studies, by Preece, de Pomerai, Repacholi and others, had strengthened the case for reliable non-thermal effects. In particular, the evidence from Preece's study – the first to suggest a direct effect on humans – was influential in the IEGMP's conclusions. But the explanation that new evidence prompts fresh consideration is not sufficient when we consider that reports of non-thermal effects had existed as long as the technologies on which they had been blamed. A broadening of the issue beyond expert attention reframed the science and its relevance to the issue. Engagement with non-experts altered the approach to the available evidence and its attendant uncertainties. One IEGMP member, on considering the relationship between uncertainty and policy, said this:

> "Yes, I suppose there are two sorts of uncertainty, aren't there, in considering questions of scientific advice to government. One is uncertainty about the quality, meaning or significance of published work, and the other is uncertainty in the sense that the work hasn't been done at all... Both sorts of uncertainties exist about mobile phones, but I would say there's far more uncertainty about the published work" (Interview transcript, No. 6)

The view that the existing published science had been done poorly or was of little relevance to the question of mobile phone safety was reiterated by other IEGMP members. This opinion begins to suggest that the IEGMP reconstructed the state of the relevant scientific knowledge by looking more critically at existing studies. Rather than the arrival of new scientific evidence or the discovery that scientific uncertainty had been previously covered-up, we can see that the reason for reconstructing uncertainty was that different questions were being asked of science, as pointed out in the previous chapter, yielding less well-constructed answers. We therefore begin to see that policy action and decisions about levels of uncertainty become intertwined. The interviewee above continued:

> "we all agreed to take a precautionary approach, and we all agreed that overall there seemed to be sufficient evidence that there could be interactions between radiofrequency radiation and tissues and cells at levels of exposure that should not cause significant heating. And that kind of alarmed us a little bit. So while none or virtually none of the claims that had been made, even the most extravagant of them would seem to imply a health risk, we said 'hold on, probably best to be cautious'." (Interview transcript, No. 6)

The appraisal of evidence, and subsequent attribution of a level of uncertainty, seems linked to the decision to be cautious from the outset. The precautionary recommendations of the report (discussed below) were seen by most of my interviewees to be an exercise in good risk *management.* One IEGMP member disagreed that the Stewart report's review of the science had been any more or less uncertain than previous reviews such as those of AGNIR:

> "the way in which it was written... precautionary principles and all this sort of stuff... in that sense, the flavour and the slant and the audience is a bit different. I'm not sure that an AGNIR or ICNIRP or whatever document would in any real sense pretend to any greater degree of certainty or any lesser degree of certainty. I don't think in that sense people said 'I don't know' where in another document they would have said 'I do know'. I think they'd have said the same thing. I think it's the wrapping around it." (Interview transcript, No. 5)

In his view, the decision to be precautionary from the outset flavoured only the policy aspects, which had been tacked onto the scientific review for political reasons rather than scientific ones. However, this interviewee views uncertainty in a similar fashion to that described in the previous chapter – as an obstacle on the path to truth, resolvable through further research. The uncertainties that emerged as important in the Stewart report were contrived at a public health policy level, sufficient to justify a re-assessment of mobile phone regulation and of network expansion. The precautionary emphasis framed uncertainty as likely to suggest the need for policy action, rather than just the need for further research. The picture of uncertainty that was created, from subtle shifts in interpretation, emphasis and judgements about relevance, was therefore noticeably different from that produced by previous reviews.

The IEGMP, from the outset, decided not to consider the available science in isolation. AGNIR had published reviews of all but the most recent science. Unpublished scientific work and reports from members of the public were (at least nominally) integrated into the group's deliberations. (Indeed, at one point, according to the report plan found in the group's early minutes, the scientific review was destined to be included in a chapter entitled 'public perceptions and concerns'). The IEGMP received, through their NRPB secretariat, written evidence ranging from existing scientific archives, through reminders to pay careful attention to certain studies, to letters in which people who had suffered the ill effects from mobile phones described their experiences. These letters narrate individual experiences of illness brought on by mobile phone or mobile phone mast radiation exposure. Among the symptoms reported in these letters are halitosis, diarrhoea, memory loss, headaches, sleep loss and, most intriguingly, "Busby Berkley-style nightmares".[10] Such symptoms were similarly reported by members of the public at the IEGMP open meetings. The reporting of these symptoms is what experts might traditionally have rejected as 'anecdotal evidence', and the exploration of this theme is the subject of the next chapter. But the claims made by these non-experts, and the associations they typically drew – to previous scientific advice failures (see below) and to the work of scientists such as Preece and dePomerai, or more campaigning participants such as Coghill and Hyland – were seen by some as influencing the perception of scientific uncertainty. This scientist was one of very few people with strong criticisms of the IEGMP:

> A: The opinion in every country except the United Kingdom is that there's enough science to base the standards on. We still don't know everything. There's still questions being pursued but, you know, there's a fairly broad basis of knowledge to draw on, and the knowledge is that there are no hazardous effects, and even the biological effects are very iffy. They're not really considered established science. But in the UK, for some reason, we had an expert report that was more political...

[10] These symptoms appear in various letters submitted as written evidence to the IEGMP (PRO HP4)

> Q: Could you suggest some reasons why they [the Stewart group] did such things, why that report diverged from the...
>
> A: ...well it was the method of operation, their modus operandi... They held open meetings... but basically they were listening to people who weren't scientists, who were activists, people, you know who didn't understand radiofrequencies, you know, who didn't have any scientific knowledge and who were really complaining about their right to deny the industry base [station] sites and I suppose if you hear that every night, eventually you begin to take the point of view of the people you're listening to. And that's basically what's happened with this expert report" (Interview transcript, No. 27)

The doubts that were expressed by the IEGMP over the completeness of scientific understanding were seen by this interviewee as being politically-motivated, leading to a false representation of scientific understanding. My analysis has shown this sentiment to be half-true. The IEGMP did not just consider, or review, the science. The group reconstructed the science as a policy-relevant body of knowledge, with its attendant shortcomings. So rather than a unilateral expert effort to determine *the* degree of uncertainty, decisions about uncertainty emerged from negotiations in which groups enrolled bodies of knowledge and drew connections, constructing credibility and applicability. Within these negotiations lay decision stakes which are used to justify immediate action, precaution or increased scientific robustness, depending on the stance. Uncertainty is not certified with the same vigour as scientific consensus, so it is prone to a greater degree of interpretative flexibility. And we have seen how the Stewart report's context led it to reconstruct a body of knowledge which, after all, was never intended to be 'mobile phones science'.

The IEGMP operated under, and responded to, public scrutiny. A media briefing provided to the group by the NRPB's scientific spokesperson, Mike Clark, illustrates some aspects of the political landscape in which the IEGMP worked. Clark's advice is to "pull up the drawbridge" until the work of the group is complete, preventing media encroachment (although the news media did get a feel for the report from public meetings). A list of questions that Sir William Stewart might be asked, based on the NRPB's previous involvement in this debate, includes the following: Firstly, "Why isn't Roger Coghill/Dr Hyland/Alasdair Philips on the expert group?" and secondly, "Isn't this just like BSE? Your Expert group will report saying there is no evidence of a risk, only to find out later that some people have been affected."[11] The first question, which many members of the public did indeed ask, illustrates that non-expert constructions of what constitutes relevant, credible expertise can pose real problems for experts who must convince others of their credibility. The second question raises the spectre of a key scientific advice failure that defined much of the work of the IEGMP.

Scientific Advice after BSE

The Stewart report was published as the public inquiry by Lord Phillips and colleagues into the science and policy around the emergence of BSE and vCJD (Phillips et al 2000) was coming to an end (The inquiry report was published in October 2000). This massive piece of work took apart the machinations of the scientific advisory process that had led initially to reassurances that British beef was 'perfectly safe' (since 1988) and subsequently (in 1996) to the admission that BSE could be linked with a variant of Creutzfeldt-Jacob disease in humans (Millstone and Van Zwanenberg 2001, p. 99).

The Phillips inquiry revealed how the Government, through the Ministry of Agriculture, Fisheries and Food (MAFF) had largely ignored important uncertainties about the possibility of BSE crossing a species barrier and causing disease in humans. The government protected its interests by insisting that it was following scientific advice, although MAFF actively sought to influence the conclusions and presentation of the report from Sir Richard Southwood's advisory committee (Millstone and Van Zwanenberg 2001). The general impression was that BSE as a 'risk issue' (cf. Leiss 2001) was mismanaged with awful consequences. The collapse in the authority of governmental scientific advice led to what Jasanoff calls 'civic dislocation', with the UK public looking elsewhere for advice on food safety (Jasanoff 1997, p. 223), establishing individual narratives of risk and trust. At the heart of this disenchantment with expertise was a belief that the Government had disingenuously misled the public, believing that consumers would overreact (at the expense of the UK's agriculture industry) to qualified advice about scientific uncertainty.

[11] Media briefing for the IEGMP from Mike Clark, NRPB

The impact of policy failures over BSE extends far beyond the human deaths from vCJD (however many hundreds this may eventually total)[12] and the harm to the British beef industry. BSE will define exchanges between experts, the public and the UK government for many years. For the controversy over mobile phones, BSE acted as an archetype for easy comparison. It was clear from evidence submitted to the IEGMP that reassurances of safety would mean little in the context of previous mistakes. Critics of the existing style of scientific advice over mobile phones could reasonably argue:

> "A number of other hazards have first been denied, but have subsequently been proved to be real, including radium cream, x-rays during pregnancy, thalidomide, asbestos, smoking and BSE."[13]

Another critic (a physicist) considered the 'discourse of compliance' (identified in the previous chapter) in the light of BSE:

> "I don't know much about that science, but government ministers feeding their children beef burgers, saying, 'totally safe, blah blah blah.' And you could just look at a lot of reports on BSE and simply change the word to 'mobile telephony'. The same mentality, the same mind set prevails." (Interview transcript, No. 9)

In the case of BSE, some scientists had argued from the outset that serious dangers existed, and later been proved wiser than the authorised advisory bodies. So with mobile phones, any scientist offering opinions which differed from the advisory norm were considered by the public to be acting in their interest. In particular, the unorthodox views of Gerard Hyland, gained currency. Hyland's suspicions of the inadequacy of the NRPB's guidelines grows from a concern that, because live human bodies are sensitive EMFs generators, they can be disrupted by external EMFs. According to this theory, the human body will react in an unpredictable, non-linear way to long-term EMF exposure. Hyland is convinced that this theory suggests a mechanism of interaction for the non-thermal effects that have been demonstrated by scientific research and by reported symptoms (see Hyland 2000). (Interestingly, his non-linear explanation also claims to explain why non-thermal

---

[12] At the time of writing, there had been 137 human deaths from vCJD that were connected to BSE (source: The UK Creutzfeldt-Jakob Disease Surveillance Unit, University of Edinburgh)

[13] Alasdair Philips, IEGMP Minutes of oral evidence, 21st January 2000

effects are so difficult to replicate). Newspapers, eager for an 'expert' counter to the official guarantees of safety, used Hyland and the even less-credentialed Roger Coghill as sources for many of their stories.

The IEGMP, once assembled, could not escape the long shadow of BSE. In a public lecture, Colin Blakemore, an IEGMP member who was also a member of AGNIR, explained the wariness of the committee:

> "We decided post-BSE - and this is very much in our minds - we found ourselves three years ago thinking that we were in a position somewhat similar really to that of the members of the Southwood Committee, the committee commissioned by the Government to advise in the 1980s after the first case of BSE, what the potential risks and hazards might be. We found ourselves thinking, 'what if, in ten years time there is a massive increase in the incidence of brain cancer and we haven't spotted it, we might well be blamed in the same way that the members of the Southwood Committee have subsequently been blamed about BSE'...We decided then, following the emerging principle of transparency in dealings between government and particularly government science and the public, we decided to call public meetings around the country, seven of them, I think. We advertised these widely in local newspapers, essentially saying, 'come and tell us about your concerns about mobile telephones', and people came in their hundreds to these meetings."[14]

Blakemore justifies the operation of the committee in terms of the mistakes of BSE. But BSE also influenced the final conclusions of the committee. Sir William Stewart, was asked to explain his conclusions to the House of Commons Trade and Industry Select Committee, which, in 2001, had turned its attention towards the issue of planning policy for mobile phone masts:

> "Firstly, the BSE inquiry impacted upon us. Never again will any scientific committee say that there is no risk..."[15]

BSE did not necessarily induce broad cultural shifts towards appreciating the scale of uncertainties in scientific advice. But it did perhaps remind advisory scientists that their activities are political and that the stakes can be very high. Sir William Stewart's

---

[14] Colin Blakemore, public lecture, "Mad Cows & Mobile Phones" the Andrew Olle Scientific lecture, 14th August 2002, Broadcast on "The Science Show," 30th November 2002, ABC (Australia) (Transcript from www.abc.net.au/rn/science/ss/stories/s736667.htm, accessed 28th July 2003)

[15] Sir William Stewart, Minutes of Evidence to the HCTISC, 13th March 2001

evidence continued the above comment with reference to another historical public health lesson learnt after failure to grapple with uncertainties:

> "... Secondly, looking at the ionising radiation saga over the past 50 years one sees that the acceptable levels for ionising radiation were reduced three times and years later they were reduced another three times and I believe that they have been reduced another three times. Are we at the same stage today as we were with ionising radiation 40 or 50 years ago? I suspect that we are not at the same stage, because, unlike ionising radiation, there is no real evidence that non-ionising radiation can break DNA." [16]

Ionising radiation had once been considered sufficiently safe to allow routine X-rays to confirm pregnancies and check the fit of new shoes[17]. However, the easily-visible effects of prolonged exposures forced the 'safe' level downward during the twentieth century. Effects of exposure to low-level ionising radiation were shown to accumulate over long periods. So for critics of the short-term basis of the NRPB's regulation who were well-versed in history, it was perfectly sensible to worry that effects of our unprecedented levels of microwave exposure would be revealed too late.

These past policy failures and public health misunderstandings exerted their historical weight on the uncertainties that the Stewart report identified. Along with other (pre-)cautionary tales such as the suppression of evidence of harm from smoking, they provided the context of trust in expertise. In this context, reassurances of safety would be essentially worthless without qualification.

We should not forget in considering the impact of BSE that Sir William Stewart was chief scientific advisor to the Conservative Government from 1990 to1995. Although he admitted in his evidence to the Phillips inquiry that that his direct involvement in the BSE saga had been 'negligible',[18] his intimate view of advisory reassurances that later revealed as mistaken must have affected his appreciation of

---

[16] Sir William Stewart, Minutes of Evidence to the HCTISC 13th March 2001

[17] Marie Curie was perhaps the most famous victim of a mistaken impression that "What you can't see can't hurt you". She is thought to have died from leukaemia in 1934 after long-term exposure to the radioactive substances that were the subject of her work.

[18] The BSE Inquiry / Statement No 187, Sir William Stewart, page 4, http://www.bseinquiry.gov.uk/files/ws/s187.pdf accessed 20th August 2003

the politics of expert advice. In interviews with other members of the IEGMP, the tone of the Stewart report was revealed as the product more of its namesake than a *committee*. Although the scientific reviews were a group effort, it emerged during my research that the report's recommendations were largely the interpretations of a man with significant political experience.

Recommendations of the Stewart report

The IEGMP, after a surprisingly short period of work (less than 9 months), published their report on the 11th May 2000 with a press conference at the Royal Academy of Engineering. Governmental leaks to the press, (variously attributed by interviewees to the Department of Trade and Industry and the Department of Health), had led to a crop of reports some weeks before that the IEGMP would authoritatively state that scares over mobile phones were unfounded. In the light of the ongoing auctions of third generation licences (which would eventually total £22.5 billion), some of my interviewees saw these leaks as a move to reassure an uncertain set of bidders. The Guardian newspaper reported on the 28th April 2000 (the day after the completion of the auction) that the expert group report would give mobile phones "a clean bill of health"[19]. This article emphasised that mobile phones did not heat the brain significantly, so the expert consensus would be that they could cause no harm. In the light of the previous chapter, it is clear that this story misrepresents the controversy that emerged over mobile phones. The debate and uncertainty over the poorly-understood *non-thermal* effects of mobile phone radiation was not mentioned.

Members of the group responded immediately, announcing that their conclusions would be significantly more equivocal. They expressed anger that the thoughts of their group had been distorted and leaked to the press.[20] Accusations that the conclusions of their report were somehow linked to the auction of third generation licences would undermine the group's credibility in what had become a deeply politicised debate. The conclusions that did emerge were very different from the one-dimensional reassurance offered by previous leaks. A look through the

---

[19] "Expert report gives mobile phones a clean bill of health", Patrick Wintour, Chief political correspondent, The Guardian, Friday April 28, 2000

[20] "Mobiles to carry health alert." Kamal Ahmed, The Observer, 30th April 2000

conclusions of the Stewart report shows how the IEGMP attempted to bring the controversy under political control, opening up areas of uncertainty and reframing the role of the public in scientific advice.

The summary of the group's recommendations begins by tentatively pointing out that many people own mobile phones and find them very useful. The group then justify their work by pointing out that there has been public concern about both phones and masts accompanying their ubiquity. The summary describes how mobile phone technology is one of a number of emitters of radiofrequency radiation in our environment and goes on to describe the NRPB guidelines designed to protect against the known dangers of heating. It is then pointed out that:

> "... rather little research specifically relevant to these emissions has been published in the peer-reviewed scientific literature. This presumably reflects the fact that it is only recently that mobile phones have been widely used by the public (paragraphs 2.1-2.12) and as yet there has been little opportunity for any health effects to become manifest." (IEGMP 2000, paragraph **1.16**)

Having pointed out that scientific understanding is not as adequate as might previously have been assumed, and that effects have been detected that cannot be thermally explained, the report recommends a broadly precautionary approach. The group makes the following recommendations under its interpretation of precaution:

1. ...that the UK adopts ICNIRP guidelines rather than continue with the NRPB's guidelines. (paragraph **1.27**)[21]. (The House of Commons Select Committee had recommended the same thing a year earlier).

2. ...that current planning rules for base stations are unacceptable (paragraph **1.30**). Base stations have obvious impacts on people's well-being (paragraph **1.31**) and have often been sited insensitively. Permitted development rights[22] should be revoked for base stations (paragraph **1.36**). Existing base stations should be audited to ensure their compliance with ICNIRP guidelines (paragraph **1.40**).

---

[21] As noted in the previous chapter, the notable difference is ICNIRP's additional five-fold safety factor in the SAR restriction for the general public.

[22] Previously, permitted development rights had eased the planning application process for all mobile phone masts less than 15 metres high. It was decided in the 1980s that the development of mobile phone networks was sufficiently important to the UK economy to relax normal planning procedures.

3. ... that mobile phones are labelled with their SAR levels, determined by an internationally standard procedure. Labels should appear on the handset's box, in leaflets at stores, on a national web-site and as one of the phone's menu options (paragraph **1.52**) (see discussion in previous chapter).

4. ... that children should be discouraged from using mobile phones (paragraph **1.53**) (see below for further discussion).

5. ... that scientific research should look specifically at mobile phone frequencies (paragraph **1.55**), that this research should consider uncertainties about pulsed radiation, and that dosimetry should be reliably calculated for each experiment (paragraph **1.56**).

6. ... that an independent research programme should be set up, funded jointly by Government and industry. "In developing a research agenda the peer-reviewed scientific literature, non-peer reviewed papers and anecdotal evidence should be taken into account" (paragraph **1.58**) (see below and chapter six).

7. ... that leaflets should be sent to all households to provide accurate and reliable information to the public (paragraph **1.61**). Hands-free kits and shields, if shown to work effectively, can be used as a way of reducing exposure (paragraphs **1.64**, **1.65**) (See 'aside' in previous chapter).

8. ... that the NRPB, a target of much criticism (see Chapter four), should aim to improve its treatment of public concern about mobile phones. Not only should the NPRB strengthen its non-ionising radiation research (paragraph **1.71**), but "the totality of the information available, including non-peer-reviewed data and anecdotal evidence, be taken into account when advice is proffered." (paragraph **1.70**) (see chapter six)

These recommendations served to subtly alter the emphasis for scientific advice about mobile phone health risks. The report took care to announce that the 'balance of evidence' suggested that mobile phones do not cause health effects. But the modalities in the group's summary set the agenda for a new appreciation of uncertainty. As one IEGMP member explained to me:

> "...show me that thing [my copy of the Stewart Report]. This is an extremely carefully worded sentence 'We conclude that the balance of evidence indicates that there is no general risk'. Maybe it's not that sentence... Ah, here it is. This is carefully worded: 'The balance of evidence to date suggests that exposures do not cause adverse health effects to the general population.' You see, *'balance'*, *'at present'* ['to date'], *'the general population'*, and then we've gone on importantly to say that there is now scientific evidence that there may be biological effects." (Interview transcript, No. 30)

These 'modalities'[23], emphasised by Sir William Stewart in public meetings, opened up areas of policy-relevant scientific uncertainty. The IEGMP emphasised that the suggestion of (non-thermal) biological effects was reason enough to be cautious. They also made a point of questioning in public the regulatory maxims that had become part of the EMF advisory tradition. As described in the previous chapter, the NRPB guidelines were built on an acceptance that thermal effects were dangerous and that susceptibility to these effects did not vary between people. The recommendations of the Stewart report questioned this accepted body of knowledge in a way that those with closer scientific experience of the issue might not have done. Sir William Stewart's evidence to the House of Commons Trade and Industry Select Committee throws more light on the approach taken by the IEGMP under his guidance:

> "Biological effects do not necessarily translate into health effects, but neither do they necessarily not translate. It is simply not possible to say that there are no potential effects on the human population. It is difficult to talk about the population because populations vary. Antibiotics do a wonderful job for the general population, but there is a subgroup in the population that is allergic to antibiotics; they cannot take them. There is a sub-group in the general population who cannot eat nuts because they are allergic to them. That is why we refer to the general population."[24]

One important difference between the Stewart report and other expert analyses was the suggestion that people might vary in their sensitivities to RF radiation. One IEGMP member admitted that he had been swayed by members of the public narrating their experiences of mobile phones and base stations:

---

[23] Another word borrowed from ANT to describe the qualifications added to scientific statements to adjust their status as conjecture, truth, prescription etc. (see Latour 1987, p. 22).

"I came to the conclusion at the Belfast meeting, although it had been through my mind before, that there may well be a genetic subgroup that are particularly susceptible to mobile phones." (Interview transcript, No. 30)

In addition to accepting some of the claims of people who claimed to be electrosensitive (hypersensitive to non-ionising radiation – see Chapter six), the IEGMP considered, on the basis of some limited evidence, that children would be more susceptible to any possible hazards from mobile phones.

"The biologists take hold" – Precaution for Kids

The most controversial of the Stewart report's recommendations pointed to uncertainties about the differences in radiation absorption between adults and children.

> "**1.53** – If there are currently unrecognised adverse health effects from the use of mobile phones, children may be more vulnerable because of their developing nervous system, the greater absorption of energy in the tissues of the head (paragraph **4.37**), and a longer lifetime of exposure. In line with our precautionary approach, at this time, we believe that the widespread use of mobile phones by children for non-essential calls should be discouraged. We also recommend that the mobile phone industry should refrain from promoting the use of mobile phones by children (paragraphs **6.89** and **6.90**)."

This recommendation marked the Stewart report apart from other expert reviews of the issue and came as a surprise both to industry and scientists. Experienced observers of the various sub-controversies that had defined the mobile phones health debate considered that the evidence for such a recommendation was scant. Some dosimetric models (from Om Gandhi's laboratory in Utah) which were reported in the media[25] had suggested that a child's head absorbs radiation more deeply, but this evidence, along with attempted replications which had shown no difference between adult and child absorption (from the lab of Niels Kuster in Zurich), had not been referred to by the IEGMP. The IEGMP felt, especially in the

---

[24] Sir William Stewart, Minutes of Evidence to the HCTISC, 13th March 2001

[25] "This is how a mobile phone heats your brain", Sunday Mirror, front-page story, 7th March 1999. Also, "The Mobile Menace" Tonight with Trevor McDonald, 8th November 2001

light of the recent evidence of cognitive effects (from Preece et al 1999) that children should be afforded greater caution than adults.

Many followers of the Stewart report's progress considered that this recommendation was influenced more by political expediency than science, influenced by the increasingly fashionable uptake of mobile phones among children. The Stewart report certainly provided embattled parents with a useful argument against their children owning or overusing a mobile phone.

> "I don't think they have so much to show for this specific precaution for children... Children might be a sensitive group, but it might be a little bit too hard warning children from using the phones... we can discuss if they *need* to use the phone, but that's another issue." (Interview transcript, No. 15 (MTHR member))

One interviewee was less reticent, arguing that, "this idea that children are more vulnerable is complete politics" (phrasing the word 'politics' to sound as derogatory as possible) (Interview transcript, No. 27).

The 'precaution for kids' recommendation stood out from what was otherwise an equivocal, cautious analysis of a complex issue. The news media seized on the recommendation, reporting the Stewart report as causing confusion by recommending restrictions of children's exposure without strong supporting evidence.[26] After a request from the Chief Medical Officer, Professor Liam Donaldson, for a clarification of what they had meant by 'children', the group stated that they considered that people under 16 should be discouraged from using mobile phones.[27]

---

[26] In an earlier piece of work, I analysed media coverage of the Stewart report, highlighting the interpretation of uncertainty in the popular press (Stilgoe 2000, 2001b) (Examples: "So are mobiles a risk or what?" (Daily Mirror, 12th May 2000), "All we want to know is: Are mobiles safe?" (Sunday Mirror, 14th May). "Parents confused at shambolic phone report" (Daily Mirror, 12th May))

[27] Clarification requested by Professor Liam Donaldson, Chief Medical Officer, on issues discussed in the Expert Group report on Mobile Phones and Health, First issued 16 June 2000, http://www.iegmp.org.uk/report/clarification.htm

Sir William Stewart, at subsequent public meetings and conferences, continued to argue that mobile phones should not be marketed specifically at children.[28] Industry, who had been implicitly accused of selling dangerous products to children, felt they were the victims of an attempt by an advisory body to assert their independence without any real evidence. One industry representative told me how he considered the recommendation political, rather than scientific:

> "I think it was unfortunate how they went away from their... scientific review and started moving towards policy areas. Talking about children, particularly, with no real fact base to actually say that... It didn't appear to be very well thought through. We had this whole build-up to the Stewart inquiry for several years. We were going to have this question answered, and then the London Evening Standard... the day the Stewart report came out was 'CHILDREN!', it wasn't 'PHONES ARE SAFE!'." (Interview transcript, No. 25)

Recommending that children should limit their use of mobile phones opened new areas of uncertainty where previously it had been considered (see previous chapter) that compliant mobile phone technology was safe, and that it was safe for all.[29]

The title of this section originates from the minutes of a discussion at a meeting of the WHO and ICNIRP in Cape Town. Asked what the drivers were behind the recommendation of limiting childhood exposure, one attendee replied that the "biologists took hold in IEGMP"[30]. This comment highlights a salient feature of the science behind mobile phone risk assessment. Understandings from areas of physics and biology must be combined to provide a complete picture. The (often inharmonious) interaction between physics and biology, and biologists and physicists, opens up myriad contested areas of understanding, allowing for new uncertainties.

---

[28] Some companies associated with the production of mobile phones have since publicly stated that intention to move away from making phones more attractive to children. Disney publicly stated in the wake of the Stewart report that they would stop collaborating with mobile phone manufacturers.

[29] This statement should be qualified by saying that NRPB guidelines had previously included a safety factor for situations in which children were present. But this addition was due to the size of their smaller bodies acting as more efficient antennae for EMF absorption.

[30] Minutes from WHO/ICNIRP Conference on EMF Biological Effects and WHO Standards Harmonization for the African Region and WHO RF Research Coordination Meeting Cape Town, South Africa, 4-7 December 2001

One AGNIR member described his experiences of understanding an issue in which radiowaves, traditionally the preserve of physicists, interact with biological tissues. In the following excerpt, he is discussing the previous concerns over ELF radiation from overhead powerlines.

> "I had an interesting experience relating to the much weaker Extremely Low-Frequency radiation. Because, there was a time... when Nobel-prize-wining scientists in the States said 'look this is utter nonsense. The amount of energy conveyed is such that there could not possibly be any effect. It's less than the noise that's going on the whole time'. And this was a firm view... I had to think, well now do I accept this advice and say it's a waste of time doing research on it? We just say 'no it couldn't possibly have any effect.' So I thought, well no. Effects on human physiology, you don't put your money on what a Nobel-prize-winning physicist says. I'd rather have a Nobel-prize winning physiologist come to it. And I asked two Nobel-prize winning physiologists in this country and they both thought about it and they both said 'no, I couldn't possibly say that it couldn't have an effect'." (Interview transcript, No. 29)

The differences in understanding between biological and physical perspectives cannot however simply be caricatured as physicists arguing that RF radiation is simply not powerful enough to cause harm while biologists provide evidence for subtle effects. Criticisms can equally be aimed at physicists who suggest danger by biologists who argue that there is no cause for concern. This excerpt is from an interview with a neuroscientist who forcefully argued that the biological evidence exonerated mobile phones:

> "... he's a theoretical physicist and he has these ideas that aren't based on science, they aren't based on experimental science and there's no evidence whatsoever for what he says, and not only that but he misunderstands some of the basic principles in biology, because he's a physicist." (Interview transcript, No. 27)

The interactions between scientists across a wide range of disciplines expands the contested territory in which scientific results and their relevance to public health are discussed. When a review of scientific evidence is compiled, therefore, uncertainties can emerge from the meta-analysis that are qualitatively different from those narrow uncertainties experienced at a laboratory level. We have seen so far in this chapter how, in the case of mobile phones, the reconstruction of uncertainty as both cognitive and policy-relevant undermined previous analyses of the issue that had, as the previous chapter illustrated, been considered sufficiently robust to support a discourse of compliance.

Revisiting the discourse of compliance

In the previous chapter, I linked the NRPB to a discourse of compliance which characterised discussions of mobile phone safety before the IEGMP engaged with the public controversy which had emerged. A good deal of the public antipathy encountered by the IEGMP had been focussed on the NRPB, who had been the first port-of-call for concerned members of the public. The Stewart report, under its broad remit to "consider present concerns", addressed the question of how the NRPB could have better handled anxious members of the public. The recommendations that followed can be read as an attempt to advise a regulatory body on improvements in risk communication, but they can also be read as an attempt to move away from the prevalent discourse towards one of engagement. The following paragraphs respond directly to the criticisms expressed during the controversy:

> "**1.67** Whilst there is no criticism of its science, we recommend that NRPB gives greater priority to the execution of a more open approach to issues of public concern such as mobile phone technology and that it is proactive rather than reactive in its approach."
>
> "**1.68** We recommend that public concerns about risk be addressed by NRPB in a more sensitive and informative manner."
>
> "**1.70** We recommend that in a rapidly emerging field such as mobile phone technology where there is little peer-reviewed evidence on which to base advice, the totality of the information available, including non-peer-reviewed data and anecdotal evidence, be taken into account when advice is proffered." (IEGMP 2000, chapter 1)

This last paragraph (**1.70**) has greatly influenced my project's focus on anecdotal evidence, which is considered in more detail in the next chapter. For the purposes of this more general analysis, these recommendations specifically addressed to the NRPB tell us a good deal about the role that the IEGMP saw itself filling. The Select Committee on Science and Technology had, in 1999, rejected the criticisms of the NRPB that emerged in the course of its deliberations. It was felt that the NRPB would continue to have advisory responsibility in the area, and that other committees should take care not to threaten the authority of their advice or its scientific basis. The Select Committee suggested that the NRPB should be strengthened in the area of non-ionising radiation to better cope with such controversies in the long term, with the IEGMP being a temporary measure (with temporary impact). The IEGMP, reconsidering similar criticisms of the NRPB, went further. Though taking care not to undermine the integrity of NRPB's scientific research, the IEGMP sought to change the way the NRPB thinks about and performs its role as an advisory body. In considering the public context of the mobile phones controversy, the IEGMP saw the limitations of a discourse based on the premise that compliance with guidelines dictates safety. In particular, responding to the vociferous criticisms expressed by members of the public, the shortcomings of a compliance-based approach to dealing with planning objections were made clear:

> "**6.43** The location of base stations and the processes by which they are authorised appear to be the aspects of mobile phone technology that generate most public concern... In assessing the potential impact of a planned base station on health, the current approach in the UK is to determine whether it might cause exposures in excess of NRPB guidelines... If this can be ruled out satisfactorily, risks to health are not considered further.
>
> **6.44** We believe this approach is not optimal since it does not allow adequately for the uncertainties in scientific knowledge. Although it seems highly unlikely that the low levels of RF radiation from base stations would have significant, direct adverse effects on health, the possibility of harm from exposures insufficient to cause important heating of tissues cannot yet be ruled out with confidence. Furthermore, the anxieties that some people feel when this uncertainty is ignored can in themselves affect their well-being." (IEGMP 2000, Chapter six)

The two paragraphs above go further by explicitly rejecting the tactic of using compliance as an endpoint for discussions about safety. But it will have become apparent in this chapter that other recommendations of the Stewart report *implicitly* undermined the discourse of compliance by acknowledging the contingencies of the science behind the guidelines in a public arena. The IEGMP's style of precaution empowers non-expert decision-making by suspending judgements as to the rationality of non-expert decisions (in any case, as described above, non-experts are eminently sensible to question advisory reassurances in the light of previous mistakes). In the previous chapter, we saw how issues such as the labelling of phones and international disagreement challenged the discourse of compliance. The Stewart report, by recommending labels, changes in guidelines and new areas of research, highlighted the political factors which had shaped previous scientific advice reintegrating the *advisory* role with the *scientific.*

The efforts to rationalise such a precautionary approach are illustrative of the problems of maintaining a balance between authority and public sensitivity in scientific advice. In order to expand the issue beyond compliance, which had disenfranchised people who felt aggrieved living near base stations, the IEGMP adopted a definition of health (from the WHO) which included the effects on 'well-being' mentioned in paragraph **6.44**. The minutes of the IEGMP's third (private) meeting indicate the sort of discussion that took place on this issue. This excerpt follows a discussion of the previous evening's public meeting at which the group were reminded that the majority of public concern was about base stations:

> "WHO define health in terms of material and physical wellbeing, and therefore consider psychosocial effects. Base stations can elicit stress reactions in a minority of people that may lead to clinical symptoms. This type of effect cannot be addressed by simply restricting exposure, and the only real option is better education."[31]

In the light of the criticisms of naïve conceptions of the public understanding of science I described in chapter two, we could take exception to the call for 'education' rather than 'engagement'. But the important point is that the group is starting here to appreciate that the health issue is largely inseparable from the broader social issues of control of exposure, perception of risk and trust. As Sir William Stewart explained to the House of Commons Trade and Industry Select Committee:

> "People can choose whether to use a mobile phone or not and legislation should not be introduced for that because it is not necessary. But if the population has no choice and there is a perceived risk associated with it, then one has to take a different

---

[31] Minutes of the 3rd meeting of the IEGMP, 12th November 1999, Edinburgh

approach to the issue. That was one of the facets that we considered in relation to masts."[32]

Once the line was blurred between health and well-being, the question of risk became subordinated to wider questions of trust, safety, choice and democracy. However, as the previous chapter described, the discourse of compliance had constructed NRPB guidelines and the science behind them as mutually reinforcing. Undermining the discourse that had characterised the debate up to the point of the Stewart report was therefore likely to threaten the authority of scientific knowledge in this area. The fresh emphasis of the Stewart report therefore required careful negotiation. The minutes from the group's seventh meeting point to discussions over lowering guidelines. The group had decided to recommend lowering the guidelines to ICNIRP's levels…

"…but it was important to be clear about the reasons for this… there was a need for a more precautionary approach with respect to the sensitive subgroups for whom risks were not clearly quantified… It appeared likely that compliance would not be a significant problem… There was some discussion about how to address non-thermal effects in guidelines. Although the existence of non-thermal effects was beyond reasonable doubt, it was not clear whether there were any health implications. Thus, it would be premature to base guidelines on non-thermal effects. The possible existence of non-thermal health effects should be recognised, and good research should be put in place to investigate them. There should be an assumption that guidelines may need to be revised in the light of this research."[33]

We begin to see the problems encountered in rationalising a new approach to scientific advice, in which the discourse of compliance is rejected because it "does not allow adequately for the uncertainties in scientific knowledge" (paragraph **6.44**), but uncertain effects cannot be incorporated into guidelines because it is "not clear whether there were any health implications". The discussion above only indicates that the question of the adequacy of current guidelines must be left open.

Discussions in the light of the group's decision to adopt a precautionary approach were littered with references to the dangers of undermining existing guidelines. The guidelines were constructed as authoritative by their basis in well-established science.

---

[32] Sir William Stewart, Minutes of Evidence to the HCTISC , 13th March 2001

[33] Minutes of the 7th Meeting of the IEGMP, 4th February 2000, Belfast

To suggest their inadequacy would be to undermine the credibility of this body of knowledge. The above snapshot of the group's discussion includes the fact that the vast majority of existing mobile phone equipment complied with ICNIRP levels, so the move would not cost the industry a great deal. Industry would be able to remain distanced from regulation, as long as the guidelines were not *technology-based* rather than *science-based* (discussed below). But the IEGMP took care to suggest that the philosophy behind guidelines must remain consistent. Once precaution is advocated and non-experts are empowered to make individual decisions about limiting exposures, defending the guidelines becomes more difficult.

In Sir William Stewart's evidence to the House of Commons Trade and Industry Select Committee in 2001, his cross-examination revealed the inconsistencies which inevitably arise when a scientific advice document is defended concurrently on pragmatic and rational grounds. Among the recommendations of the Stewart report was an audit of base stations, especially near sensitive sites such as schools, to reassure the public of their low levels of exposure. The Select Committee asked why Sir William had not considered other sites where children were present:

> "My point is that I do not care much whether it is a church or a football stadium, so long as they meet the guidelines. Those are the guidelines that have been nationally accepted at the present time. We should adhere to those until evidence becomes available that they should be higher or lower."[34]

In the light of the rejection of the discourse of compliance seen in paragraph 6.44 of the Stewart report, this answer seems inconsistent. But it illustrates the difficulties of maintaining authority while defending scientific advice that explicitly addresses issues of public, as well as scientific, concern. The tactic that is revealed is interesting. When the Stewart report is challenged by scientists who claim it did not represent the true state of the science, the explanation most often given is that it was 'flavoured' by the political elements of public concern, distrust and the spectre of BSE. But when the Stewart report is itself challenged on the grounds of the myriad policy and advisory questions that it has opened up to challenge, the easiest retreat is to appeal to rationality, arguing that it is the guidelines, supported by expertise,

[34] Sir William Stewart, Minutes of Evidence to the HCTISC, 13th March 2001

which provide protection (as above).[35] A tension emerges, which contributes to the discursive inconsistency, between *science* and *advice*, between *rationality* and *pragmatism* and between *public reassurance* and *public engagement.*

Another feature of the Stewart report indicated by the comment above is that Sir William sees his work as a political stopgap, situated in political time and space by the level of public concern. His appeal to the guidelines on the basis of the evidence that supports them is qualified with the assurance that the guidelines should be changed when scientists have reason to suspect their inadequacy (or indeed, their unwarranted stringency). The Stewart report is seen as a holding pattern for the reorganisation of long-term scientific advice on mobile phones. But, as mentioned in the previous chapter, the guidelines for protection from the thermal effects of electromagnetic fields (and the science of bioelectromagnetics), extend far beyond the particular politics of the mobile phones health issue. The Trade and Industry Select Committee asked Sir William why he recommended carefully auditing exposure from base stations near schools when the exposure from computer screens within the schools is likely to be orders of magnitude higher:

> (Sir William Stewart) "Yes. It is an extremely difficult thing on which to get a balance. The argument that has been put to us, not in relation to computer screens, but in relation to microwave ovens, is that you have a choice about using a microwave oven and you have a choice about using a computer but you do not have a choice on whether a base station is stuck outside your house or not."
>
> (Christopher Chope, MP) "With respect, in a school you do not have a choice about using a computer. Computer rooms are set aside and children are forced to sit in front of the computers. Do you think that parents should have the right to withdraw their children from computer rooms?"
>
> (Sir William Stewart) "I shall not get involved with computers. I am talking about mobile phones and masts and I shall stick to that."[36]

NRPB scientists I interviewed reminded me that the SAR basic restriction guidelines existed before mobile phones, and were designed to apply to any technology

---

[35] I am reminded of Bruno Latour's maxim: "When controversies flare up the literature become technical" (Latour 1987, p. 30)

[36] Minutes of Evidence to the HCTISC, 13th March 2001

emitting EMFs. Changes to mobile phone regulation would therefore necessitate the consideration of other technologies. The implications of the recommendations of the Stewart report beyond the mobile phones issue were crystallised in the suggestion that the NRPB guidelines should be lowered to the level of the international (ICNIRP) and European guidelines. On the 1st of May 2003, the NRPB released a consultation paper (their first) in which the move to adopt ICNIRP levels was mooted on precautionary grounds (NRPB 2003). This consultation document was the most lucid addition to a debate which had captured scientists, activists and policy-makers following the Stewart report. Disagreements over the applicability and interpretation of precaution brought to the fore expert constructions of both expertise and the public.

Disagreements on precaution

The precautionary language used in the NRPB's consultation document is surprising, given the NRPB's previous dependence on its reputation as dispensers of 'science-based' advice. The document demonstrates clearly an acceptance of the need for broader-based scientific advice. But if we look closer, we see that this rhetoric of precaution implicitly protects the scientific authority of existing guidelines and their science base:

> "In consideration of the Precautionary Principle, scientific data and its uncertainties are only one input into the evaluation" (NRPB 2003, paragraph 28)... "Factors other than scientific ones should be considered in assessing the applicability of the Precautionary Principle. There is clearly considerable public concern about exposure to RF radiation from mobile phone masts. It is the view of the NRPB that RF mobile phone radiation should... be considered as an issue for the application of the Precautionary Principle" (ibid., paragraph 32).

The point is made that precautionary approaches should affect the risk assessments that takes place, leaning towards cautious assessment of safety (ibid., paragraphs 473, 474). But the document leans overall towards suggesting that the scientific evidence is a consistent bedrock, and that the responsibility for taking into account 'other factors' lies with the Government (ibid., paragraph 2). A response to the consultation document, from the UK Electricity Association, whose relevant controversy over power-frequency EMFs is still bubbling under, reminds the media that such precautionary measures in no way undermine previous guidelines or existing science:

> "The electricity industry is totally committed to the safety of our customers, the public and our staff. We comply with all relevant limits and guidelines and in the case of electric and magnetic fields (EMFs) look to Government to determine what these limits are and how they should be applied. We fully support this approach and the consultation process being followed.
>
> The NRPB's consultation is basically about deciding on what are the best safety factors. The new proposals do not change what the science says but are more about looking at what could be the pros and cons of changing the safety margins from those we use now. This does not mean that the old guidelines were fundamentally flawed, or that they were protecting against the wrong things. It is simply asking the question, should we have even greater safety margins than we already have, and if we do, how will we benefit.
>
> We believe in open debates on issues such as this, and will be responding to the consultation in the normal way once we have had the opportunity to consider the detail."[37]

'Safety factors', in this statement, are seen as a political veneer to be laid on top of well-established guideline levels, representing the best science available – a difference in political philosophy rather than risk assessment. This is one example of an attempt to defend the authority of science, in this case by retreating the boundary of authority away from the now-contested territory of guidelines, in the face of perceived challenges (cf. Jasanoff 1987). These challenges, which were usually interpreted as unscientific (or anti-scientific), came from advocates of the application of precaution to the case of EMFs (A special edition of the professional journal "IEEE Technology and Society" (Winter 2002/2003) contains a number of viewpoints on precaution and EMFs). One scientist described this situation to me:

> "... there's been a battle going on between the people who believe in precautionary measures because there isn't enough science and the real scientists who know what science there is and who are experts in their field." (Interview transcript, No. 27)

---

[37] Dr John Swanson, Scientific Adviser to the Electricity Association, quoted in a press statement: NRPB consultation on limiting exposure to Electromagnetic Fields, 1 May 2003

At the heart of the arguments against the adoption of precaution usually lies a construction of an irreverent public who will worry unduly if experts admit uncertainty. As one AGNIR interviewee put it: precaution, by supporting views that are unscientific, can spread "alarm and despondency" amongst the public (Interview transcript, No. 29).

Debates about precaution are debates about whether science should be a lone voice in determining safety, with compliance as an endpoint for disputes, or whether the scientific evidence should be one contributory factor in decisions about safety. Certain scientists felt that the call for precaution, including reframing scientific uncertainty, was encouraging the development from an issue controlled by expertise to one controlled by public whims. Michael Repacholi, head of the WHO's international EMF project, was the only member of the IEGMP to publicly dissent from the group's precautionary recommendations. An article in Microwave News at the time of the Stewart report's publication claimed that Repacholi argued against Stewart's recommendation that children should be discouraged from using mobile phones. The MWN article, putting the Stewart report in the context of diverse international policies on mobile phone regulation, refers to a WHO document which warns that "scientific assessments of risk and science-based exposure limits should not be undermined by the adoption of arbitrary cautionary approaches"[38]. The implicit reference here is to policies such as that in Switzerland, which follows the principle of As Low As Reasonably Achievable (ALARA) laid down in Swiss environmental law. The Swiss policy claims to be 'technology-based' rather than 'science-based', considering the level of EMF that is necessary for the technology to work effectively.

MWN's dissenting view comes from an Austrian scientist, Michael Kundi, who is quoted as saying "the claim that precautionary measures might 'undermine' science is a platitude... Any proposal for a guideline or limit value has to apply principles that are not purely scientific" (MWN, May/June 2000, pp 5-6). As discussed in the previous chapter, guidelines can never be a representation of scientific truth. There

---

[38] "Electromagnetic Fields And Public Health – Cautionary Policies", World Health Organisation Backgrounder, March 2000, http://www.who.int/docstore/peh-emf/publications/facts_press/EMF-Precaution.htm (accessed 15th August 2003)

is no one nexus from science to regulation. Decisions must be taken about the relevance of uncertainty and the adequacy of existing knowledge.

The negotiation of precaution reveals a negotiation of boundaries around 'what science is' and its role in providing advice. If guidelines are perceived as sufficiently science-based that they represent a monolithic body of scientific knowledge (as in the 'discourse of compliance'), a precautionary policy can either be viewed by experts as undermining science or it can be negotiated as a pragmatic 'safety factor' addition to these guidelines. In either case, there is a retreat from political encroachment to defend areas of established cognitive authority. Precaution, by forcing experts to consider 'inexpert' facets of a controversy, reveals expert constructions of the general public. Although it is not the purpose of this thesis to assess whether such constructions of the public were astute, we can see that they, as with many other expert-led prescriptions of the public understanding of science, served a rhetorical purpose (cf. Locke 2002). Activists who supported precautionary policies pointed out that experts were being disingenuous when they claimed that the public would react irrationally to precautionary recommendations (Field notes, "Mobile phones – Is there a health risk?" conference, 20th-21st September 2001).

The Stewart report played a large part in setting the agenda for broader consideration of the mobile phones health debate in its public context. By recommending a precautionary approach to the handling of the issue, it helped expose the politics, and so undermine the credibility of the prevalent discourse of compliance. Some of its recommendations have still not, at the time of writing, been followed up by Government, but a few have markedly changed the shape of the issue.

Mobile phones and health after Stewart

an anecdote about a morning mast protest that never was

16th September 2002

The leaflet advertising the "anti-phone mast morning" in Hawksley court, part of a council-owned estate in Stoke Newington, was not optimistic about the possibility of being able to confront the network operators with their objections. Hackney council, having agreed to rent out its roofspace, had been told that contractors would arrive on the 16th September. Residents of the estate wanted to be ready to meet them with their concerns that they felt had remained unheard.

I arrived at 9 o' clock to find a three people sat behind a trestle table waiting for the network operator not to turn up. A few others gathered around, drinking tea from polystyrene cups. After I had introduced myself, John took me round the corner of the building to cheerfully show me the existing mast, a construction about two metres high on the roof of the estate where, it was assumed, nobody could get near it. He also showed me the structural damage that the mast's erection had contributed to.

The mast had arrived in December 1998 without warning, courtesy of Mercury (which was soon to be renamed 'One2One' and subsequently 'T-Mobile'). The council, owners of the roof, had leased it to the operator without consultation of the residents who it might immediately affect. Kate claimed that although the mast had been there for the last four years, no-one had received any rent from the operators until two weeks ago.

A month after the mast had gone up (15th January 1999), some of the residents were invited to a consultation meeting, to talk about the existing mast, they assumed. But this meeting, arranged by Orange, was set up to discuss the siting of more masts on the rooftop, a prime location because the five-storey estate is built on high ground. The council, despite claiming they would stand 'shoulder-to shoulder' with residents, granted a lease to Orange later that year. After all, John said, the council had to make money from their real-estate.

In the spring of 2000, residents received a letter from the council telling them of Orange's plans to arrive later that month to put up their new masts. The residents organised their opposition, based on a complicated series of concerns. Petitions were signed, letters were written, and the local paper published articles by Dianne Abbot, the local MP. After all, Islington council didn't allow masts on its residential blocks, so why should Hackney?

The first problem, the residents told me, was the lack of public consultation throughout the process. Kate said that one of the operators had a department of two people to deal with public objections nation-wide. The operators' policy was to put up the masts before people had time to object.

The second issue was that of unproven health risks. John said that the authorities only cared about the thermal effects of radiation. Little or no work was being done into the non-thermal effects of pulsed radiation. What was needed was for someone independent to step back and look at the whole issue. Research should be funded by industry and run independently. The campaigners did not assume there was a risk. They just want to be able to trust the quality of research that was being done to tell them whether the masts are safe. BSE was still fresh in people's memories, along with the problems with asbestos and childhood cancer from power lines. They were also worried about the children on the estate. Even though the operator says the mast is safe because you can't get too near it, children play on the roof and get much closer than they should.

Some of the residents said that they got headaches when they were near the mast. Kate said, "I felt physically sick. Now whether that's in my mind, I don't know." John admitted that much of what people experienced was "a psychological thing". But, he said, a perceived health risk, and the stress this causes, has to be taken into account.

(All names have been changed)

This scene suggests, despite its singularity, a broader discourse for the consideration of public interactions with expertise. It also provides a vignette in which members of the public are not just perceiving (distorting) risk. They are actively constructing layers of trust and engagement to come to terms with the complexities of the science and politics involved. The issues of health and well-being, consultation, uncertainty and independent research resonate with the recent shift in advisory attitude. Opinions on these issues informed the Stewart report. But crucially they are also informed *by* the Stewart report.

After the Stewart report, a number of bodies issued documents which responded, either directly or indirectly, to the issues raised by the IEGMP.[39] There were also protracted Government responses,[40] which addressed some of the recommendations and obscured others, and (largely vacuous) statements from network providers and mobile phone manufacturers.[41] But Sir William Stewart maintained a degree of

[39] See, for example, BMA (2001), Green Alliance (2001) (a report commissioned by BT), Zmirou et al (2001), Health Council of the Netherlands (2002), NRPB (2003)

[40] Government responses, Department of Health http://www.doh.gov.uk/mobile.htm, accessed 20th October 2003

[41] To take one example, the Mobile Operators' Association, representing network providers at the time of the Stewart report, welcomed the IEGMP both addressing public concern and supporting the existing science base. ("Response to the Stewart Report: A report welcomed by industry" 10th May

control over the development of public science in the area through his Mobile Telephones Health Research (MTHR) programme, originally commissioned by the House of Commons Science and Technology Select Committee, but formed as the only directly visible output of the Stewart report.

More work for MTHR

The MTHR programme (affectionately referred to as 'Mother' by one of my interviewees) was set up to allocate £7.4 million of research funding (collected in equal shares from government and industry) to a collection of projects which would address the uncertainties raised by the IEGMP which, as described above, were broader than those that were considered by the NRPB. The MTHR committee kept many of the original IEGMP (Blakemore, Challis, Repacholi, Stewart, Barclay, Rugg), but lost the two IEGMP lay members, and added nine other scientists (including two from other European countries (Kjell Mild and Niels Kuster) and two from the NRPB (Alastair McKinlay and Zenon Sienkiewicz) and a social scientist, Simon Gerrard from the University of East Anglia.[42]

Sir William Stewart claimed that the aim of the programme was to plug the gaps in our knowledge and provide scientific answers,[43] but it was clear from the outset that the MTHR would equally provide an opportunity for these experts to engage with members of the public whose fears had not yet been calmed. At the time of writing, the MTHR had held 3 public meetings (on the 8th November 2001, the 11th November 2002 and the 4th November 2003) to discuss the various calls for research proposals (and to reassure industrial representatives that their money was not being wasted). Press releases were issued and leaks dripped information of research developments to the news media, especially when this research was sufficiently

2000 – http://www.mobilemastinfo.com/media/issue_statements/11_05_00.htm, accessed 20th October 2003)

[42] Full MTHR membership: Professor L W Barclay, Professor C Blakemore, Professor G Breakwell, Professor L J Challis, Professor C Chilvers, Professor P Elliott, Dr S Gerrard, Professor T Grant, Professor N Kuster, Dr A McKinlay, Professor J Metcalfe, Professor K Mild, Dr M Repacholi, Professor M Rugg, Dr Z Sienkiewicz, Sir William Stewart, http://www.mthr.org.uk/members/index.htm, Accessed 18th August 2003

[43] Sir William Stewart, interviewed on BBC1 Breakfast News 25th January 2002

different from previous work that it could be described as 'addressing public concern' (see chapter six).

The questions asked by the MTHR continued to redefine the mobile phones controversy as the Stewart report had done. The topics of research were an important site for the negotiation of control between experts and members of the public. But the fact that the public had a voice in making important research decisions was seen by some as a betrayal of the ideal of independent, value-free research. As with the Stewart report, some interviewees saw the MTHR programme as an unjustified effort to wrest control away from the 'real science' being performed by independent (and by implication apolitical) bodies such as the NRPB:

> "...so I'm afraid the UK government has got it all wrong, they've made science into something political, and the body that was the scientific stronghold [the NRPB], they've taken the research money away from, and said we'll administer it through the Department of Trade of Industry" (Interview transcript, No. 27)

The MTHR programme was certainly created to allocate research funds in a more publicly-accountable manner and it has certainly, as we shall see in the next chapter, gone about its job in a way that is just as *public* as it is *scientific*. It has therefore adopted the mantle of the IEGMP as a site for engagement and public criticism, distracting some critical attention away from the role of the NRPB and industry in continuing to manage the issue. The nature of the MTHR programme has therefore allowed it to have significantly more impact on the continued production of public science than might have been suggested by its relatively paltry budget (£7.4 million)[44].

The science that will emerge from MTHR funding will contribute, in terms of the production of *evidence*, only a small part to a series of international schemes looking at the same area. These have been, or are being, funded by the American cell phone industry (Wireless Technology Research (1993-1999)), the World Health Organisation (who began their international EMF programme in 1996), and various

---

[44] This figure might have seemed relatively large if only laboratory studies were funded, but the desire of the IEGMP to see relevant and robust epidemiology studies has stretched the MTHR programme's budget. One case-control study, whose principal investigator is IEGMP and AGNIR member Anthony Swerdlow, was funded to the tune of £960,000.

other national governments. These directed research programmes, particularly the WHO, have suggested the need to increase the robustness of the science that is being done, providing standard measurements of SAR, type of radiation etc.. The Stewart report identified the problems of extrapolating a risk assessment from a body of studies that consider a range of exposures and called for a similar standardisation of experimental protocol. In its deliberations, the IEGMP revealed a body of evidence, some of which was of questionable relevance to exposures from mobile phones. Despite the Stewart report's appreciation of previously-marginalised evidence, it made clear its intention to protect future scientific work from a similar degree of deconstruction. Much earlier science had been easily dismissed because of failures in replication of effects.

Replication Disputes

There continues an international effort to scientifically answer questions that have been asked by the public controversy (fuelled in no small part by the original suggestive studies described in the previous chapter) as well as strengthen the base for existing knowledge about thermal effects. Since the Stewart report, many attempted replications of studies such as those by Preece et al (1999), Repacholi et al (1997) and Lai and Singh (1995, 1996) have failed to produce positive results. But, as the canon of SSK would predict (see especially Collins 1985), the results of such replications have been greeted by the scientific community and by activists with multi-layered disagreements. One scientist, responding to the question of replication, told me:

> "There's invariably some essential aspect to the experimental protocol which is different. So it's not a replication. And since, in biology, it's probably why it's very much a multi-factorial situation - it's very unlike experiments in physics where, you know, it's almost impossible to get the same conditions. You could have exactly the same experimental set-up, you use mice, I want to replicate, my mice might already be stressed, yours weren't, or something. You know, it's almost virtually impossible to guarantee absolute coincidence of conditions which are necessary for a replication to be a replication." (Interview transcript, No. 9)

With any replication study, it is possible for others to find differences that can be used to reject the relevance of the research. Even when a study (in this next quote, it is Alan Preece's research that claimed a cognitive influence on humans with mobile phone exposure) is replicated and produces a similarly positive effect, other scientists can claim that the difference in experimental design negates the replication's purpose:

> "… he says that the research in Finland [funded by Nokia, carried out by Mika Koivisto] has replicated it, but as a scientist, if you look at the research in Finland they did different measurements. And their whole set-up is different. The phone for Preece was at this angle [mimes], and these people had it this way [mimes]. They're exposing a different part of the brain. Everything about the Koivisto research is different than Preece's research, so there's no way that they replicated what he had" (Interview transcript, No. 27)

In the previous chapter, I mentioned an epidemiology study that claimed an increased relative risk. Aside from an extension to this study (see Hardell et al. 2002), epidemiology has so far found no correlation between mobile phone use and increased risk of cancer. For many epidemiologists, this is taken as an indication that mobile phones do not present a long-term risk. But others have argued that such studies are irrelevant, either because their time horizons are not long enough to identify diseases with long latency periods (such as cancer), or because they look for effects in the general population, putting a smokescreen in front of effects which might occur in a susceptible subgroup.

In addition to the deconstructive gaze of fellow scientists and other observers, the persuasive power of replication studies is reduced because they rarely intend to copy exactly the original experiment. As Sir William Stewart said at an MTHR public meeting, "why should we replicate a sub-optimal study?"[45]. Science is caught in a paradox of attempting to improve on previous work while increasing the robustness of existing findings, and the progress of research in a sensitive public debate will be constrained by such disputes over replication. A key constituent of the rationale behind the predominant regulatory philosophy is that thermal effects are clearly reproducible, while non-thermal effects are not. Clearly, therefore, there is a great deal at stake in discussions about what constitutes a valid replication of a suggestive study.

---

[45] Sir William Stewart, speaking at the MTHR Research Seminar, 11th November 2002

Planning Disputes

The science, completed or ongoing, that experts use to assess possible risks is, since the Stewart report, largely inseparable from the public disputes which have framed the issue of mobile phone health effects. IEGMP members were surprised at public meetings that the antipathy felt by the public to mobile phone masts was greater than concern about the phones themselves, despite the much lower exposures from masts. As the mobile phones controversy unfolded, its most visible manifestation was the increase in protests, and often direct action, against existing or proposed base station sites. The Stewart report attempted to give more control to members of the public who would be directly affected (in whatever way) by the masts. Recommendations to improve consultation and increase the powers of the public in planning decisions were however obfuscated by the Government, who were concerned that local councils were not equipped to handle both the science and the politics of full, open consultation. The Governmental view was presented in a letter from housing and planning minister Nick Raynsford to local councils in June 2000:

> "It is our view that, if a proposed development meets the ICNIRP guidelines (as recommended by Stewart on a precautionary basis), it should not be necessary for a planning authority, in processing an application, to consider the health effects further."[46]

A civil servant reiterated this position to me in an interview:

> "Health effects can be taken into consideration, but they're met by the requirement of compliance with the ICNIRP exposure guidelines... So the bottom line is, if it complies with ICNIRP... then that health consideration, which was a judgement that health considerations can be part of the planning process. There was a judgement on that a few years ago. But that criteria is met by compliance with the ICNIRP exposure guidelines." (Interview transcript, No. 11)

This argument confused me, as I am sure it confuses the many people who encounter it in their dealings with planning authorities. It does not add anything to the debate, nor does it alter the balance of control of planning decisions. The

---

[46] Letter from Nick Raynsford MP to all local planning authorities, 29th June 2000, quoted in the Local Government Association Response (annex 1), submitted as evidence to the House of Commons Trade and Industry Select Committee (tenth report), 2001

previous chapter described how questions of compliance were questions that were constructed by expertise in such a way that they can easily be answered within the framework of existing, certified knowledge about electromagnetic fields. So to say that 'health effects can be taken into consideration' until compliance is demonstrated is to ignore the most important question being asked by non-experts – are the guidelines sufficient to protect us?[47]

Confusion, partly encouraged by the Government's involvement, as to whether health concerns should be a 'material consideration' in planning decisions, has led to local fragmentation of the planning issue. After the Stewart report, some local authorities imposed a moratorium on new base station sites on council property while others were more permissive. Confusion about what counted as a legitimate argument against the erection of new base stations was exacerbated by planning processes that were intended to make consultation easier. The Stewart report, by broadening the debate beyond compliance, caused problems for a planning system that was not equipped to deal with more sophisticated complaints from members of the public (as demonstrated in the discussions between Sir William and the House of Commons Trade and Industry Select Committee which looked at the issue in 2001). Local newspapers continued to carry regular reports of public disquiet against mobile phone masts, while the national press tended to concentrate on new scientific reports which spelt danger. The local issue of planning disputes, which was framed by many experts as just another NIMBY (Not In My Back Yard) problem, was divorced from the wider (if not universal) issue of the adequacy of scientific knowledge about mobile phone health effects. The Stewart report had begun to open a door which allowed non-experts to view, and engage with, the uncertainties in the scientific evidence. But many members of the public found this door closed during planning decisions.

In December 2000, a group was formed to combat previous frustration at planning disputes over mobile phone base stations. Mast Action UK was launched, with the sponsorship of the constituency MP of its leader, Julie Matthews, at the House of Commons (Burgess 2004, p. 214). Their purpose was to collect coherent arguments that base stations were being sited insensitively considering uncertainties about the

---

[47] ... and other more recursive questions, such as, 'why should we believe you when you tell us that the guidelines are sufficient?'

health effects of RF radiation. The group tried to clarify that their objections were not based upon the aesthetically unappealing nature of the larger masts, nor upon a desire to restrict the service provision of the mobile phone networks. After the Stewart report, it was felt that Government, central and local, needed to more fully appreciate both the relevant uncertainties and the legitimate health concerns of members of the public. In 2001, the House of Commons Trade and Industry Select Committee heard evidence from industry and politicians on the issue of changing planning legislation to more closely follow the recommendations of the IEGMP.

I have already mentioned the House of Commons Trade and Industry Select Committee's investigation of the planning issue, at which Sir William Stewart was invited to defend the conclusions of his report. The report of the select committee made concrete many of the confusions that had been identified since the Stewart report and endorsed the Government's recent efforts to improve consultation. The IEGMP's foray into planning recommendations was questioned by the select committee, who considered that the expert group were not fully equipped to consider such things (House of Commons 2001). The Stewart report, by recommending changes to planning rules, had gone far beyond what many people had expected, an authoritative, independent review of the scientific evidence. But the IEGMP, encouraged by their charismatic and politically-astute chair, had always intended that their review should be different from the NRPB's attempts at controlling the controversy over mobile phones.

Conclusion

By the time the IEGMP began their analysis of the issue of mobile phones and health, the issue had expanded far beyond being 'scientific'. Public controversy and a loss of trust in expert advice and industrial responses had meant that questions about the riskiness of mobile phones were intertwined with questions about the quality and independence of scientific advice and the political factors that had gone into shaping the discourse of compliance. The IEGMP ostensibly considered the issue from as many viewpoints as possible, with a scientific review considered necessary but not sufficient for a complete understanding. Rather than looking from a policy perspective at how best to deal with the controversy, or from a scientific perspective at what the evidence tells us, the IEGMP did both concurrently. The

IEGMP's product, The Stewart Report, presented a reconstruction of the relevant science. The uncertainty that had previously been regarded as unimportant for regulation was given greater prominence.

The Stewart report was well-received by the majority of scientists, activists and concerned non-experts. Some experts felt that the report did not provide a true picture of scientific knowledge about the issue, but conceded that the reason for this was the necessary political slant. On activist email lists, in pressure groups and in publications such as Microwave News, the Stewart report was held up as an example of a sensible and sensitive "Call to Action" (MWN, May/June 2000, p.19). By adopting what was perceived as a fresh approach to uncertainty, the Stewart report undeniably contributed to the agenda of arguments from all sides in any discussion of issues of mobile phone safety. If we compare the Fact Sheets from the World Health Organisation from 1998 and 2000 (the latter updated in the light of the Stewart report), we can see how the discourse of one of the most influential global health advisory bodies is subtly augmented after feeling the influence of the IEGMP. Both documents unsurprisingly claim that further research is needed, ambitiously stating, in effect, that 'the answer' is just around the corner (the 2000 fact sheet states that, "It will take about 3-4 years for the required RF research to be completed, evaluated and to publish the final results of any health risks"). The 1998 fact sheet leaves the issue there – a reassurance that experts are doing all that can be expected of them to clear up the issue. But the later document adds that policies based on precaution might also be practical:

> "Precautionary measures – If regulatory authorities have adopted health-based guidelines but, because of public concerns, would like to introduce additional precautionary measures to reduce exposure to RF fields, they should not undermine the science base of the guidelines by incorporating arbitrary additional safety factors into the exposure limits. Precautionary measures should be introduced as a separate policy that encourages, through voluntary means, the reduction of RF fields by equipment manufacturers and the public"[48]

[48] Electromagnetic Fields and Public Health: Mobile Telephones and Their Base Stations, WHO Fact Sheet N° 193 Revised June 2000 http://www.who.int/inf-fs/en/fact193.html accessed 20th August 2003

As described in this chapter, there is a tension expressed in the WHO's call not to undermine existing guidelines, a tension that originates with the construction of guidelines as a representation of scientific certainty. A reconstruction of the controversy in which expert uncertainty is portrayed as problematic, as in the Stewart report, changes the context and content of scientific advice. Uncertainties that were once considered slight and/or manageable can resurface to undermine previously authoritative science-based advice.

We have seen in this chapter that there is no true level of scientific uncertainty to be revealed through study. Just as scientific knowledge is constructed and compiled in a context, so uncertainty is constructed as the product of negotiations that involve broader concerns and fluctuating decision stakes.[49] Uncertainty is constructed through engagement, and non-experts have played an important part in prizing open gaps in knowledge, but expert assessment adds authority to constructions of uncertainty and their implications. I asked one member of the IEGMP committee why he disagreed that there was such a thing as a mobile phone health *debate*. His response illustrates perfectly the move to a new advisory consensus: from a state of *adequate knowledge* (cf. Campbell 1985) to a state of *workable uncertainty*:

> "I think if you go far enough out to the fringes, that would be possibly true, but I think the general consensus would be that there are uncertainties about the health consequences of mobile phone technology. People might vary in the emphasis that they put on the importance of those uncertainties, or what those uncertainties might be, but I think one can always, of course, find people who hold very strong and extreme views, which are not necessarily well informed by the science, and that's true on both [sides]. I'm not suggesting that that's less true one side than the other, but I think, if one looks away from the edges, I'm not sure whether there's so much a debate as a consensus that more research and more information is needed."
> (Interview transcript, No. 20)

This interviewee, who had little experience of the issue before his involvement with the IEGMP, sees a level of scientific uncertainty (with emphasis on *scientific*) at a

---

[49] My account would suggest that the model of Post-Normal Science (e.g. Funtowicz and Ravetz 1992) as represented by independent variables of uncertainty and decision stakes might be limited (cf. Yearley 2000; Jasanoff and Wynne 1998). It seems as though decision stakes and uncertainty can be co-constructed as an issue gains political importance.

policy-relevant level in the light of his involvement with the public science of mobile phone health effects. Conversations with other IEGMP members revealed a similar confidence that scientific uncertainty was under their control; that research was in place to fill in the relevant gaps.

But the uncertainty considered important by the IEGMP was a trickier beast than that once guarded by the NRPB. In the IEGMP's efforts to address broader concerns, it became apparent that public science knew very little about non-thermal effects, very little about genetic variations in EMF susceptibilities between people and very little about the effects of long-term, low-level microwave exposure. The only thing, it seemed, that science was certain about was the heating effect of microwaves that was already formalised in guidelines.

Uncertainty had moved beyond the control of whatever 'core-set' might originally have been charged with resolving this particular scientific controversy (I refer the reader back to the section on core-sets in chapter two). Indeed, at the end of my research, I still have no idea who the core-set might be. Certainly the interviewee above was not "technically informed in the science of mobile phones". Knowledge and advice about mobile phone risks might once have been the responsibility of a community of bioelectromagnetics scientists, or the NRPB. But we have seen in this chapter how rapidly previous attempts at closure of a controversy lost public credibility and required the intervention of more general and more 'independent' experts. This independence was partly required to distance decision-making from clear vested interests, but it was also required to distance decision-making from the previous core-set's (in so far as they existed) opinion on the adequacy of knowledge.

In attempting to regain social control of scientific uncertainty, the IEGMP played an active role in reconstructing both a body of relevant knowledge and the rules for public engagement. In engaging with non-experts, the IEGMP both responded to and shaped public concern. If we try to fit the work of the Stewart report into the framework supplied by Daniel Fiorino (1990) (see Chapter two), it is difficult to separate the *instrumental* elements of public engagement from the *substantive*. Non-experts did substantively contribute to a re-framing of the debate (a topic that will be discussed further in the next chapter). But the IEGMP, driven by a chair with significant political initiative, was fully aware of the instrumental benefits of being seen to be responding to concern, especially at a time of such low public trust.

A more compelling view of the role of the IEGMP and the public in reconstructing advice and uncertainties is that the IEGMP enrolled the public (and, similarly, were enrolled by the public) (cf. Latour 1987). 'Public concern' was therefore constructed alongside 'public science'. Control of the issues is negotiated as both develop. Just as non-experts demand a modicum of control over the relevance of scientific advice and the appreciation of non-scientific factors, so experts aim to control uncertainties, constructing areas of what-we-don't-know as largely expert territory. As they accept new uncertainties, the public are shuffled away from the contested ground of uncertainty.

This leads to a conclusion that public engagement, rather than questioning expert dominance of an issue, can also assert it. A credible piece of scientific advice, in which uncertainties are broadened, but controlled, is also an attempt to define and re-shape what the public are concerned about. We saw above how arguments over the relevance of precautionary approaches carry constructions of public concern and public reaction. Similarly, many of the opinions that have been expressed in calls for new research or better planning consultation have been based on claims to know what keeps non-experts awake at night.

The uncertainty constructed by the IEGMP, and the call for broader consideration of mobile phones science-in-context opened the door to all sorts of claims that might, in a more strict advisory environment, have been rejected as irrelevant. The next chapter focuses on how the diverse constructions of one such body of claims has contributed to the shaping of this issue. In this context of public engagement with scientific and policy uncertainty, one feature emerged as peculiarly salient. Many of the negotiations that took place seemed to revolve around the merits or failings of 'anecdotal evidence'. This term, as suggested by the title of the next chapter, is my next focus.

6 Anecdotal Evidence

an anecdote about anecdotes

At the Public IEGMP meeting in Liverpool, a number of attendees described their individual circumstances to the members of the committee... "I came here to tell you that I've suffered side effects from short-term use of a mobile phone... Essentially, I thought I was a one-off and I made some enquiries and it turned out that I'm not. I mean, I don't use one any more, I like to think that I'm relatively back to normal, but after the enquiries I made, I found quite a few people that suffered, like, ringing in the ears, headaches and..." On prompting from Sir William Stewart, this man continued describing his symptoms. Another member of the committee, David Coggon, suggested that the man might take part in a study to assess differences in sensitivity among some people. Throughout the meeting, the committee emphasised that they would welcome forms of evidence that might not be scientific (although in this meeting, members of the IEGMP did not use the word 'anecdotal').

Another member of the audience made herself known and began to criticise the independence of the NRPB (see chapter four). Her involvement in the issue had been prompted by personal experience of the dangers of mobile phones: "In July of this year, my husband was diagnosed with brain cancer. Who is gathering all this anecdotal evidence? Because it seems to me until people start gathering this in some proper scientific evidence, various people for various reasons will continue to talk about anecdotal evidence, and so wriggle out of it."[1]

My previous two chapters have relayed the narrative of scientific advice that has led to a subtle reconstruction of the issue of mobile phone health risks. With the emergence of a public controversy, the roles of scientific knowledge and public engagement have been concurrently redefined. In the previous chapters, I have mentioned that much of the evidence for the health effects of mobile phones might be considered 'anecdotal'. Public engagement with expertise has largely been shaped by individual reports of harm, attributed to mobile phones or their base stations. It is the purpose of this chapter to explore the notion of 'anecdotal evidence' in detail: how it is represented by experts, how it might be used in science and what it might

[1] Source: Transcript of IEGMP public meeting, Liverpool, 9th December 1999

tell us about public science, where experts and non-experts negotiate knowledge and ongoing research.

There exists a scientistic discourse of anecdotal evidence in which science exists partly as a means of escaping the frequently erroneous stories that emerge from individual, unregulated inquiry. In an attack on the prevalence of pseudoscience, Michael Shermer, director of the Skeptics Society, argues that…

> "**Anecdotes Do Not Make a Science**… Without corroborative evidence from other sources, or physical proof of some sort, ten anecdotes are no better than one, and a hundred anecdotes are no better than ten. Anecdotes are told by fallible human storytellers… What we need are properly controlled experiments, not anecdotes" (Shermer 1997, p. 48)

This view occurs frequently in the rhetoric of popular science, which paradoxically often uses stories to convey the message of scientific achievement, rather than allowing science to speak for itself. Discussing the role of stories in science and science in stories, Jon Turney notes that public science questions how science and stories might fit together:

> "The boundaries of science are increasingly blurred, according to some, while others seek to enforce a more clear-cut distinction – to devalue 'stories' and restrict discussion to 'facts'." (Turney 1998, p. 201)

'Stories' might be commonplace in normal scientific practice, a lesson learned from the history of early modernity (e.g. Dear 1991; Shapin and Schaffer 1985). But efforts to forward a scientific discourse in public tend to provide a harder attitude to anecdotal evidence. The scientistic construction of anecdotes appreciates how powerful they can be. But this power is seen as supporting fringe scientific or non-scientific thought. With this view of anecdotes, it is the responsibility of experts to emphasise to an impressionable public that anecdotes, rather than representing truth, hinder scientific progress towards it. [2]

---

[2] As with many features of science-in-public, the use of expert evidence in the courts provides an analogy. Jasanoff notes that, in American courts, the modernisation of acceptable scientific evidence has reduced the status of individual claimants. With the increase in the number of class-action suits, individual testimony is subordinated to statistics and probabilities (Jasanoff 2002).

Robert Park, a self-professed debunker of fraudulent and foolish scientific claims (including the claim that powerlines cause cancer (Park 2000, Ch. 7)), sees a reliance on anecdotal evidence as one of the 'Seven warning signs of bogus science'.

> "If modern science has learned anything in the past century, it is to distrust anecdotal evidence. Because anecdotes have a very strong emotional impact, they serve to keep superstitious beliefs alive in an age of science. The most important discovery of modern medicine is not vaccines or antibiotics, it is the randomized double-blind test, by means of which we know what works and what doesn't. Contrary to the saying, "data" is not the plural of "anecdote"." (Park 2003, p. 20)

Such comments harbour a very firm construction of anecdotal evidence, its impact in the public sphere and what constitutes 'proper' science. These constructions fit with a general trend towards 'evidence-based' practices, which emerged as a way of representing the 'science-based' response to conditions with myriad available solutions. Anecdotal evidence lies at (or falls off) the bottom of the hierarchies of evidence that are used to assess the best practice, carrying the perception that it is a tool of 'alternative' treatments or quack therapists (see, e.g. Evans 2003).

To many, 'anecdotal evidence' is antithetical to science. But it is not clear what anecdotal evidence is, nor what role it plays in science, politics or public science. This chapter investigates how the status of anecdotal evidence is instrumentally constructed and its meanings are solidified. It aims to contribute to an important and growing literature on knowledge traditionally considered 'inexpert', but it limits its analysis to just one term.

I began my investigation of 'anecdotal evidence' in the mobile phones controversy with no presumptions about its definition or status, other than a suspicion that experts might consider it firstly 'unscientific' and secondly easy to ignore, that is without great epistemological or rhetorical power. I aim in this chapter to question these presumptions by providing a spectrum of definitions and meanings of the term. This indicates how it is considered by different actors and how it might therefore contribute to the construction of public science issues. I look at how the contested role that anecdotal evidence is given in science shapes, and is shaped by, the public context of the debate over mobile phone risks. And I describe, more generally, why anecdotal evidence might have been sufficiently persuasive to find itself defining the social and scientific context of a controversy. My conclusions reflect on what this analysis of a single term can tell us about the constructions of science in public, of public engagement with science and of the permeable boundaries that might separate these two domains.

Defining and Unwrapping Anecdotal Evidence

The term 'anecdotal evidence' has definitions, but these say little about its meaning. It might seem egregious to leave it this late to provide a definition for the term that is central to my thesis, but we will see in this chapter how the flexibility of the term allows actors to define it in a highly personal and contextual way. By this stage, readers may well have constructed their own working definition of 'anecdotal evidence'. It might include some of the following elements: individual, unscientific, unreliable, unreplicated, unreplicatable, story-based. The word 'anecdote' is derived from the Greek word for 'unpublished', which might suggest its innate opposition to the scientific principle of peer-review. Such elements are important in beginning to consider the term, and they are certainly all relevant, albeit with different groups accenting different elements. However, the only fixed definition that will be offered by this chapter emerges from constructions of what (or who) 'anecdotal evidence' comes to stand for in the case of mobile phone risks.

During interviews, the first question I asked on the topic of anecdotal evidence, unless it had been discussed in reference to other issues, was prompted by one of the recommendations from the Stewart report (see chapter five):

> **1.70** "We recommend that in a rapidly emerging field such as mobile phone technology where there is little peer-reviewed evidence on which to base advice, the totality of the information available, *including non-peer-reviewed data and anecdotal evidence*, be taken into account when advice is proffered" (my emphasis)

Before continuing, we should consider what definition of the term is implied with its inclusion in this recommendation. Firstly, anecdotal evidence is differentiated from peer-reviewed evidence. This seems straightforward. Secondly, it is differentiated from *data,* even data that has not (yet) passed the test of scientific acceptance. Thirdly, its inclusion in a recommendation suggests that, normally, anecdotal evidence is not taken into account when advice is proffered. This chapter aims to illuminate the above recommendation by deepening an understanding of anecdotal evidence in and around the Stewart report. This should provide a sense of what was being considered by the committee when they considered the term sufficiently important for inclusion.

I used recommendation **1.70** as a prompt for interview questions about anecdotal evidence. The following responses may therefore be prejudiced by views about this recommendation, or the Stewart report more generally. But the excerpts repeated below serve to define the term in the context of public scientific advice, rather than arbitrarily. They are informed by actors' experiences of evidence, science and the public.

Indeed, I argue in this chapter that, by tracing the usage of the term, constructions of 'anecdotal evidence' act as a reliable touchstone not only for opinions on mobile phone safety, but also for opinions on the relationship between expertise and the public.

What is anecdotal evidence?

One scientist, when asked what he considered anecdotal evidence to be, replied, "We all know what it is, but to define it..." In trying to construct a definition, he mentioned the following: "individual studies," "random, individual claims without any control data." He also noted that he would "almost consider some of the scientific literature as anecdotal" (Interview notes, No. 14). The final point is one to which we will return later in this chapter. For the time being, we can see that, as a laboratory scientist, he gave the term meaning in the context of his own work.

Responses from other actors typify a view that was commonly held, by committee members, scientists and interest groups alike, that anecdotal evidence is defined by its origins. An interest group representative defined 'anecdotal evidence' thus:

> "I mean it would be reports of people like me that would speak to [people] reporting things that... may or may not be due to phones or masts, but they believe they are" (Interview, No. 3)

Scientists tended to refer to the origins of anecdotal evidence with members of the public, or their doctors:

> "So for example, suppose for the sake of argument, this isn't true, but I'm trying to give an extreme example. Suppose it suddenly turned out that lots of doctors were phoning up the Department of Health from all over the country and saying "I'm seeing a lot of acoustic neuromas. It seems to be the case that nearly all these new cases that I'm saying seem to be coming from people who've been using a GSM phone for the last 2 or 3 years." That's anecdotal evidence. In no way is that rigorous epidemiological evidence of an association between phone use and acoustic neuroma." (Interview transcript, No. 20)

> "Well, that will be people saying, oh, I use my mobile phone and I get a headache." (Interview, No. 21)

> "It's precisely like those people who claim to be electrosensitive" (Interview transcript, No. 7)

These comments reveal two main groups of people whose evidence scientists consider to be 'anecdotal'. Firstly, those who report chronic, externally observable disease from mobile phones use, such as brain tumours and attribute them to mobile phones. And secondly, a supposedly susceptible group of people, some of whom 'claim to be electrosensitive',[3] reporting symptoms which suggest a different reaction to EMFs from that of the general population. Electrosensitivity (an illness whose aetiology and existence are highly contested) demands further consideration at a later date, in another piece of work. For now, however, we should understand that the label, used more by sufferers than by scientists, acts as a condensation point for many constructions of what 'anecdotal evidence' might mean with regards to mobile phones. Electrosensitivity has emerged as a collection of symptoms with scant (peer-reviewed) scientific support, relying instead on reported symptoms from sufferers and case reports from doctors.[4]

---

[3] As with many contested illnesses, the name is itself contested. Electrosensitivity is sometimes known as electromagnetic/electrical (hyper)sensitivity or radiofrequency/microwave sickness.

[4] Electrosensitivity is less widely accepted and studied in the UK than it is in the rest of Europe. In Sweden especially, the illness has received a great deal of attention, with the creation of the FEB (The Association for the Electrically and VDT-injured) – See www.feb.se. The issue of electrosensitivity took a surprising turn in 2002, when the then-director general of the World Health Organisation, Gro Brundtland (once Norway's Prime Minister) announced that she had developed a sensitivity to mobile phones.

Members of the Independent Expert Group on Mobile Phones who I interviewed unsurprisingly discussed anecdotal evidence in the context of recommendation **1.70** from the Stewart report. Their responses to the question, "What did the group mean by 'anecdotal evidence'?" tended to take the form of justifying the inclusion of the term in their report. One member described how the chairman had "very good antenna for what would satisfy the interest groups." He claimed that the term was included in the report as a way to placate the people with "very strange ideas". "To ignore them would have been foolish" (Interview notes, No. 5). The insights into the purposes of scientific advice are enlightening, and will be explored later, but these comments also reveal an implicit definition of anecdotal evidence. Anecdotal evidence is defined by this interviewee in reference to an 'other', a theme discussed below.

It is clear, therefore, that we cannot remove the term from its context in attempting to offer a definition. 'Anecdotal evidence' does not exist as an interesting idea outside perceptions of its source or status. The only firm conclusion we can draw is that 'anecdotal evidence' *is* whatever 'anecdotal evidence' *means* to different actors and different groups. As Peter Winch has said, "to give an account of the meaning of a word is to describe how it is used; and to describe how it is used is to describe the social intercourse into which it enters" (Winch 1958, p. 123).

The meanings an interviewee ascribes to the term reflect not only the position of the interviewee in the debate, but also the views they hold about 'science' and its relationship with policy and the public. Because the term was discussed in the context of scientific advice, interviewees provided opinions on both its epistemological and political relevance. This relevance is reflected in much of the broader discourse that has contributed to the shaping of the debate.

This project studies the social nature of scientific expertise. The crucial question we must therefore ask is "How do experts interpret and deal with anecdotal evidence?" We must consider how it is constructed and when, how and why it is accepted or rejected. As an entry point to the juxtaposition of anecdotal evidence and science, I asked all of my interviewees what part they considered anecdotal evidence to play in science.

The Epistemological Status of Anecdotal Evidence

Anecdotal evidence has provided my focus as a traditionally inexpert form of knowledge, and my analysis illuminates how it might relate to science and the political dimensions of a public science debate. My conclusions will describe the hybrid political and epistemological meanings of anecdotal evidence, so it might seem perverse to consider assessments of its epistemological worth separately. However, this section provides a framework, moving up a ladder of epistemological status, for the consideration of all elements of the construction of anecdotal evidence. At each rung, I aim to demonstrate the meanings bestowed upon 'anecdotal evidence', and the contested territory it helps to define. Constructions of the term are pleasingly multi-layered and so difficult to squeeze into a simple hierarchy. I aim to demonstrate the significance of different opinions, personal and institutional, of anecdotal evidence, on the construction of public science. I begin at the bottom.

1 Anecdotes as nonscience

> A: "Anecdotal evidence is whatever you feel… It's subjective. Anecdotal evidence is anything that's subjective."
>
> Q: "And what role do you think it has to play in science?"
>
> A: [long pause] "We always have to take into consideration anecdotal evidence, because we're dealing with human beings and people have feelings and thoughts and they have to be addressed." (Interview transcript, No. 27)

The above quote characterises the lowest epistemological status afforded to 'anecdotal evidence' that I came across. The term is used by this scientist to construct a non-scientific participant in an issue whose contributions need to be "taken into consideration", but not in any epistemological sense. 'Evidence' from non-scientists is reduced to 'feelings and thoughts'. As we shall see later, this conflation of anecdotes with public concern defines the politics of a crucial part of the politics of mobile phones science. At this level, 'anecdotal evidence' represents information to be ignored because it does not involve scientific rigour. The subjective non-scientists cannot see beyond their own experiences towards generalizable knowledge. An IEGMP member has strong views on the problems with anecdotal evidence in public science. Having previously insisted that, "the existence of an individual with a brain tumour is not science" (Interview notes, No. 5), he went on to describe the role of science in such situations:

> "That's why you have science. It's to avoid anecdotes. It may be sensible in sociology, but if you want to know whether X causes Y, the fact you can find a person who has X and Y isn't evidence." (Interview transcript, No.5)

This treatment of anecdotal evidence protects science from non-science (including sociology). Non-scientists are making claims (however weak) to know about phenomena, but this 'evidence' is seen as no evidence at all, possessing no cognitive merit. This conception sits well as an example of boundary work (cf. Gieryn 1983, Jasanoff 1987). Science, in encountering a hybrid domain in which it is queried by policy and the public, uses a construction of anecdotal evidence to define what science is, or rather what science *isn't*. Science *isn't* subjective, it *isn't* individual, it *isn't* localised to a particular health scare and it *isn't* presented by non-scientists. In more social theoretical terms, anecdotal evidence is an 'other' used to define the normality of science and the abnormality of taking non-scientific evidence into consideration. The *otherness* of anecdotes, especially for the latter scientist, reinforces opinions about the purposes and practices of science-in-public. For scientists with experience of public engagement, as we will see later, anecdotal evidence represents a certain group of people, judgements about whom may not be easily separated from judgements about their evidence.

For interest groups and activists who criticise existing regulation, the rejection of anecdotal evidence represents a rejection of valid knowledge on the unjustifiable grounds that it does not come from the right source. As we saw in chapter four, existing science embodied by industry and the NRPB was constructed as aloof and defensive. Anecdotal evidence, via its perceived rejection, was instrumentally constructed by non-scientists. So the boundary of monolithic 'science' is also reinforced by non-scientists, a point we should bear in mind when looking at the dynamics of any complex public science debate. Disputes over anecdotal evidence are used by outsiders to construct the *otherness* (and irrelevance) of science (cf. Michael 1996). Ideas of what it means to be 'scientific' (in this case by rejecting anecdotes) in public did not differ hugely between experts and non-experts. The problem was that opinions differed as to whether science was sufficient for good policy. This theme will be explored further when we look at criticisms of the NRPB and their attitude towards inexpert evidence.

The image of 'anecdotes as nonscience' serves a purpose for all actors. It polarises the debate, sharpens distinctions and allows either side to construct a target. But it does not fairly reflect the more considered opinions most of the scientists I interviewed had of anecdotal evidence. The mobile phones health debate was the stage for negotiations between experts and non-experts, including negotiation of the role of different classes of evidence. As such, the more interesting contested territory is revealed when anecdotes are viewed alongside science, rather than in simple opposition. And in these cases, explanations based around boundary-work may not be so helpful.

2 Anecdotes as providers of hypotheses

The views of the first scientist quoted above are not fairly represented by the excerpt that I used. This interviewee appreciated that the unscientific nature of anecdotes need not preclude their role in the construction of good scientific experiments. The discussion here had turned to provocation studies, designed to test whether electrosensitivity was a real, objectively observable phenomenon:

> "Science isn't based on anecdotal evidence. Scientists always look at anecdotal evidence to form hypotheses, and when they test it, there's the finished research. Are these people really hypersensitive to radiofrequencies? Conclusion, no." (Interview transcript, No. 27)

This view is closer to the status most usually bestowed on anecdotes: that they provide a target for research. Even without judgements as to the reliability of evidence, this might fulfil scientists' and regulators' desire to address current concerns, contributing a scientific voice to the construction of public debate. For scientists looking for productive research topics or a new source of funding, anecdotal evidence can provide an interesting question to be answered scientifically. Scientists might well feel a responsibility to respond to suggestions of poorly-understood effects.

As an aid to research, one laboratory scientist likened anecdotal evidence to being shown where to look for a needle in a haystack (Interview notes, No. 14). An

IEGMP member agreed that deciding upon research was a key problem, and that anecdotal evidence could help:

> I don't think, again, you can generalise. In epidemiology, for example, it's probably the starting point of a lot of epidemiological studies, because often epidemiological studies may be set up on the basis of some sort of weight of… anecdotal evidence. In my area… I'm not sure that it plays a particularly strong role at all, but… the basic rule in science is that it's extremely difficult to come up with rules about how to generate hypotheses that you're going to test experimentally. It's not even clear when one does many experiments that you're testing specific hypotheses, so I think scientists can get their ideas from anywhere, absolutely anywhere. *The question is not where your ideas come from, it's the quality and the rigour with which they are subjected to empirical assessment.* (Interview transcript, No. 20, my emphasis)

Again, anecdotal evidence is placed firmly outside science. Science, for this interviewee, might be inspired by anecdotal evidence, but the purpose of science, and the source of its authority, lies in the formation of robust, non-anecdotal knowledge:

> "Well I think, yeah, I mean, it acts as a pointer to decide what research you would design round that to investigate it. And I don't think it should be any more than that" (Interview transcript, No. 2).

Some scientists, rather than considering anecdotal evidence as a pointer for *productive* research, took it more seriously as a precursor for a public health problem, and therefore a justification for more *relevant* research. A lab scientist took the view that anecdotal evidence could well represent the 'tip of the iceberg'. In this next excerpt, he is discussing the widely-publicised claim that radiation from mobile phones promotes brain tumours:

> "Again, it may be coincidental. People might even be selective about their memories and which side they use their mobile phone 'oh it must have been that side, because that's the side the tumour's come up on,' but if one just dismisses that evidence as of no value, because it's purely anecdotal… there is also a possibility that that might be the tip of the iceberg, and there might actually be a lot of other cases where there might be some link between mobile phone use and, whether it's a tumour or some other condition… I think it's a matter of not ignoring those groups simply because they're not part of a replicated scientific study." (Interview transcript, No. 7)

Scientists realised that anecdotal evidence has potential epistemological power. In contested public science, the 'tip of the iceberg', if ignored, can be dangerous or embarrassing. Asked to define anecdotal evidence, this scientist from the NRPB turned to its problems and its worth to scientific inquiry:

> "Well, that will be people saying, oh, I use my mobile phone and I get a headache. There are people who say all sorts of claims about how mobile phone masts affected [them and their] children – there are children with learning difficulties due to a mobile phone mast being put up. So that's the anecdote. Now, it is a very difficult area, because the anecdote – one should never dismiss the anecdote – because there are some famous ones, like Jenner travelling the country and going to the west of England and being told by a farmer, my cowgirls never get smallpox. Cowgirls never do, and he thought, 'bloody hell', you know, but it was the scientist who twigged. Now that anecdote might have been around, but it took a prepared mind to see the importance of it." (Interview transcript, No. 21)

These scientists accept the status of anecdotal evidence as suggestive of "a possibility" and therefore an opportunity for research to help support or reject such claims. However, it is again implicit that without scientific intervention, anecdotal evidence can say nothing about an issue. As the last scientist puts it, the assessment of veracity is the job of science ('a prepared mind') and science alone.

Anecdotes and (popular) epidemiology

The last comment on the interaction between Jenner and the anecdotes that led to the smallpox vaccine leads us to an important discussion about the role of non-experts in the production of knowledge about public health. Phil Brown's explanations of popular epidemiology (see chapter two, (Brown 1987, 1992, 1997)) note that the public can see modern epidemiology as irrelevant to their problems. As he puts it, epidemiology is "often more concerned with protecting the increasingly rigid standards of scientific procedures than with safeguarding public health" (Brown 1997, p. 137).[5] As Epstein would have it, there is a struggle between purity

[5] At a workshop involving ES sufferers, interest groups and scientists, my findings on the expert treatment of anecdotal evidence were responded to by one physicist whose work involved the claimed link between powerlines and childhood cancer. He argued that all good science should start with anecdotal evidence, but that this curiosity was being discouraged in scientists' education. (Royal Society of Medicine, 17th September 2003)

in science and relevance (Epstein 1996). Popular epidemiology represents an attempt to get back to the style of investigation (often called 'shoe-leather' epidemiology after its most commonly-exhausted resource) that Jenner and John Snow had pioneered in the 18th and 19th centuries, respectively.[6]

Within popular epidemiology, therefore, we should not see public engagement as just non-expert knowledge contribution. Popular epidemiology emphasises the social features of scientific controversies, implying political and judicial remedies in the process (ibid., p. 139). Taking anecdotal evidence as a variation on popular epidemiology, we are reminded that anecdotes are politically-laden. Anecdotal evidence is not just single pieces of data. It is evidence *for* something. Anecdotal evidence makes claims not just to know (in a modest way) about a condition and its causality, it also makes claims about knowledge and politics. In particular, anecdotal evidence claims that science does not know about something, and that the appreciation of non-experts is necessary to understanding a public science issue.

3 Anecdotes as complementary/alternative knowledge

"Anecdotal Evidence: Ignore it at Your Peril!"

This quote comes from the web site of Microshield[7], a product that is claimed to reduce the amount of RF radiation absorbed by the brain. Les Wilson, the inventor of the Microshield was asked to give oral evidence to the IEGMP:

> "People contacting Microshield had reported a number of symptoms. Whilst these had not initially been documented they had now compiled a database of symptoms reported by over 2000 people. However, he complained that he had been unable to

[6] A recent article (McLeod 2000) has re-analysed the biography of John Snow (often credited as the founder of modern epidemiology). Snow is remembered partly for a story ("an appealing tale because it is short, dramatic and heroic" (ibid., p. 923)) in which he (or someone else) removed the handle of a water pump on Broad Street (now Broadwick Street) in Soho, central London. This ended the local cholera epidemic, which Snow had attributed, by plotting deaths on a map, to the single water supply. McLeod's appraisal argues that the map and the pump were perhaps less important than they are made out to be, but Snow remains the father of finding out about disease at ground level.

[7] Microshield 'Symptoms' page, http://www.microshield.co.uk/~symptoms.html, accessed 23rd September 2003

find anyone who could make use of these data." (IEGMP Summary of Evidence, Les Wilson, Microshield PLC)[8]

The symptoms reported to Microshield are similar to those reported by people who claim to be electrosensitive. Such symptoms are not explained by science, so, it is claimed, anecdotal evidence should be used to assess what might cause them. For groups who feel disempowered by 'science-based' regulation, constructing anecdotal evidence in this way is an effective way of strengthening a position as not just a representative of alternative concerns, but also of a distinct type of expertise. We have seen above that scientists might regard anecdotal evidence as suggesting a potential public health problem. The view epitomised by Microshield's comments begins to suggest that anecdotal evidence might be afforded a status *because* of, rather than despite, its source outside expertise.

Two features that were described in the previous chapters are salient here. Firstly, mobile phone usage is almost ubiquitous. Secondly, the technology is new and, (most) scientists claim that there are significant uncertainties about the biological impact of such technologies. For opponents of a regulatory system that they see as unreasonably skewed towards 'sound science', ubiquity and uncertainty allow for persuasive arguments in favour of taking anecdotal evidence more seriously. Gerard Hyland, a firm critic of the regulatory consensus, has used this tactic to support a greater awareness of effects which are poorly explained by orthodox science (but might be explained by his theories). His exchange with the COST 281 committee, who control an ongoing EU project looking at mobile phone safety, clearly demonstrates the contested territory within regulatory science. (As we will see later when we return to the NRPB, the rejection of anecdotal evidence had become a feature of non-expert unease):

> "It should be stressed that the anecdotal nature of many of the reported health problems – such as headache, sleep disruption, impairment of short term memory, nose bleeds and, more seriously, an increase in the frequency of seizures in some children already suffering from epilepsy - does not constitute grounds for dismissing them out of hand, as is so often advocated. For given the paucity, to date, of

---

[8] The IEGMP responded to Les Wilson's evidence: "The group were interested in Mr Wilson's assertion that the use of shields was not supported by industry because it implied that a danger existed." (Minutes of 8th Meeting, IEGMP)

systematic epidemiological studies pertaining to this relatively recently introduced technology, such reports are an indispensable source of information – a point acknowledged in last year's Report of the UK Commons' Select Committee."[9]

The response from the COST 281 steering committee:

- "Anecdotal reports do not meet the minimal requirements for scientific data.
- If documented at all, anecdotal reports suffer from risk perception bias and individual conditioning.
- It is agreed within the scientific community as represented by their international bodies, that by their nature anecdotal results cannot be the basis for decisions on causal relations.
- Therefore, it is misleading to claim that they were accepted as an indispensable source of information. However, they can be and are motivation for well designed scientific investigations, which have so far failed to show a causal relationship between health symptoms and environmental EMF exposure."[10]

This comment represents an attempt to formalise a standard of acceptable evidence, and to place anecdotal evidence beneath it. Anecdotes, 'by their nature' are placed on the edge of science, and outside the scope of decision-making (science-based regulation). Although 'their nature' is not explained further, it is clear that the COST 281 committee have a firm idea of what anecdotes are and where they come from. Dissenting scientists such as Gerard Hyland have an equally clear (but different) idea of what anecdotal evidence is, what it stands for, and crucially what its rejection says about the organisations who control regulation and research. Hyland's response is interesting, because it illustrates the flexibility of anecdotal evidence in making political and scientific arguments:

> "**Section 2.6. Use of non-scientific information:** Here, I am criticised for saying that 'anecdotal reports are an indispensable source of information.' Contrary to what they say, nowhere do I claim that such reports are 'accepted'. My original comments simply reflected those of (*i*) the UK Commons' Select Committee, that such reports

[9] European Parliament, Directorate General for Research-Directorate A, STOA - Scientific and Technological Options Assessment, Options Brief and Executive Summary PE nr. 297.574, March 2001

[10] 'Scientific Comment on Individuals Statements of Concern About Health Hazards of Weak EMF' www.cost281.org/activities/hyland_comment_final23-11-2001.doc

are an indispensable source of information that can 'usefully serve to target further research', and (*ii*) the conclusions of the Royal Society of Canada that there is already sufficient anecdotal evidence (of problems of a neurological nature) to justify further research."[11]

Hyland has retreated to a more orthodox, and more easily defended description of the power of anecdotal evidence. In doing so, he has sacrificed insights into the social aspects of its rejection by experts. This is a pattern which we will come back to in this chapter's conclusion.

The House of Commons Science and Technology Select Committee had, in 1999, publicly accepted a role for anecdotal evidence, justified by a lack of scientific knowledge about health effects. Setting the scene for the IEGMP, the report noted the many criticisms of institutional rejection of anecdotal evidence, such as those targeted at the NRPB (see below) and responded:

> Paragraph 36: Anecdotal evidence can, however, usefully serve to target further scientific research. We agree with the Royal Society of Canada that the evidence for neurological problems reportedly caused by mobile phones, including symptoms such as headache, nausea, tiredness, sleep problems and memory loss, is unclear but there is sufficient anecdotal evidence and uncertainty to justify further research.

The IEGMP reinforced this view in recommendation **1.70** (reproduced at the start of this chapter). As IEGMP members explained:

> "Yeah, it was because we were interested in the totality of the evidence, the views, whether peer-reviewed or not. And then it's up to us to analyse what some of the stuff is saying." (Interview transcript, No. 30)

> "'Anecdotal' is often used pejoratively. We didn't mean it pejoratively." (Interview transcript, No. 20)

...which would suggest that the inclusion of the term was an attempt to move away from a scientistic rejection of alternative evidence. However, some interviewees disagreed with the inclusion of the recommendation, and disputed its purpose. One IEGMP member doubted that the recommendation should be treated with the same

[11] Response to COST281's 'Scientific Comment on Individuals Statements of Concern About Health Hazards of Weak EMF' www.cost281.org/activities/HylandResponsetoCOST281.doc

seriousness as others in the report. It served political rather than rational ends. The previous chapter looked at the Stewart report as a political and scientific review, and the treatment of anecdotal evidence reveals similar patterns. Disagreements about the inclusion of anecdotal evidence get to the heart of the problems of hybrid scientific advice, constructing an issue that is at once scientific and public. This theme is crucial, and will be discussed in depth in my concluding chapter, but it is worth noting at this stage the centrality of opinions on anecdotal evidence to the construction of this issue.

So why the need to redress the balance with the inclusion of 'politicised' recommendations supporting alternative contributions to knowledge? A contributing factor is the political battle that had occurred between the public and the scientific establishment, represented in the UK by the NRPB. While small by the standards of many scientific controversies, discussions between engaged publics and scientific authority defined the recent history of the controversy. Their focus was often on the contested status of anecdotal evidence within expertise.

Anecdotal evidence, the NRPB and the IEGMP

The tension between 'anecdotes as unscientific' and 'anecdotes as complementary' characterises much of the history of the debate. Prior to the Stewart report, for opponents who doubted mobile phone safety, the NRPB had become a caricature as a monolithic protector of science and a rejecter of important evidence. The NRPB, as I described in chapter four had come to represent an institutional 'thermal consensus', although beneath the surface there was significantly greater appreciation of the inherent uncertainties. As the public debate expanded, and scientists continued to provide evidence, of varying standards, of non-thermal biological effects, the NRPB was perceived by many people as protecting this consensus, and its status as an expert authority, without an open mind. Many activists and members of the public who had approached the NRPB felt that their concerns had not been taken seriously, and often that the evidence they had presented for the NRPB's perusal had been rejected.

Chapter four considered the NRPB's 'discourse of compliance', in which the only legitimate ground for challenge was whether a technology complied with existing guidelines. Claims that guidelines were insufficient to guarantee safety were treated defensively or ignored. Anecdotal evidence, by its nature, claims effects that should not occur beneath the guideline levels. It therefore asks questions that the discourse of compliance is not happy to answer. The struggle between the NRPB and non-experts about the definition and usefulness of anecdotes allows us to further explore the context of evidence. One scientist, in criticising the Stewart report's conclusions, explained that the reason the IEGMP felt the need to address the question of anecdotal evidence was the NRPB's inadequate communication with the public, which had led to problems of trust:

> "...you can't say to somebody who says 'I'm worried I've got cancer', 'we're operating to all the guideline limits so you have no worries,' you have to say, 'what's on your mind, what's bothering you, tell me, and I'll tell you exactly what the research in that area is, and I'll explain exactly what you're worried about and we'll deal with it.' ...instead of addressing people's concerns, they say 'wait a minute, we've got guideline limits we're operating with, end of story.' The NRPB's been doing that for a long time." (Interview transcript, No. 27)

This interviewee, along with many others from very different perspectives, criticises the NRPB for its reliance on compliance. But what this scientist refers to as 'concerns' (such as the presence of cancer) might also be seen as anecdotal evidence. 'Ignoring public concerns' follows the pattern that other actors in the debate have considered to be 'rejecting anecdotal evidence'. This conflation of concern and evidence will be discussed later.

One advocate of research into electrosensitivity presented a memo to the Science and Technology Select Committee in which she summarised the sentiments that many groups felt over the past behaviour of the NRPB:

> "The NRPB has failed miserably to recognise this condition when setting standards for exposure. Their position that there is "no scientific evidence" to back up the existence of Electrical Sensitivity is untenable due to the fact that they and other scientific bodies have not set up any kind of relevant research program."[12]

Scientific orthodoxy, with the NRPB as its public face, was perceived as unwelcoming of any information which might destabilise its existing consensus.

---

[12] Memorandum submitted by Ms Sarah J Scott, House of Commons Science and Technology Select Committee, 23 June 1999

Criticisms of orthodox science or policy often focussed on the importance of anecdotal evidence and its unjustified rejection. Many groups saw the NRPB as an institution which constructed the public as receivers of scientific advice, but not contributors. But, as I pointed out in chapter four, the NRPB, behind their advisory façade, were more appreciative of both scientific uncertainty and the role of anecdotal evidence. This NRPB scientist described his attitude to anecdotes:

> "We don't dismiss anecdotes, but they have to be low down because if I listed all the complaints that are supposed to be due to mobile phones and masts they would fill a medical dictionary. Everything – Cancer, Alzheimer's, MS, Aids, Cot Death – everything. But if there were consistent complaints to medics, to GPs, of hearing difficulties, or severe headaches after using mobile phones, we wouldn't dismiss them." (Interview transcript, No. 21)

Recommendation **1.70** from the Stewart report, which prompted my research, falls under the heading "National Radiological Protection Board" in the report's executive summary. This suggests that it was conceived as a direct response to criticisms of the NRPB over its unwarranted 'science-based' attitude. An IEGMP member explained their sentiments:

> "The great problem that the committee felt was that every time there was some evidence coming up of adverse effects, the first thing that the NRPB said was "was it... peer-reviewed?". Now, because it's not peer-reviewed doesn't mean to say it hasn't got some substance to it. And, in a rapidly emerging field, the number of peer-reviewed papers are likely to be small. And secondly, anecdotal evidence and non-peer reviewed evidence often leads to pointers about the type of research that needed to be done..." (Interview transcript, No. 30)

The NRPB, as the first point of contact for those interested in scientific advice on mobile phones, had come to embody the opposition. They represented 'science-based' regulation, which included a strong construction of what counts as science and what does not. One NRPB scientist explained to me the problem of maintaining scientific authority in a politicised, public context. This passage is from the beginning of an interview which had started with a discussion of a recent confrontation the scientist had had with a group of electrosensitive people:

> "We accept the symptoms are real, we believe that they are real, and then we say well, scientifically, we cannot help you, we can't do legislation, we can't write guidance

based upon what is largely, or totally, anecdotal evidence, a few individuals here and there." (Interview transcript, No. 32)

Members of the NRPB felt they had been placed in an impossible position. They were expected to carry out world-class scientific research, set guidelines for exposure and act as the public face of the Government's regulatory policy. As an organisation who feel they should only advise on science, the NRPB does not officially take anecdotal evidence into account.

The Stewart report's criticisms of the NRPB, prompted by public meetings and evidence from aggrieved actors, were compiled in the "Public Perceptions and concerns" chapter, drafted by the two lay members of the committee. A member of the committee explained to me the NRPB's image amongst those members of the public they had dealt with:

> A: "The public perception of the NRPB was not good. But then that is a very small percentage of the public, because most normal human beings [don't] know what the NRPB's about. But the few people… who've dealt with NRPB were not that impressed. And the letters that I saw…tended to be in the line of; 'how arrogant can you be, you are ignoring public perception, you are not consulting with the public, your committees don't have any lay people, et cetera et cetera.'… Yes, I think that the summary is that the public perception of the NRPB was that it was remote and arrogant.
>
> Q: What about your perception of it?
>
> A: "I think NRPB was the typical sort of group of people who are just so focussed and busy on what they're doing, and who believe that they are experts, that they lose track of what normal people are like, think like and what they want to know. I would think that NRPB thought, 'as long as we tell people that we're looking at it, they should just trust us.' Unfortunately, that might have been OK 20 years ago, but the public were saying, 'Government told us that BSE was OK'". (Interview transcript, No. 19)

The first paragraph of the above comment reminds us that the majority of the public were not sufficiently engaged in the debate to know who the NRPB were or what their attitude was. Antipathy towards the NRPB generally came from those whose concerns (or evidence) had been rejected. This is one sense in which anecdotal evidence has shaped public engagement with this issue.

An NRPB employee, responding to the criticisms within the Stewart report, said:

> "We get that sort of criticism. We weren't upset, if you like, by the Stewart Report... And sometimes people can be quite unreasonable, you know, the great British public, so I think that it's more that we are viewed as this institution that's a bit cold and in an ivory tower, do you see what I mean? I think that's what Stewart was saying, that you've got to come out a bit more on the, a bit more people-friendly instead of having all this, sort of, 'we're scientists and we know best.'" (Interview transcript, No. 21)

These comments highlight the political tone that anecdotal evidence had acquired as it was used in the context of the mobile phones controversy. In the context of the discourse of compliance, anecdotal evidence does not just represent bad evidence. It also represents a political game that the NRPB and other bodies were not willing to play. Engagement with the public regarding the kinds of questions posed by anecdotal evidence would not only have undermined an existing regulatory framework, it would have gone against the NRPB's role to provide advice based on the available science.

To the IEGMP, it was clear that anecdotal evidence was used to make claims about science and politics. To improve the credibility of advice, therefore, anecdotal evidence was nominally included in the group's deliberations. But much of the discussion of the NRPB and the IEGMP considers the importance of responding to 'public concern' rather than explicitly 'anecdotal evidence'. A feature of the expert treatment of anecdotal evidence in this controversy has been its transmogrification (in part, see conclusion) into public concern once it is taken heed of by experts. This subtle switch in discourse illuminates attempts to maintain scientific and public credibility in the hybrid domain of scientific advice (Irwin et al 1997, p.28). This conflation of evidence with concern acts to reduce the opportunity for constructive discussion, by denying the public epistemological contribution. Addressing public concern and taking into account anecdotal evidence in practice might be a very different problem. We therefore have to consider whether the inclusion of anecdotal evidence might change science at a deeper level. Hints at this are suggested by the second recommendation from the Stewart report in which anecdotal evidence was included:

> Paragraph 1.58 We recommend that a substantial research programme should operate under the aegis of a demonstrably independent panel. The aim should be to develop a programme of research related to health aspects of mobile phones and associated technologies. This should complement work sponsored by the EU and in other countries. In developing a research agenda the peer-reviewed scientific literature, non-peer reviewed papers and anecdotal evidence should be taken into account (paragraphs 5.270-5.272).

This recommendation led to the establishment of the Mobile Telephones Health Research Programme, funded jointly, although meagrely, by industry and government.

Anecdotal evidence in operation? - The MTHR

By the time the IEGMP had produced its report (May 2000), anecdotal evidence had begun to take on a stable construction in the debate. Among experts and non-experts, the term had begun to stand for people who attribute illness to their phone use, or people who live near a base station and complain about increased incidence of disease, or report symptoms. As a function of the conflation of evidence and concern discussed above, anecdotal evidence had also been constructed by expertise as an umbrella term for the fears, criticisms and ideologies of the engaged public.

The main contributing factor to the stabilisation of the construction of anecdotal evidence, and therefore of a group of people who are likely to provide it, is its inclusion in literature such as the reports from the House of Commons Select Committee IEGMP. The Stewart report set the agenda for global debate about mobile phone safety, and continued to do so through the public face of the MTHR programme (described in the previous chapter). The report recognised that many groups had felt aggrieved by institutions such as the NRPB dismissing anecdotal evidence. In reacting to this, the committee heard from groups who constructed their own experiences as anecdotal, often positively (I discuss this type of construction in more detail below). The inclusion of 'anecdotal evidence' in the recommendations made concrete a construction of anecdotal evidence that was positive, but controlled. It calmed the use of 'anecdotal evidence' as a claim against scientific orthodoxy, and it helped bring its purpose back under the epistemological umbrella (I will return to this point in the conclusion). The struggle for the definition and importance of anecdotal evidence was set to continue in the prescriptions of the MTHR programme, nominally independent from the NRPB but relying on much of the latter's expertise. Although the MTHR programme's lack of funds means that it does not have the required muscle to make a global scientific impact, it helps illustrate the public dimension of directed research into a public health issue.

I have described above how some see anecdotal evidence as important because of, rather than despite, its unscientific nature. With scientific ignorance, the most contested epistemological territory becomes the direction of future research. Even scientists with confidence in an existing consensus see gaps to be filled in. But interest groups saw current research as biased towards an increasingly irrelevant orthodoxy. One activist defined anecdotal evidence in the context of the planned research of the MTHR programme:

> "Well I would say that it should be the instigator of good peer-reviewed science. In other words, that should give you the clues for what to go out and look at… I think what the Government/industry-funded [programme] at the moment is doing is looking purposefully in the wrong directions. There's one or two things they're funding that are quite good. There's other things that they're actually being very awkward about and actually denaturing, so what will come out of it is a negative. It won't be one thing or the other, but it's not going to be concise, well it might be concise, *but it isn't useful science*" (Interview transcript, No. 3, my emphasis)

This attitude, taken by many scientists and activists, suggests that research into health effects is being guided by a desire to preserve the structures of what constitutes good science. In doing so, unscientific knowledge is rejected and an orthodoxy is maintained. The activist quoted above mentioned to me that, even though the research has begun to seriously address non-thermal effects (see chapter four), many of the scientists and advisers are still stuck with a 'thermal mindset' (Interview notes, No. 3).

As discussed in the previous chapters, almost all scientific research has concentrated on the possible effects of acute exposures to EMFs. It is these effects on which guidelines are based. However, interest groups have argued that, if we are to take into account public concern, or treat anecdotal evidence of sufferers seriously, scientists must research the effects of the chronic, low-level exposures encountered by those who live near base stations. Exposures from base stations are many times less than exposures from handsets in terms of the dose rate, and most people who live near base stations are exposed many orders of magnitude below the guideline levels. But the suggestion is that the dose accumulated over many years might be harmful. Countless news stories have reported symptoms in people living near base stations, including 'cancer streets' containing a number of base stations. Activists have used a stable construction of anecdotal evidence (or public concern) to undermine consensual science, and in doing so, have illuminated the reliance upon assumptions about acute exposures (based on a dose *rate*) and harm from thermal effects. The interviewee above demands that, if the MTHR is going to make a difference, it must take areas of ignorance seriously, not just continue working within the scientific consensus with research into thermal effects, or research that relies upon the assumptions behind thermally-based guidelines.

The other major body of anecdotal evidence which questions the scientific consensus is the symptoms of electrosensitive people. The possible existence of electrosensitivity questions the assumption that the population respond homogeneously to weak electromagnetic fields. It also suggests that non-thermal effects are very real to some people, despite their lack of replicability in other models. Sir William Stewart, who was the first chair of the MTHR committee, said to the Trade and Industry Select Committee in 2001:

> "I am a believer that perhaps we should look for a set of volunteers who feel that they are adversely affected by mobile phones, by buzzing in the head or sore heads. We need to know the extent to which mobile phones are directly causing these effects. I would like to see a programme set up that included people who would be prepared to be volunteers. I have received letters from people who have said, "Every time I pick up a mobile phone I get a sore head. Could I be included in a test, if there is one?" That is an important point. We get back to the point of populations not being homogeneous. My general view – it is purely speculation – is that if mobile phones have an adverse effect they probably have an effect on a sub-group of the population."[13]

[13] Sir William Stewart, Minutes of Evidence to the HCTISC, 13th March 2001

As Sir William describes, many people who live near base stations, or who suffer from symptoms associated with mobile phones, consider that their experiences present a useful resource and so have offered themselves for study. They want to be studied scientifically. The MTHR meeting of 11th November 2002 demonstrates the tensions in directing research either towards public relevance or scientific robustness. Alasdair Philips, the most active activist in the controversy, asked Sir William Stewart, the departing chair of the MTHR committee, whether they were considering funding studies on people living close to base stations. Another participant, a regional director of public health, pointed out that animal and tissue studies will not address public concerns. 'The people' required definitive epidemiological studies, which would necessarily entail using human volunteers subject to low-level chronic exposures. The reply from Sir William was that the issue of human volunteer studies is very complex (for ethical and practical reasons) and that base station exposures were so much lower than exposures from handsets (Field notes, MTHR meeting, 11th Nov 2002).

This exchange is indicative of the contested ground in public science when research is ongoing and there is high pressure on limited resources. For some (mostly experts), there is a tension between 'addressing public concerns' and strengthening the scientific consensus (one committee member told me that another had insisted the MTHR should, "provide answers rather than more questions"[14] (Interview transcript, No. 28)). For others, it is a battle between relevant and irrelevant knowledge.

With the MTHR, fuelled by Sir William Stewart's enthusiasm to look at some new, more contested areas,[15] we see an institutional sway towards taking anecdotal evidence (or 'reported symptoms'/'public concerns') seriously, at least in the design of research projects. More than just creating hypotheses, the intention is to study sufferers in their environment, taking advantage of their position, which, in the case of those near base-stations, is unique. On the 20th March 2003, the MTHR

---

[14] This quote was relayed to me by a member of the MTHR committee who had seen firsthand the problems of shaping relevant research while maintaining some semblance of a scientific consensus.

[15] It was also suggested to me that the acceptance of the possibility of a vulnerable subgroup (who might be electrosensitive) was given added support by Kjell Hansson Mild, a Swedish scientist who was brought onto the MTHR committee.

committee issued a press release. This announced three new studies funded after its second call for proposals. In the light of the discussion above, two of these are relevant:

- "An epidemiological study of early childhood leukaemias and other cancers near to mobile phone base stations."
- "A study to investigate whether exposure to radiofrequency signals is linked to symptoms reported by mobile phone users."

The chairman, who by this time was Lawrie Challis, a physicist, was quoted in the press release justifying these new studies according to the high levels of public concern about base stations and electrosensitivity.

So the MTHR represents an attempt to move, despite its meagre funding, away from the laboratory, and the framing assumptions this might impose, towards assessing concerned or suffering people in their environments. At least at face value, it has taken some of these symptoms and worries seriously in a way that previous science did not. It remains to be seen whether the studies mentioned above will be welcomed by their participants, interest groups and the public when they report. It would be no surprise if the studies revealed little and became embroiled in another cycle of deconstructive criticism about their methods, measurements and assumptions (cf. Collins 1985).

4 Anecdotes as science

> "And if there's a lot of anecdotal evidence, then it becomes non-anecdotal."
> (Interview transcript, No. 2)

We have seen how anecdotal evidence usually represents evidence that emerges from *outside* science. There is a prevalent expert construction of anecdotal evidence in boundary work. It is usually constructed outside the science/public boundary in an attempt to strengthen the cognitive particularity of *scientific* evidence. However, in the light of MTHR's treatment of anecdotal evidence, we can see the potential of anecdotal evidence not just to play a part in science, but to provide a distinct type of knowledge with which to open up new areas for scientific attention. Before drawing conclusions as to the nature and role of anecdotal evidence in public science, we must complete our taxonomy of epistemological status by considering whether anecdotal evidence and scientific evidence might be as different as some of my interviewees and I have assumed. Some cursory answers from interviewees suggest that scientists might refer to science as 'anecdotal' and often see no qualitative difference between classes of evidence.

The quote above suggests that anecdotal evidence might differ from scientific ("non-anecdotal") evidence only in its quantity. Anecdotes, by this reasoning, can be *de-anecdotalised* (see below), cleaned up and amassed, possibly through research, and possibly, in line with the model of popular epidemiology (Brown 1987, 1992, 1997), at a grassroots level. Conversely, this same scientist, appreciative of scientific messiness, went on to consider whether some science might be anecdotal. Providing a personal definition of anecdotal evidence, he said:

> "I think it's incomplete studies. I think of it in terms of incomplete studies, work in progress, which is coming up with a result. [Discussing a recent study he has been involved with]... it needs repeating to be a good study. It needs repeating in another area... but on the other hand...this finding is so important that I did actually put it to a small group at NRPB... and that's the sort of evidence I think needs incorporating. It's been presented at conferences and that's a realm where you look and see what people will feedback. What comments you get, adverse or supportive. I think there's a lot of information out there that has not yet got to the peer-reviewed stage, and that can take a year or even more. Therefore, you need to include it, and that's what I regard as anecdotal." (Interview transcript, No. 2)

This comment is interesting for two reasons. Firstly, it emphasises the contingencies of scientific practice, especially when trying to construct studies of public relevance. And secondly, it suggests that 'anecdotal evidence' might be an impotent term for boundary work when these contingencies have been laid bare by public scrutiny. We have seen above that anecdotal evidence can be seen to play many roles around science. But the use of the term *within* science deserves further attention.

The scientist above looks at anecdotal evidence in a positive way, as suggestive of a real problem. But using the term as he does above serves to emphasise three things. Firstly, its anecdotality: clarifying the distinction between the contingencies of research and real scientific knowledge. Secondly, the status of the scientist as the guardian of what counts as *relevant* anecdotal evidence in research design. And thirdly, the importance of peer-review in removing contingency and constructing proper science. He is not using the term 'anecdotal evidence' in boundary work, to label something as outside science. But he is supporting the barrier of peer-review by emphasising the anecdotality of science that has not yet passed this test of robustness. In admitting that science can be anecdotal, he removes the power of the term 'anecdotal evidence' to construct public knowledge, but his boundary work nevertheless protects certified scientific knowledge as a source of cognitive authority. He is implicitly demarcating anecdotal evidence from verified evidence by separating the domains of scientific research and scientific knowledge.

So once we accept that anecdotal evidence does not have to remain outside science, we can consider how its status might be improved. We have seen above how anecdotal evidence can provide an important impetus for relevant studies in public science, but some areas of science illustrate the more direct use of anecdotal evidence in answering questions of public concern.

Data as the plural of anecdote? – 'De-anecdotalising' evidence

Examples from occupational medical literature illustrate how anecdotal evidence, once appreciated as a source of information, can be 'de-anecdotalised'. This chapter had described how individual cases are constructed as 'anecdotal' because of their lack of generalisability. One study collected a number of case reports (and a few *collections* of case reports) under the title "Neurological effects of radiofrequency radiation" (Hocking and Westerman 2003). The paper makes the point that, below current (thermally-based) guidelines for mobile phones, most of the evidence confirming the absence of harmful effects has come from epidemiology. The authors claim, "another source of data is case reports, of which there have been several regarding peripheral neurological effects (dysaethesiae)" (ibid. p. 123). Through reviewing cases of symptoms experienced during various RF exposures, they conclude that symptoms can be produced at levels insufficiently powerful to cause heating. This raises the possibility that a mechanism exists that has not been previously considered. It also raises the possibility that a subgroup of people (the subjects of these case reports) might be hypersensitive to RF radiation.

A letter to the British Medical Journal reported on a study that, rather than accumulating case reports, directly examined symptoms experienced by mobile phone users (Chia et al 2000a):

> "EDITOR – Hand held cellular telephones, using pulse modulated signals of frequency 870-995MHz, are being used increasingly. Most reports of health symptoms related to use of these phones are anecdotal."

The letter goes on to describe the study, which took a random sample of 808 people, "who were interviewed by trained medical students using a structured questionnaire… A two tiered approach was used to try to mask the true purpose of the questionnaire in which headaches and health symptoms were dealt with in the earlier sections before respondents were asked about their use of cellular phones." The study reported that prevalence of headache increased with mobile phone use (Chia et al 2000b).

In these articles we see an appreciation of both the usefulness and the failings of anecdotal evidence. Its scientific advantages lie in the possibility of identifying marginal and surprising effects. Its disadvantages, as we have seen, are that it is seen as individual and subjective. Scientific respectability is therefore attempted both through accumulation and through removing the potential for recall bias, as in the latter study. The Hocking and Westerman study is a meta-analysis of existing collections of case reports. It demonstrates that anecdotes, when piled high, can be accumulated to form persuasive arguments for the limits of our understanding of subtle physiological effects.

As we have seen previously, however, the anecdotality of this type of science is seen by some as an unresolvable problem. Dr Clarence Tan, the head of the Singapore Health Authority, dismissed the Chia et al study, discussed above: "It is always dangerous to take anecdotal evidence and generalize it". He had previously insisted that "as long as the exposure is below international guidelines, it is safe", (Microwave News, Sept/Oct 2001) (see 'Discourse of Compliance' from chapter four).

We have seen in the previous few sections that science and anecdotal evidence are no strangers, despite attempts by some to emphasise their differences. Anecdotes can be reshaped and built into bodies of persuasive evidence, although views of its robustness vary among scientists (often depending on the perceived degree of overall scientific uncertainty). The act of accumulation reduces the anecdotality of evidence. This problematises the boundary work adage that "data is not the plural of anecdote". However, anecdotes cannot be transformed unproblematically into bodies of knowledge. A tension is evident among experts between subjective symptoms (experienced by the sufferer) and objective symptoms (externally observable). The move from one to the other is very difficult. Many provocation studies have attempted to test (in a manner that is just as forensic as scientific) the reality of reported symptoms, but these are plagued by the interpretative flexibility of any controversial science (see Collins 1985). For acceptance, anecdotes need to be framed in such a way as to make them scientifically robust. They can be framed as 'case reports' by doctors, or used as the basis for rigorous study of reported symptoms. It should come as no surprise that knowledge traditionally considered outside the realm of science plays an important role in shaping scientific practice (cf. Hilgartner 1990). But the 'de-anecdotalisation' of anecdotal evidence adds another facet to our understanding of how knowledge and evidence are treated across, and on either side of the science/public boundary.

An emphasis on the anecdotality of some scientific activity can act as a way of controlling research and interpretation of 'scientific' results. Colin Blakemore, a member of IEGMP and AGNIR, responded to a press report in which he was quoted as saying that mobile phones gave him headaches. The response stated that this was not what he had said, and added:

> "The anecdotal impressions of scientists are no more important than those of anyone else. What is needed is further research, as indicated in recent advice from the NRPB and AGNIR"[16]

He is here undermining his own opinions in an effort to allow the science to speak for itself. This is an association of anecdotes with an expert, but also a distancing of expertise from evidence. He is emphasising the distinction between person and knowledge. Anecdotes are subjective, even if they come from an expert. Knowledge is objective. While the interviewee quoted above distances scientific practice from scientific knowledge, Colin Blakemore distances *science* from scien*tists.*

So what can we learn from moves to bring anecdotal evidence into the fold of science? As we have seen with the MTHR programme and with arguments over the anecdotality of science, anecdotal evidence can be useful in framing scientific

[16] "Risk from the Use of Mobile Phones" – Press release, University of Oxford, 4th March 1999

questions, but it is liable to come under fire if it is not sufficiently de-anecdotalised when science claims to provide answers. But, as we shall see in this chapter's conclusion, the adoption of anecdotes represents a way of controlling the role of the public in public science. By considering anecdotal evidence as just another (limited) form of knowledge, experts can head off criticism about the treatment of public evidence. This reduces anecdotal evidence to weak evidence, and strips it of any unique epistemological properties it might have. It also reduces anecdotal evidence to a (weak) epistemological contribution. However, the social meanings behind anecdotal evidence demonstrate that in public science debates, it contributes much more than an attempted contribution to knowledge.

Before continuing our critique of expert constructions of anecdotal evidence in a public science controversy, we should consider what deeper socio-political meanings such evidence, and its description, carry for non-experts. We can then consider how differences between expert and inexpert constructions might lead to confusion about its usefulness. We can also indicate how the explicit inclusion of anecdotal evidence in scientific advice(and subsequent research) represents a deeper political statement about public engagement and expert control of a controversy.

Anecdotal Evidence as Narrative and Testimony

We have seen above how anecdotal evidence can be used to define, shape and straddle the cultural boundaries of science. We have seen how actors give (or deny) epistemological status to anecdotal evidence in shaping relevant knowledge. But we have also seen how these epistemological meanings can focus the social and political dimension of the debate.

One of the purposes of this thesis is to encourage acceptance of a view of co-production in public science – that science and socio-political order will construct one another in the course of a public science issue. Science and society are therefore necessarily intertwined and so define one another. There are some important reasons why anecdotal evidence has served as a crucial site for controversy in this debate that are not simply epistemological. We therefore need to ask some broader questions about the social importance bestowed on anecdotal evidence by non-experts.

This project, which concentrates on expert constructions, is unable to offer a full circle of perspectives on anecdotal evidence. However, we can draw upon existing insights to suggest why, for members of the public, anecdotal evidence might be an influential source of information for decision-making. Insights from medical anthropology suggest that narrative plays a vital part in the constitution of a normal life for those people who suffer from an illness. As Byron Good puts it, "narrativization of suffering serves to reconstitute the lifeworld "unmade" by chronic pain" (Good 1994, p. 136). His aim is to explore "how illness narratives are structured in cultural terms and how these reflect or give form to distinctive models of lived experience" (ibid.) Good makes the point that the conversations he had with sufferers and health care providers alike were largely story-based. We might therefore expect anecdotes to follow the same pattern. The type of evidence retold in the IEGMP's public meetings, in the letters received by the group and on countless Internet sites, have a narrative structure.[17] Just as sufferers of chronic pain construct narratives, so we might expect the narration of these stories of exposure and illness to have a beneficial effect.

Within the records of the IEGMP's work, there are many accounts from people who experience symptoms either from mobile phone use or from long-term exposure to a nearby base station. The type of evidence that were submitted, and the types of anecdotes that have defined the public context of the controversy, all contain some explanation of causality. This is unsurprising, because a narrative needs to give meaning to an individual's suffering for it to provide a useful mitigation. However, it reminds us that evidence is in itself worthless unless it is presented as evidence *for* something. In this sense, these stories become testimonies. Testimonies, as described by Judith Lewis Herman, can restore the dignity that might have been lost as a side-effect of illness or trauma. Crucially, "testimony has both a private dimension, which is confessional and spiritual, and a public aspect, which is political and judicial" (Herman 1992, p. 181). In the same way, anecdotal evidence reports on experiences, but links these experiences with a broader context. In the case of mobile phones anecdotes, the testimonies were testimonies of

[17] A narrative has features of a personal story. So, for example, a description of symptoms might emerge as a chronological account of the development of abnormality or pain, together with an attempt to give meaning to these symptoms.

rejection and marginalisation by experts who had refused to accept salient gaps in scientific understanding.

When the MTHR's first round of research funding was announced, Jim Mochnacz, who had written to the IEGMP reporting his long-term exposure and subsequent collection of symptoms, was interviewed on BBC's One O' Clock News. Before relaying his symptoms to the camera, he was introduced. "Jim Mochnacz believes this latest research has come too late. He's convinced extensive exposure to mobile phones caused him serious illness, which eventually got so bad, he had to give up work." (BBC1 News, 25th January 2002). This quote begins to illustrates the interconnectedness of the diagnostic and political aspects of testimony. But it also reminds us that, in adding weight to anecdotal evidence, as well as giving it meaning in broader context, the media play a crucial part.

We have already seen how anecdotal evidence has come to stand for groups of people, often with perceived political intentions. But why might these people submit anecdotal evidence to a public forum, rather than just constructing a personal narrative with which to rationalise their symptoms to themselves? While some sufferers might offer their symptoms to experts as an epistemological resource, there is a political motive for submitting this kind of evidence to an expert panel. Crucially, for those whose illness is not accepted as real by science, anecdotal evidence is not just evidence of the reality of symptoms, but evidence of the obstinacy of a scientific establishment.

The causality offered as a vital part of such evidence is political as well as medical. Personal or interest group reports of symptoms associated with mobile phone technology such as those submitted to the IEGMP usually included allegations of a cover-up, of the NRPB's close association with industry or of the partisanship of various experts. The IEGMP public records reveal a disenchantment with expertise and a lack of trust. And in the absence of reliable or credible scientific (global) knowledge, it is understandable that sufferers will readily retreat to their own personal (local) understandings.[18]

[18] A similar phenomenon was noted by Sheila Jasanoff in her analysis of the civic dislocation caused by the handling of BSE/vCJD (Jasanoff 1997, p. 223).

Anecdotal evidence carries deep political as well as epistemological meanings. Interactions between experts and anecdotes can therefore prize open the hybrid nature of scientific advice and affect the direction of relevant scientific research, without recourse to the acquisition of significant 'lay expertise' (although many would consider their evidence to be a distinct kind of expertise).

Returning to the quote used in this chapter's introductory anecdote, from the IEGMP public meeting in Liverpool:

> "In July of this year, my husband was diagnosed with brain cancer. Who is gathering all this anecdotal evidence? Because it seems to me until people start gathering this in some proper scientific evidence, various people for various reasons will continue to talk about anecdotal evidence, and so wriggle out of it."[19]

Anecdotal evidence is a claims-making activity, contributing to the definition of a social problem. Post-structuralist literature on the sociology of social problems considers the role of claims-making by different groups in the definition of social problems (see Schneider 1985). The claims-making process often begins with attempts to force public recognition of private concerns. We can now see how Trudy Clarkson (quoted above), in an attempt to give meaning to her husband's condition, is claiming both a medical causality of symptoms and a political causality to explain why uncertainties over marginal effects from phone use are not being researched. She is at once emphasising the importance of anecdotal evidence in asking (and answering) difficult questions and the problems of expert constructions of the term.

Conclusion

In a public controversy where public interest may be hampered by asymmetries of expertise and a feeling of alienation from technocratic processes, a single small story can provide a vital entry point. When the foot and mouth epidemic spread through rural Britain in the spring of 2001, the prescriptive subtleties of various epidemiological models of the spread of the disease were lost in the campaign to rescue a pretty little calf called Phoenix. Similarly, a report of a brain tumour (or other less serious disease) caused by mobile phone use can condense the intricacies

[19] Trudy Clarkson, from the transcript of the IEGMP public meeting, Liverpool, 9th December 1999

of the public science controversy over mobile phones, much to the annoyance of many scientists.

But, to return to the question side-stepped at the start of this chapter, what is 'anecdotal evidence'? Firstly, as we saw most recently, anecdotal evidence is, for some, a way of giving a situation meaning. It is a way of getting a handle on a complex scientific debate. Arie Rip uses the phrase 'window on the world' to describe these ways of seeing the global in the local (Rip 2003a). But our concern in this chapter has been the view experts have of anecdotal evidence. To consider the extent to which anecdotal evidence has done much of the reconstruction work described in the previous two chapters, this chapter has concentrated on the people who would perhaps like to consider themselves on the other side of the window, the experts who would represent global, universal knowledge.

This chapter has begun to flesh out a definition of anecdotal evidence in a scientific context by outlining the multiple statuses afforded to it by (mainly expert) actors. To summarise, we can extract a few key features of the term in this context:

1. Anecdotal evidence has little claim to universality. As such, to many scientists, it represents an antithesis of robust scientific knowledge.

2. Anecdotal evidence is often defined according to its source. For experts, this often means 'outside science'. The mass of erroneous knowledge 'outside science' makes anecdotes easy to ignore as an epistemological resource.

3. Many scientists see anecdotal evidence as important in the shaping of relevant research projects.

4. Many actors have argued that, because of the large areas of scientific uncertainty or ignorance (see chapter five), anecdotal evidence, even if regarded as weak, is often all that is available.

5. Anecdotal evidence is evidence *for* something. As well as representing an instance of an illness, it represents arguments about how this illness should be understood. As such, it is part of a claims-making process.

6. Anecdotal evidence carries important political baggage. It is a hybrid political/epistemological resource.

7. Anecdotal evidence, to actors outside the scientific orthodoxy, is seen as easily rejected. These same actors often emphasise its usefulness. The rejection of anecdotal evidence serves to reinforce a view of scientific and political orthodoxy as undemocratic and self-serving. Anecdotal evidence is therefore evidence of both danger and scientific obstinacy.

8. In public, anecdotal evidence can contribute not just to the design of relevant research. It can also provide raw materials for accumulation (what I call 'de-anecdotalisation').

9. The contested nature of anecdotal evidence represents a microcosm of the contested nature of public science. It can also serve to direct research though negotiations about its importance and relevance.

This final point captures my initial incentive for researching into anecdotal evidence – a suspicion that opinions about anecdotal evidence would reveal opinions about the relationships that interest studiers of science and technology: between experts and non-experts or science and politics. Impressions of the nature and usefulness of anecdotal evidence reveal opinions about the roles that experts and non-experts each see themselves and the other playing. The individual constructions of the term 'anecdotal evidence' are positively related to opinions about the role of the public in scientific debates and the ability of science alone to settle questions of public importance. Anecdotal evidence can therefore be a useful touchstone for the broader issues that define public science.

Ordering the boundaries of science and society

We have seen through the early sections of this chapter that anecdotal evidence is not, ex ante, a thing. At the birth of this controversy, there was no file of evidence marked "anecdotal". Some might classify evidence as anecdotal while others argue that it is scientific. Others might respond that it is not yet scientific, but that it merits serious scientific attention. Some actors might classify information as anecdotal evidence in a bid to deny its evidential worth. Others might claim that just because evidence is anecdotal it doesn't make it any less useful. Still others might claim that the *anecdotality* of evidence provides the exact justification for its consideration, because it can suggest answers where scientists cannot or will not. In the course of a public science controversy, we can see that anecdotal evidence carries meanings that are used to negotiate the social and epistemological territory of science and the public.

To an extent, experts and non-experts use 'anecdotal evidence' as a device to define the boundaries between science, politics and society. Some scientists try to construct anecdotal evidence as unscientific, and therefore unworthy of attention. A construction of anecdotes is used to define what science *isn't*, and therefore strengthen the cognitive authority that comes with tests of objectivity such as peer-review. This boundary work, as predicted by Tom Gieryn (1983) and Sheila Jasanoff (1990), is a manifestation of a need to maintain the authority of science in hybrid domains by demarcating it from areas deemed 'political'. In my case study, anecdotal evidence made claims that highlighted what science didn't know about. This uncertainty, as we saw in chapter four, had been considered outside the realm of 'sound science', falling into the 'political' domain of precaution.

Central to the problem of institutional rejection of anecdotal evidence is the problem of scientific advice acting in a hybrid domain among scientists, politicians and other interested parties. The NRPB, which had come to represent not only regulatory science, but also an industry-supported scientific elitism, became the target of criticism because of its efforts to deny anecdotal evidence a place in scientific work. People who provided evidence, often supported by interest groups, were seen as making political points rather than adding to a body of knowledge. While NRPB scientists appreciated the importance of anecdotal evidence in hypothesising about public health knowledge, the public face of the NRPB rejected anecdotal evidence outright. An expert construction of anecdotal evidence as being *nothing more* than 'public concern' led to a perception that public attempts to contribute to science were an attempted political incursion into an area that had been defined as fiercely 'science-based' (see chapter four).

However, as this chapter has shown, the boundary work of constructing 'anecdotal evidence' as nonscience does not represent the prevailing expert treatment of the term. We have seen how, in public science, where research in ongoing and under the public gaze, boundary work is complicated. It is difficult to use a term as slippery as 'anecdotal evidence' as an unscientific 'other' and maintain public credibility. Many scientists consider anecdotal evidence to play a vital part in the design of relevant public health research, perhaps by providing subjects for human volunteer studies. Others consider that the contingencies of scientific research mean that much science is 'anecdotal'. In public, the politics of boundary work become highly visible, especially when anecdotal evidence is perceived to have the potential to challenge scientific orthodoxy.

So whatever boundary work is going on tends to occur in both directions. Dismissal of anecdotal evidence is seen by some as reinforcing a boundary around 'good science' that only demonstrates how irrelevant 'good science' is (cf. Epstein 1996). Those who feel that the anecdotal evidence they provide, or know about, is being dismissed often emphasise its anecdotality. This serves to make two points: that experts are ignoring members of the public; and that anecdotal evidence might represent an alternative or contributory resource for scientists, which is easily accessible through greater public engagement.

There is much more to anecdotal evidence, when placed alongside science, that is not easily explained by seeing the deployment of the term as an example of science/public boundary work. The attitudes of many of my interviewees suggest that anecdotal evidence might strengthen the production of adequate knowledge rather than dilute it. There is no single *scientific* view of anecdotal evidence, but it is clear that the views that do emerge are informed by opinions of science, policy and the public. Anecdotal evidence is doing something at the boundary between science and non-science, so the term 'boundary-ordering device' (Shackley and Wynne 1996) might better explain its role. Boundary-ordering devices, like Star and Griesmer's (1989) 'boundary objects', have stable meanings across various social worlds, which facilitate understanding and communication.[20] But they are sufficiently flexible to accommodate individual understandings. For anecdotal evidence, this would explain why non-experts and experts came to develop a mutual understanding of what anecdotal evidence is and who it represents, with non-experts shamelessly claiming ownership of anecdotal evidence. So while anecdotal evidence did not stand for much before mobile phones became controversial, arguments over its origins, status

[20] Shackley and Wynne suggest that boundary-ordering devices differ from boundary objects by being less durable. Whereas boundary objects are normally *things*, boundary-ordering devices can be collective discourses over issues such as scientific uncertainty, which can condense discussion (Shackley and Wynne 1996).

and purpose produced a clear idea of who and what anecdotal evidence meant. Within this stable construction, the contested question was not 'who counts as anecdotal?' but 'what use is anecdotal evidence?' The constructed features of anecdotal evidence that make it such an accessible and contentious site for the negotiation of public are paradoxical. Anecdotal evidence is unscientific, but only some see this as problematic. People with anecdotal evidence often do not claim to be scientific. They claim instead to reframe problems and point towards new areas of knowledge production.

The most interesting facets of anecdotal evidence are revealed when it is reshaped. We have seen in this chapter how anecdotal evidence has been welcomed into the public science that decides the health effects of mobile phones where previously it had been ignored. The boundary-based concepts that I experimented with above do not tell us a huge amount about change. In this next section, I hope to illuminate what happens when anecdotal evidence goes places.

Anecdotal evidence as liminal knowledge

Much of the criticism of scientistic constructions of anecdotal evidence is supported by claims of severe scientific ignorance. After all, how is it justified to reject evidence from outside science if it is the only relevant evidence available? In the light of the uncertainty and ignorance constructed by various actors in the debate (expert and non-expert) (see previous chapter), contested cognitive territory expands, and there is no orthodox scientific evidence with which to counter such interjections. Standards of evidence and 'science-based' regulation are thus exposed as resting on political assumptions. Anecdotal evidence can therefore be presented (by interest groups, members of the public or other scientists) as a valuable resource only obtainable through greater public engagement. This use of anecdotal evidence as a means of exploring what science does not know is part of its appeal, as hinted at in this chapter. And it fits well with what some commentators refer to as *liminal* phenomena.

Liminality (a term originally used by Victor Turner (1969)), describes phenomena that are "betwixt and between the positions assigned and arrayed by law, custom, convention and ceremonial" (ibid., p. 95). They are without ascribed status, and possess "ambiguous and indeterminate attributes" (ibid.). Turner discusses liminality in the context of the identity of people passing through rites of passage, but the term can also usefully illuminate the scientific construction of uncertainty. Karin Knorr Cetina has described how liminal phenomena in science lie between positive knowledge and effects that are considered unknowable (Knorr Cetina 1999, p. 63). She notes how science can use liminal phenomena such as errors or anomalies to investigate the limits of knowledge, and so narrow the region of positive knowledge (p. 64). But she is studying the world of high-energy physics, a culture which has chosen to investigate liminality in this way. Not all scientific cultures would regard liminal phenomena with such interest.

As I explained in chapter two, public science is very different from the esoteric practices of particle physics laboratories, and any move towards investigating liminal areas is slowed by the weight of political and regulatory complication we saw in the previous two chapters. But we can follow Knorr-Cetina's lead, and reintroduce the liminal emphasis on change, to paint an interesting picture of anecdotes.

Liminality means a loss of structural definition. A liminal person or thing is defined simply as being like others in the same state, which can create a solidarity that is not accounted for by traditional social structures (Turner calls this 'communitas'[21]). Anecdotal evidence is liminal because it fits neither into structures of scientific knowledge, nor into a coherent set of political claims. Each anecdote is different and often the only feature they share is their anecdotality. But the liminality of anecdotes, as we have seen throughout this chapter, is used to great effect. Firstly, anecdotal evidence came to define much of the public debate over mobile phone health risks. Secondly, it defines being 'not scientific' – a position made less unattractive by growing disenchantment and falling trust in science. Thirdly, it defines the limits of what science knows, what science does not know, and what science is aiming to know.

We have seen through literature in STS in recent years how uncertainty claims can represent authoritative scientific attempts to control the bounds of 'what we don't know' (Campbell 1985, Shackley and Wynne 1996, Balmer 2000, Smithson 1989). Equally, anecdotal evidence is seen as 'evidence of uncertainty' (this is very

[21] In Turner's own words, "Communitas is made evident or accessible, so to speak, only through its juxtaposition to, or hybridization with, aspects of social structure." (Turner 1969 p. 127)

important in the light of the precautionary narrative of the previous chapter). Anecdotal evidence is often anomalous. It can be indicative of unknown effects and it can help define the limits of current scientific knowledge. Anecdotal evidence can be used to justify further research in much the same way as authoritative claims of uncertainty are. So while its power might be meagre within the structure of scientific knowledge, it has potential to reframe the definition of uncertainty. As we saw in chapter four, the discourse of compliance does not consider uncertainty and ignorance (e.g. uncertainties about non-thermal effects) important for scientific advice on mobile phone risks, despite their importance at a laboratory level, so they edge towards liminality in public science. This makes the integration of anecdotal evidence and uncertainty all the more likely.

As we saw in chapter five, the IEGMP returned uncertainties to the public issue of mobile phone safety and reassessed the importance of anecdotal evidence. In doing so, they reintegrated, at least partially, the liminal phenomenon of anecdotal evidence as a resource in public science.[22] But this has required change, which has transferred ownership and control of anecdotal evidence from non-experts to experts.

When experts begin to address anecdotal evidence as a resource rather than an annoyance, we see negotiations that illustrate the complexities of shaping science and society. Anecdotal evidence can be adopted as a formal indicator of productive research (often through medical case reports), or a source of volunteers for study, scientising (de-anecdotalising) evidence that might once have been ignored. The first step of de-anecdotalisation can take the form of formally reframing the evidence as 'uncertainty'. As we saw in the last chapter, the MTHR programme has begun addressing uncertainties that have been largely framed by the anecdotal reports of non-experts. But we must ask what is happening to anecdotal evidence when it is being adopted in this way and how, when it is afforded little status by authority, it can move across the boundaries that separate science from other forms of knowledge.

---

[22] The move from liminality back into the structure of society was originally considered by Victor Turner within the contexts of rituals and rites of passage. Although this thesis sees the Stewart report as highly influential, I will refrain from considering it as a necessary ritual.

The uniqueness of anecdotal evidence lies in its hybrid epistemological and political claims. Anecdotal evidence is epistemologically weak, which implies that it is easily dismissed by science (Longino 1990). But my analysis complicates this. Anecdotal evidence, in context, can be very powerful because of the claims it makes, the questions it asks and the connections it makes with broader issues of trust, independence and quality. However, for anecdotal evidence to move into the expert world, its epistemological and political components must be separated by those experts engaged in public science.

We have seen in this chapter how negotiations about the importance of anecdotal evidence can take place that combine epistemology and politics. The rhetorical power of an anecdote can be put aside if it is to be sold as an epistemic resource, and the epistemological status of anecdotes can be negotiated in public to change its political weight. As we saw earlier in this chapter, with Gerard Hyland's exchange with the COST 281 committee, the flexible status of anecdotal evidence, and its lack of formal definition, can make it a powerful term for undermining hybrid scientific advice. But for expert acceptance, it must be made clear what is a claim to contribute to science and what is an argument for public consultation, or for banning mobile phone masts near schools. This leads to a dual strategy, *de-anecdotalisation* and *conflation with 'public concern'*.

Anecdotal evidence can be de-anecdotalised by science to an extent: cleaned-up and amassed, or formalised in the design of a research project (for example, a provocation study designed to test claims to electrosensitivity). This de-anecdotalisation treats anecdotes as important epistemological resources but ignores any political purpose they might serve in constructing public controversy, so it demythologises them.

This chapter has also seen, conversely, how experts can strip anecdotal evidence of its epistemological claims and equate it with some form of public concern or worry. The adoption of public concern is a democratic exercise, part of the fabric of doing 'good policy'. So anecdotal evidence can be welcomed (but not as a contributor to knowledge) if the democratic incentives are sufficient (see chapter two on 'questioning expertise'). As experts tacitly formalise a role for anecdotal evidence as 'public concern', they are shaping the boundaries of acceptable public intervention in science.

Anecdotal evidence, in becoming a point for common discourse, rather than antagonism, over the role of the public in science, has been reshaped by expertise. It is no longer an unscientific, or antiscientific resource. For experts, anecdotal evidence provides a good way to justify continued research in two ways: to reduce scientific uncertainty (which anecdotal evidence has helped to cleave open) and to address significant public concern. The danger for members of the public who have been actively engaged in the controversy is that their engagement is being reshaped by these decisions. In particular, the conflation on non-scientific evidence with public concern constructs 'the public' as a formless mass of ambivalence, rather than a group of people asking important questions in an important area.

This chapter has further undermined any conception that might still exist that evidence is neutral in public science. Evidence is constructed as relevant along with the shape of science and decisions about policy. So the appreciation of anecdotal evidence can determine, or be determined by the level of scientific uncertainty:

> "Oh, no, we certainly do have a more public dimension, if you think, there were no mobile phones in '91, or if there were they were not particularly popular… so in that respect we are using anecdotal evidence more and more, just to reflect the uncertainties in the data." (Interview transcript, No. 32)

… or it can determine, or be determined by, precautionary policies:

> "You would be pretty crazy not to take that kind of information as evidence of the need firstly to mount an investigation, and secondly to be thinking about whether you should be recommending some kind of precautionary behaviour on behalf of people now, until rigorous evidence has been forthcoming." (Interview transcript, No. 20)

Indeed, the appearance of 'anecdotal evidence' in advisory discourse coincided with the sway towards precaution. Anecdotal evidence moves between scientific practice and scientific advice, undermining whatever boundary is erected to separate these areas, because of the kinds of questions that it asks. The 'balance of evidence' will rarely be affected by anecdotes, but this misses the point. The truth or falsity of anecdotal evidence does not matter so much as the assumptions that are questioned and the connections that are made. These questions can be, 'what counts as good science?' or, 'who decides what counts?' But, more recursively, anecdotal evidence also implicitly questions the procedures that assess the validity of evidence[23]. For all of these questions, science alone is ill-equipped to provide answers. In public science, it appears that anecdotes punch well above their weight.

Chapter Postscript: Anecdotal evidence and lay expertise

My discussion of anecdotal evidence was prompted by an interest in forms of knowledge that traditionally lie outside expertise. However, it has progressed without much reference to established STS ideas about 'lay expertise'. The notion of lay experts, people who possess useful knowledge but are unaccredited, has informed much of the most interesting writing about interactions between experts and non-experts. Engaging lay experts, it is argued, can make science more reflexive, more relevant, more thorough or more credible (see, for example, Wynne 1996b, Epstein 1996 and Kerr et al 1998).

Recently, however, criticisms have been articulated of the usefulness of the term 'lay expert', which is, after all, an oxymoron. Collins and Evans have argued that there is nothing 'lay' about lay experts. It just so happens that they are unaccredited (Collins and Evans 2002). "Lay people as lay people", they argue, "have nothing to contribute to the scientific and technical content of a debate" (ibid. p. 281). Collins and Evans would rather consider 'experience-based expertise' (or local knowledge) as the important contributor. Having looked in depth at the construction of 'anecdotal evidence', we can ask where it fits with concepts of lay-expertise or local knowledge.

Anecdotal evidence, as it has been formalised in the case of mobile phone risk, is almost as 'local' as it is possible to be. Knowledge, or 'expertise', in most anecdotal cases, is bounded by an individual's body.[24] Anecdotal evidence questions the framing of scientific advice just as unaccredited experts do, but, as this chapter has demonstrated, anecdotal evidence is often not a claim of expertise. Indeed, it is often presented as representing the failings of 'expert' understandings of a problem. I would argue that, rather than seeing anecdotal evidence as, a priori, an important

---

[23] This point was suggested to me by Jerry Ravetz (personal communication).

[24] Hilary Arksey's study of RSI sufferers describes how individuals claim their symptoms as the only relevant expertise for understanding an illness (Arksey 1998).

form of expertise, or (very) local knowledge, we should suspend judgements about its worth and consider what happens when anecdotal evidence meets expertise. Collins and Evans assume that 'experience-based expertise' is out there, ready to contribute to resolving technical issues in a way that science alone could not. As I hope this chapter has demonstrated, sometimes it is not clear when we move outside the domain of expertise what is useful and what is not until it is legitimised, constructed and reshaped. If we take anecdotal evidence as local knowledge, we still have the difficulties of how to generalise it beyond the personal. As we have seen towards the end of this chapter, the inclusion of anecdotes in the processes of expertise is a complicated process.

7 Conclusion: Questions and Answers

A recent collection of studies of risk issues (Leiss 2001) contains a brief chapter on 'Cellular Phones'. The theoretical direction is less constructivist than mine and the study is North American, but some themes are very similar. Their conclusion is based around a document authored for the Swiss Reinsurance Company, a strange but important perspective on the issue: part epistemology, part jurisprudence, part risk management and part advertisement. The Swiss Re document largely ignores the question of the reality of mobile phone health risks. As insurers, they are more interested in the question of whether a 'phantom risk' can pose a liability to industry despite its construction by the minds of a scared public rather than by the accumulation of scientific evidence. This question prompts an analysis and an answer that chimes reassuringly with the insights of co-production that I described in chapter two and developed in the subsequent chapters of my narrative:

> "The crucial question, therefore, is not what results EMF research will yield in the foreseeable future, but how society will evaluate such conjecture…" (Swiss Re 1996, p.4)

The high stakes in deciding whether or not harm can be attributed to EMFs results in…

> "…an extremely dangerous risk of change composed of two parts: the classical development risk, that is, the possibility that new research findings will demonstrate electromagnetic fields to be more dangerous that has hitherto been assumed; and the sociopolitical risk of change, in other words, the possibility that changing social values could result in scientific findings being evaluated differently than they have been thus far." (ibid.)

As we have seen throughout this thesis, the second 'risk of change' is a more likely and more interesting phenomenon. The scientific evidence plays only a contributory role in the shaping of a public science controversy, only speaking when spoken to. So the answers science provides are crucially dependent on the questions we are asking. Just as technologies are born with little concept of their future implications (Collingridge 1980), so they are born with little idea of their future popularity. The huge popularity of mobile phones has put difficult questions to advisory scientists about knowledge of their health effects. These questions and the advisory responses which can tell us so much about the public relationship between experts and non-experts have been the subject of this thesis.

This thesis has been researched and written with three themes in mind: scientific uncertainty, public engagement and anecdotal evidence. The consideration of these themes at the interface of experts and non-experts has led to my conclusions about the shape of public science. 'Public science' has been used in this thesis to denote the reflexive expert production and communication of knowledge about a public issue. This thesis has been one of the first projects to study public science after BSE, which most of my interviewees considered a watershed.[1] As brought out in the narrative of this thesis, the public science of mobile phone risks has taken place reflexively, with experts considering their rhetoric in the context of low public trust in expertise. It is the purpose of this conclusion to collect the thoughts of previous chapters, remind readers of their main themes and suggest some implications of my work both to future studies of public science and to policy.

Chapter two of this thesis provided a set of theoretical insights which illuminated my research. Through consideration of recent and more established constructivist insights into scientific work, I suggested that a framework of 'co-production' helps us to meld these insights with those that look specifically at the public context of science. The research for this thesis was described and problematised in chapter three, and its products form chapters four to six. Chapter four described how the NRPB, as the responsible advisory agency for all technologies generating electromagnetic fields, found itself facing a crisis of authority when its 'discourse of compliance' proved unsustainable in the context of the public controversy over mobile phone risks. This discourse of compliance contained well-constructed answers about the thermal effects of EMFs, but impeded discussion of scientific uncertainty at a policy-relevant level. Uncertainties about non-thermal effects, chronic exposures and vulnerable subgroups of the population were exposed relatively easily when non-experts realised they were not receiving answers to the questions they were asking. Non-expert questions differed markedly from those

[1] In informal discussions with others about low trust in scientific advice, it has emerged that the impact of BSE may have been much greater among policy and scientific networks than among the public, whose trust was supposed to have been shattered.

considered important by experts. This diminished the credibility of scientific advice, and mobile phones came to be defined as risk objects, despite a weight of scientific evidence which had previously seemed to exonerate them. In the background of chapter four lies the context for the growing controversy, that UK mobile phone use expanded more rapidly in the late-1990s than any other information or communication technology in history.

The benefits of mobile phones were clear to consumers, but the risks were distributed so that some non-users were also burdened, most controversially in their proximity to mobile phone base stations. Chapter five narrated the expert response to a view of growing public concern about mobile phones and their networks. Uncertainties in the science which had emerged as salient were given expert credence by a report (the Stewart Report), which addressed the issue as concurrently scientific and political. However, in doing so, a previously multi-faceted body of public disenchantment and non-scientific evidence was stabilised under an expert construction of 'public concern'. Chapter five's description of the IEGMP's efforts in reconstructing areas of scientific and public concern illuminated the expert performance of this style of public science. Chapter six extended the expert consideration of non-scientific contributions to the process of public science, concentrating on the construction of one term – anecdotal evidence.

As we saw in the chapter on anecdotal evidence, if scientific advice is not credible, it does not take much to ask questions that demonstrate its failings. Individual reports of harm, in the context of low public trust, can contribute to the deconstruction of a previously-robust advisory consensus. Anecdotal evidence can question scientific knowledge: "How much do you know about effects on vulnerable subgroups of people/long-term effects/non-thermal effects?" Or it can recursively question the basis for existing advice: "Why are only thermal effects formalised in guidelines?", "Who decides what evidence is acceptable?" and "Why aren't experts asking questions about these uncertainties?" What should be clear from the narrative of this thesis is that the motivation behind these questions, and the answers that are offered, blurs the line between scientific evidence and scientific advice.

Although these are important insights for illuminating the mobile phones controversy, there is little new in these insights as contributions to STS theory. Studiers of science and technology have been arguing for decades that the reception scientific advice is granted depends less on its authority than on the degree to which it fits with a prevailing political discourse. If expert disagreement can be unearthed, the evidence required to question scientific orthodoxy need not be particularly strong (see Nelkin 1975). So what does this thesis have to say to STS? What questions does it ask and what answers does it suggest?

In chapter two, I made the point that some of the insights that have emerged from the Sociology of Scientific Knowledge are unhelpful in understanding public science. In particular, the notion of the 'core-set', as expounded by Harry Collins, does not help us when we look at the divergent, flexible interests that construct a controversy as simultaneously scientific and public. A recent extension of the core-set model into the public sphere (Collins and Evans 2002) has highlighted its limitations. Criticism has been levelled at this paper (Jasanoff 2003, Wynne 2003, Rip 2003b), the main thrust of which has been the problem of understanding public science with tools used to consider esoteric science. Collins and Evans assume from the outset that there is a relevant type of expertise necessary to resolve a controversy. My case study has demonstrated, as a referee of the original paper argued (Collins and Evans 2002, p. 240), that relevant experts are only identifiable after the dust has settled. Collins and Evans identify this as an 'expert's regress', and admit its likelihood in public science. But they reason that its resolution is necessary for decision-making under uncertainty. However, their model is built on a simplistic idea that a core-set of relevant experts exists and that this core-set is the best-qualified group to resolve controversy.

This thesis has demonstrated that a core-set of expert decision-makers (in so far as they existed) can have their relevance questioned by public and advisory engagement. The IEGMP, by reconsidering the importance of scientific uncertainties, distanced themselves from the previous expert gatekeepers (the NRPB) and redefined who should be providing scientific answers in this area. And the IEGMP, though they claimed significant expertise of public science and public health decision-making, cannot easily be identified as a core-set. Indeed, they gained credibility from their independence from the NRPB's scientific hegemony.

Of course, the IEGMP did not throw away reams of valuable knowledge for the sake of public credibility. A critic of my study might ask whether the Stewart report really changed the shape of the public science of mobile phone risk. When the dust has indeed settled, the sorts of conversations experts are having might be the same as they were ten years previously. However, I would argue that chapter six's example of the move from implicit rejection of anecdotal evidence to explicit consideration (acceptance or rejection) is an important shift. Anecdotal evidence asks very different questions from scientific study, and the decision to listen to these questions is noteworthy. As we saw in chapter two, one criticism of SSK's applicability to public controversies has been its failure to consider that moves from implicit to explicit rejection of evidence might lie outside the scientific community (Fadlon and Lewin-Epstein 1997, note 3). As part of the move towards closure, this is one case where the putative core-set (wherever it is) cannot do its job alone. Science can attempt to close off areas of debate, but relevant questions will still be asked by non-scientists. In real-time public science, policy and the public, rather than forming neat rings around science (as in the Collins and Evans model), are endogenous to the construction of relevant expertise.

As I argued in chapter two, we require an understanding of science that allows us to interpret the construction of public, credible knowledge-in-context. The framework of co-production, to which I have tried to stay faithful in this thesis, helps us.

Returning to Co-production

As noted in the introduction, a model of co-production can go some way to solving the problems of forcing the tradition of SSK into public. Co-production has adopted from Shapin and Schaffer a maxim that "Solutions to the problem of knowledge are solutions to the problem of social order" (Shapin and Schaffer 1985, p. 332, quoted in Jasanoff 2004a). On similar lines, my thesis has gone some way to demonstrating that, for scientific advice, a response to questions of scientific *uncertainty* is also necessarily a response to questions of engagement with *public concern.* I will consider these two themes, and then consider the usefulness of seeing anecdotal evidence as a site for their negotiation.

Scientific Uncertainty

The narrative of this thesis supports the argument that uncertainty does not exist as a problem (or a resource) until it has been controlled and shaped. Experts do not pluck a lack of knowledge from the air. Scientific uncertainty is constructed to meet certain ends.

Chapter four's description of the 'discourse of compliance', practised by the NRPB and supported by the mobile phone industry, reveals a style of scientific advice in which uncertainty was demarcated as an expert, laboratory-level concern. This construction of uncertainty allowed for the (temporarily) authoritative provision of 'science-based' advice. But it hid an expert construction also of a regulatory order (in the form of guidelines which anchored both scientific advice and scientific research) and of the limits of public engagement. Non-experts were entitled to ask about compliance, but prevented from asking about the adequacy of guidelines in determining safety. Uncertainties were seen as important targets for building robust knowledge about EMF effects and strengthening the basis for guidelines. But they were not constructed as relevant to policy or the public.

Reinterpreting chapter five, we see that the IEGMP, with Sir William Stewart at the wheel, appreciated that these previous attempts at technical control of uncertainty could not prevent the growth of political uncertainty, in which an expert monopoly on credibility would be lost. Uncertainty remained constructed as an ongoing quest for scientific endeavour, but the terms of reference were expanded to include areas that had emerged as important in public (such as uncertainty over long-term effects of low-level radiation, or uncertainty over vulnerable or sensitive subgroups of the population). The Stewart report regained a degree of political control of uncertainty by adjusting the bounds for non-expert engagement with technical uncertainties. The publication of the Stewart report set a new agenda for advice on mobile phone risks, and the MTHR programme continued this in a (relatively) accountable forum. A pattern has emerged of expert attempts to regain control of uncertainties that had previously been constructed by non-experts as areas that were being ignored. Even though the jury is officially 'out' on mobile phone health risks, the MTHR committee would like to remind us who the jury are and when they might be coming back. Public uncertainties, emphasising base stations over handsets, for example, have become the target of the MTHR programme's research, which is explicitly aimed at public relevance. Experts are explicitly addressing public concerns, which forces consideration of how these concerns are shaped and reshaped in the different styles of public science described in this thesis.

Public concern

It was never the purpose of this thesis to consider precisely what about mobile phones concerned the public. However, my analysis has revealed that, within expert constructions of robust science and uncertainty, there lay constructions of public concern and the role of the public in shaping science. My research therefore allowed a consideration of the public context of the mobile phones health debate. The Stewart report and the MTHR programme, in an attempt to restore credibility, endorsed the move towards advice and research that is both credible and relevant to the public. However, in constructing the legitimacy of public engagement, these groups of experts necessarily constructed an area of legitimate 'public concern'. We have seen in this thesis that uncertainty and public engagement construct one another and enhance one another's credibility. When science claims there is a consensus over the health effects of mobile phones, it is easy to reject claims from non-experts as 'irrational'. 'Public concern' is constructed as a formless mass of unfounded fears, as nothing more than *the opposite of expertise.* A large proportion of the rhetoric that has emerged from industry and Government since the Stewart report implies that scientific evidence is robust, but that public concern, a separate domain, justifies policy action. My analysis shows that the distinction between public and scientific concerns is not so clear.

A recent book (Burgess 2004) considers the sociology of the 'public fears' of mobile phones. But, because it does not problematise expertise, it extends a model of scientific rationality to one of public *irrationality*, ascribing public risk perception to a set of factors to which expertise is immune.[2] As we have seen, areas of concern that might once have been considered 'public', such as headaches and other symptoms from phone use, or increased incidence of illness around base stations, have become 'scientific' (reframed as the possibility of effects from low-level, chronic exposure) as

---

[2] Similarly, any number of articles and books blame the public 'hysteria' over mobile phones on an irrational fear of radiation (see Park (2000) for an example). For a typical newspaper article, see "Bad medicine" (The Guardian, November 26, 2002), which claims, "the myth of brain cancer from mobile phones is steeped in society's irrational fear of radiation... Ultraviolet radiation, X-rays and gamma rays are the culprits. Visible light and radio waves are always safe." http://www.guardian.co.uk/medicine/story/0,11381,847758,00.html, accessed 27th October 2003. See also, in a more naïve moment, Stilgoe (2000).

experts explore new uncertainties. So 'risk' is neither 'scientific' nor 'social'. It is the (co-)product of representations of both nature and society.

Public concern is anything but a set of homogenous, fixed interests. It is dynamic and, crucially, shaped by experiences with past and ongoing scientific advice. Public engagement with science of the sort described in this thesis will make people reconsider their trust. Public concern, as a flexible resource, can be just as contested as scientific uncertainty. In public science, new research can be justified on grounds of scientific uncertainty and/or public concern. So non-experts can question research priorities by questioning whether expert understandings of public concern are the same as their own. Critics of the Stewart report and the MTHR programme have identified an expert desire to understand and manage public concerns. The recent funding of 'psychosocial' studies by the MTHR committee has been criticised as a way of continuing to avoid conducting studies addressing 'real' public concerns[3]. Similarly, the questions that have been raised in MTHR public meetings have suggested that non-experts might want to regain control of the issues about which they are said to be concerned. One caution we can draw from these discussions is that advisory scientists should not believe that public concerns can be taken into account in a neutral way, without creating new areas for dissent.

Anecdotal evidence

This thesis has opened up a previously unconsidered area of interaction between expert and non-experts – the contested definition and importance of anecdotal evidence. Discussions of anecdotal evidence get us to the heart of public science. As we saw in chapter six, the construction of anecdotal evidence in this case has asked questions about rationality, credibility, uncertainty and public engagement. And the varying status afforded to the term indicates expert views of the legitimacy of these questions. Crucially however, these questions cannot be sorted into 'science' or 'politics'. As a site for co-production, anecdotal evidence helps define both 'public concern' and scientific uncertainty. As anecdotal evidence is brought within the fold of expertise, the changes that occur formalise new constructions of science and the public. The difference between the NRPB's construction of anecdotal evidence (as

[3] Alasdair Philips, for example, recently criticised the MTHR programme for attempting to manage public concern (Powerwatch web site, November 2003, www.powerwatch.org.uk, accessed 23rd November)

an 'other') and the IEGMP's (as a potentially useful resource for probing uncertainty and controlling engagement) can be seen as an extension of institutional ways of knowing. Ignoring anecdotal evidence is one way of maintaining technical control of uncertainty. Accepting ('de-anecdotalising') anecdotal evidence is a way of regaining a degree of *social* control of uncertainty by prising political claims away from knowledge claims (a feature I describe in the section on 'liminal knowledge').

The axes of co-production I have described explain the emergence of concurrent scientific and social orders. But it is important to remember that the stabilisation of this order in my case study has been largely determined by experts. Sir William Stewart and his expert group realised that credible scientific advice could play an active part in reshaping social and scientific orders. In their own way, the IEGMP were doing co-production. The Stewart report reshaped uncertainty, evidence and public concern. Although there was significant non-expert involvement in asking questions of science (defining what counts as credible), decisions about the answers remained with the experts.

So this thesis represents another case study of a scientific controversy, and another case study in public science. It is, at least, a useful narration of the story behind scientific advice about mobile phones. However, part of its novelty lies in its analysis of science in a recent and difficult public context. Social theorists have diagnosed the problems of late- (Giddens 1990) or reflexive modernity (Beck 1992), in which the institutions of progress are forced to look inwards, considering the origins of the trust on which they had previously relied. Equally, however, we could look back (as I briefly did in chapter five) to the loss of public trust in the wake of such policy upsets as BSE. Either way, we now see a more reflexive science and a public wary of the motives behind engagement.

I considered in chapter two that this project should nudge science studies (in a modest way) towards a consideration of public science in context. I made the point that previous studies of science had swept the rug from under science (or at least held a corner), exposing the fragility of its authority. As should have become clear through this thesis, the institutions and groups I have studied have become more reflexive as the controversy has progressed. To an extent, the experts are unpicking themselves. They are moving targets for our analytical weaponry. The study of reflexive institutions and styles of scientific advice will challenge STS in the future and provide many interesting cases and sites for research. Following my own analysis, I would suggest that the expert construction of the legitimacy of public engagement should prompt further research.

The research for this thesis was conducted without a firm idea of its endpoint or the explanation that would emerge. As such, there are some things that I would have done differently with the lessons learned during this research. My first reservation is a general point arising from the comments above, about studying reflexive experts. As I mentioned in previous chapters, ostensible efforts at accountability and transparency are likely to backstage some of the most interesting nuances in a controversy. Studying these subjects therefore requires new levels of determination. My second reservation is that my analysis of anecdotal evidence did not go far enough in unpicking this interesting but difficult term. In the context of 'evidence-based' practices, the myriad constructions of 'anecdotal evidence' by experts and others offers a useful point of access in studying public science. A larger project on the public science of mobile phone risks would have considered a wider range of views on anecdotal evidence. Resource constraints prevented me from studying non-expert (or 'general public') views on anecdotal evidence.

My third reservation is that, in an attempt at rich description, I have lost some of the potential normative impact of my work. I began my research with a motto of 'constructive constructivism', so I should now consider whether a study such as this can usefully inform science policy beyond providing another (pre)cautionary tale.

Talking to policy

Co-production is a more appealing explanation than the alternatives that were hinted at in chapter two. We can see science more deeply embedded in a social and policy context. But as with so many improvements in description, the idea of co-production raises some analytical problems. According to other insights in(-spired by) science studies, a high level of scientific uncertainty justifies the adoption of precautionary policies (e.g. Stirling 1999) and the consideration of marginalised ways of knowing (e.g. Funtowicz and Ravetz's 'extended peer-review' (1992)). Similarly, when decision stakes are high, the same authors advocate widening the basis for policy. As my narrative has demonstrated, uncertainty is a flexible construction, the product of political dissent, low trust and high stakes. Control of this uncertainty can rest with experts or, if not handled credibly, it can be lost, expanding the contested territory. 'Uncertainty', 'acceptable evidence' and 'public concern' are all constructed together. It is not at all clear that one justifies the adjustment or consideration of any other. In this type of public science, good science and good policy are not separable.

Scientific advice can no longer be just authoritative. It must be credible, and it must be authentic (Brown and Michael 2002). In an article on the increasing focus on credibility in late-modern science, Steven Shapin uses the cautionary tale of Cordelia, a modernist child of King Lear, to explain how truth (in her case, of her love for her father) is not sufficient to convince others (Shapin 1995b). We might update this tale and replace Cordelia with her namesake, the daughter of agriculture minister John Gummer. In the spring of 1990, during the BSE controversy, Cordelia was forced by her father to eat a beef-burger under the gaze of television cameras to convince the public of the 'truth' of the Government's claims of safety. But people had little reason to be convinced by such a feeble reassurance. Credibility in this instance was managed extremely badly.

For many people, including many of my interviewees, BSE was a watershed for a new appreciation of science-in-policy. John Major, Prime Minister during the BSE saga, argued that we should 'follow the science', thinking that it would lead to rational policy. But this thesis has shown that, when science is only one factor in the construction of credible policy, we should make it work for us. Science will not resolve societal uncertainties, and this is exacerbated by the dispersion of trust caused by previous policy failures. So what can we say about the Stewart report, which remains *the* important, agenda-setting piece of scientific advice about mobile phone risk?

Firstly, the Stewart report's precaution was significantly less costly than precautionary approaches would have been in the regulation of other technologies.[4] 'Precautionary' decisions such as the lowering of guidelines to ICNIRP levels barely impacted on an industry whose compliance came as standard. Mobile phone

[4] Precautionary policies in other countries have been more stringent. Adam Burgess describes a "small bloc of ultra-precautionary anti-EMF states", Italy, Switzerland and Slovenia, where guidelines for base stations are well below ICNIRP, because of concerns about long-term health effects (Burgess 2004, p. 197). Network operators have experienced great trouble building new networks under these regimes (Field notes, "Mobile phones – Is there a health risk?" conference)

handsets are such a popular technology that an expert endorsement of uncertainty (including promises of expert control) had little impact on sales (except, perhaps, to children). The only area in which the Stewart report might have impacted upon industry was in the call for planning changes. But, by the time the Stewart report came along, the networks for second generation (GSM) mobile phones were all but complete. (Below, I consider the implications for the next generation of networks)[5].

Secondly, the Stewart report addressed both science and policy which, if it was to regain control and remain credible, was almost inevitable. The previous construction of regulatory science had made discussions of public uncertainties very difficult. And policymakers were restricted from discussing uncertainty by an adherence to 'science-based' advice. The IEGMP, by explicitly addressing political themes, was intriguingly considered by interest groups to be less 'political' than the previous efforts of the ostensibly science-based, *apolitical* NRPB. In chapter four I reiterated Andrew Barry's point that attempts to demarcate areas as apolitical will lead to new sites of contestation (Barry 2001). Sir William Stewart realised that credibility could not come from sharply demarcating areas as scientific and political, as rational and irrational. In blurring such boundaries, the Stewart report reintegrated scientific advice with the groups who had been disenfranchised by the discourse of compliance.

So can I critique the Stewart report? My answer has to be, 'Not yet'.[6] In chapter two, I described how constructivist studies of science can fit into a pattern of 'post-hoc irrationalisation', in which policy failures are accounted for after the event. The Stewart report has been received well by the majority of interested parties (with the exception being those who feel, with a firm construction of the public, that precaution will heighten people's worry). However, the agenda set by this style of scientific advice will be tested when new controversies arise, when significant results

[5] The Stewart report has been used by the Government to advertise a new approach to science-in-policy. The IEGMP was A Good Thing, it is claimed, because the NRPB was constrained by providing advice based on science. The IEGMP was able to respond to 'non-thermal' effects, the evidence for which is "at best, ambiguous, but public concerns are as much based on anecdotal evidence". (Implementation Of Guidelines 2000, Report by the Chief Scientific Adviser Professor David King, Annex A - Departmental Reports: Department Of Trade And Industry, http://www.ost.gov.uk/policy/advice/implement_2000/index.htm, accessed 27th October 2003)

[6] Slipping back into ANT parlance, it might seem that the Stewart report has enrolled me.

emerge from the ongoing science, or if new innovations cannot be accommodated (see below). Similarly, only a historical analysis, conducted after the dust has settled, will reveal whether the public participation embraced by the IEGMP was *substantive* (improving the quality of risk assessments) or *instrumental* (improving the public credibility of advisory rhetoric) (cf Fiorino 1990). For the time being, for the reasons of co-production described in this chapter, we cannot easily say where the Stewart report stops doing science and starts doing politics.

In discussing the possible normative applications of STS, Hans Radder has argued that, with global warming, "there is nothing in the constructivist account… to make us more precautionary" (Radder 1998, p. 330). Similarly with mobile phones. My constructivist account of public science has problematised the notion of uncertainties requiring special policy consideration. My analysis reveals an interesting extension to the literature on precaution. In a reflexive context, precautionary expert recommendations can become rhetorical devices for control of public science.

A theme that has run through this thesis has been the tension between robust advice, based on the best available evidence, and inclusive public science, based on the need for public credibility. In the current context of 'evidence-based' policy-making, my analysis suggests that an awareness needs to be maintained that the 'evidence' is not the only evidence available. We must consider which groups are being marginalised as we seek robustness and rationality, and what impacts this will have on public trust and the credibility of policies. Policy is intrinsic to the construction of what counts as good evidence, so decision-makers should not convince themselves that their actions are rational, or that evidence-based policy prevents ideological contamination.

A suggestive counter-example: MMR

Perhaps the most interesting way of considering the Stewart report's style of public science is to offer the briefest of counter-examples. My illustration will be worsened by my lack of research (a full comparative study would be fascinating), but I offer the example of the recent controversy over the MMR (Measles, Mumps and Rubella) vaccine. Some features are shared with the mobile phones debate: a pervasive technology, low public trust, some unreplicated scientific evidence of harm (a link with autism) and a large body of evidence that would fit into the category 'anecdotal' (as constructed during the mobile phones debate). The dissonance between expert and non-expert constructions of danger is similar to that which became a feature of the early mobile phones controversy (see chapter four). This quote is from a recent study of the MMR controversy:

> "In a juxtaposition that neatly captures the contrast between the population-centred calculations that support the public health argument and the individual-centred 'lay experience' frequently employed by the anti-vaccine coalition, [Dr Simon] Fradd's point is countered by Anne Brummer, who says that 'just one look at her son proves a link between autism and the MMR jab.' (Daily Mail, 25th March 1998)." (Moore 2003, p. 15)

This vignette suggests a familiar pattern of the construction of uncertainties, causality and the importance of anecdotal evidence in deciding about this emotive issue. And the context of low public trust in expert advice again provides the background to debates about the MMR vaccine. But in the case of MMR, the benefits of the technology are not as clear to individual users. Vaccination is partly a public good, with benefits of mass immunity accruing above and beyond an individual's protection from disease. The implications of precautionary policies on MMR are therefore considerable. The stakes of policy decisions are markedly different from those of the mobile phones controversy.

A precautionary approach in the case of MMR, taking existing suggestions of harm as evidence of scientific uncertainty, might allow parents to opt for alternatives such as single measles, mumps and rubella vaccines, or no vaccines at all. This would, according to most public health models, increase the incidence of measles, mumps and rubella. The evidence for harm is more persuasive to non-experts than it is to public health scientists, who view it alongside a body of certified knowledge suggesting safety. But the evidence of harm is also multi-layered, as we saw with anecdotal evidence about mobile phones. Andrew Wakefield, the lead researcher on the original study reporting the link with autism and implicit supporter of parents' anecdotal evidence is described as not just a scientist, but "a champion of parents who feel that their fears have been ignored"[7]. Just as with mobile phones (see chapter six), the evidence that has been presented in support of precaution is presented both as evidence of new uncertainties (such as the possibility of a susceptible minority) and as evidence of scientific obstinacy. The UK Government continues to insist that the MMR vaccine is safe, ignoring the evidence offered by mothers and scientists such as Andrew Wakefield. The Department of Health will not take the 'evidence' of autism even as suggestive of the need to research those affected, arguing that its responsibility is to provide the "best possible information"[8]. Uptake of the MMR vaccine has reduced from 92% in 1995/6 to 84% in 2002/3 (BBC News, 26 September, 2003) and epidemiologists have suggested a possible return of a measles epidemic (e.g. Jansen et al 2003).

This counter-example is not offered as a guide to good policy in public science controversies. I merely suggest that cases such as MMR act to keep in check any prescriptions I might make. We cannot separate the 'science' in a public science issue from the decision stakes. MMR operates in a context of low trust, but the stakes might be sufficiently high (for example, a return to a measles epidemic) to justify constructing a sharper definition of science in the public sphere, excluding evidence that might be considered 'anecdotal'. Ostensible appreciation of 'anecdotal evidence' as part of a precautionary policy may be very costly. What is clear is that we can't assess public science without a clear picture of what is at stake (and this will usually be a matter of dispute, with groups exaggerating or downplaying possible consequences). Case studies of public science can therefore play an important role.

We can learn from the MMR example that the required style of public science is crucially dependent on what is at stake in making claims or decisions. This limits the power of the mobile phones case to speak beyond its borders, but it reminds us that risk is a multidimensional concept that can only be accounted for within its context (cf. Leiss 2001). My case study is perhaps best-utilised informing future developments of controversies that will continue to grow from the mobile phones debate.

The future's bright, the future's... uncertain

I made the point in this thesis's introduction that the safety of mobile phones will be a societal rather than scientific decision. The question 'are mobile phones safe?' will

---

[7] From an editorial in the Daily Telegraph by Lorraine Fraser, 21st January 2001

[8] Public Health Laboratory Service/Health Protection Agency press release, 26 September 2002

not be answered until questions of trust, credibility and the validity of ongoing research are resolved (cf. Wynne 2003). It is impossible to predict, given the complexity of interaction in the co-production of public science, whether an issue will spiral out of control, or spiral into self-importance. But there are some developments that will affect the British future of the public science of mobile phone risks.

Firstly, the MTHR committee continues its work, funding some new areas of research and some old ones. Whatever settlement comes from this research will more likely be the product of public engagement than authoritative scientific evidence. It will be interesting to see whether the products of this research are treated with the same degree of non-expert scepticism as the planning of studies has been. By the time this research is complete, its relevance may already have been undermined by the prevalence of new technologies and the production of new concerns.

Secondly, there is 3G – the third generation of mobile phone networks – the licences for which earned the Government £22.5 billion the week before the Stewart report was published. Thanks in part to the Stewart report, the third generation networks should not benefit from the same Permitted Development Rights given to the second generation networks in the 1990s. For the existing four networks (now called Orange, T-Mobile, mmO^2 and Vodafone) this should not pose as big a problem as it will to Hutchinson 3G, who face a difficult search for sites for their new "3" network.[9] 3G will also bring a new set of uncertainties, if interested parties are so inclined to pick them apart. New frequencies, higher powers and new methods of modulating signals will lead some to question again the relevance of what experts claim to know. Similar questions have already been raised about a new network for emergency services (TETRA). Many police officers have complained of illnesses caused by the technology, which has been in operation since 2001, and the network has caught the attention of many of the most vocal activists in the mobile phones controversy.

[9] Personal communications with civil servants and other commentators suggest that the rollout of third generation networks has not been greatly affected by the Stewart report. Any problems experienced by 3G operators more likely rest with the huge amount paid in licences and the slow uptake of the technology by consumers.

Thirdly, there is a possibility that industry will take into account health concerns without ostensibly admitting their importance. As described in chapter four, a phone that radiates less energy into human tissue is a more efficient product. While we might not see a situation as in Sweden or Germany, where low-SAR phones are advertised specifically at concerned consumers, we might yet see a demand for 'safer' phones met by industry.

New innovations, with the voices of some of the same groups of non-experts, will continue to ask new questions. New technologies, such as 'Wi-Fi' and 'Bluetooth',[10] designed to increase the wireless potential of computers and mobile phones, will change our exposures to microwave radiation and attract some of the concerns described in this case study. New scientific studies will produce new results, but whether these are understood as providing answers or asking questions will be a decision for society as a whole, as well as providing a focus for further research of the type seen in this thesis.

[10] Wi-Fi (Wireless Fidelity) is a system in which Internet connections are provided via wireless connection between laptops and a mini base station. Bluetooth is a technical standard for wirelessly connecting a range of Information and Communication Technologies (ICTs) using small microwave transmitters and receivers.

References

- Abraham, J and Sheppard, J (1999) 'Complacent and conflicting scientific expertise in British and American drug regulation', *Social Studies of Science*, 29(6), pp. 803-43
- Adey, WR (1997) 'Bioeffects of mobile communications fields', pp. 95-131 in Kuster, Balzano and Lin (Eds.) *Mobile Communications Safety*. London: Chapman and Hall
- Adey, WR, Bawin, SM and Lawrence, AF (1982). 'Effects of weak amplitude-modulated microwave fields on calcium efflux from awake cat cerebral cortex', *Bioelectromagnetics*, 3, pp. 295-307
- Agar, J (2003) Constant Touch: A global history of the mobile phone. Cambridge: Icon
- ANSI (1982) ANSI C95.1-1982: American National Standard Safety Levels with Respect to Human Exposure to Radio Frequency Electromagnetic Fields, 300KHz to 100GHz.
- Arksey, H (1998) *RSI and the experts*. London: UCL Press
- Arksey, H and Knight, P (1999) Interviewing for social scientists: an introductory resource with examples. London: Sage
- Balmer, B (2000) 'Science, Politics and Germ Warfare: Confessional Uncertainty in Expert Advice', paper presented at the 'Demarcation Socialised: or, Can We Recognise Science When We See It?' Conference, University of Cardiff, 24-27 August 2000
- Balzano, Q and Sheppard, A (2002) 'The influence of the precautionary principle on science-based decision-making: questionable applications to risks of radiofrequency fields', *Journal of Risk Research*, 5(4), pp. 351-369
- Barnes, B and Edge, D (1982) *Science in Context*. Milton Keynes: Open University Press
- Barry, A (2001) Political Machines: Governing a technological society. London: Athlone

- Beck, U (1992) Risk society: Towards a new modernity. Sage: London
- Bloor, D ([1976] 1991) *Knowledge and social imagery* (Second Edition). London: University of Chicago Press
- Bloor, D (1999) Anti-Latour, *Studies in the History and Philosophy of Science*, Vol. 30, No. 1, pp. 81–112
- BMA (2001) *Mobile phones and health: An interim report*, British Medical Association, Board of Science and Education, London: BMA Publications Unit
- Brante, T (1993) 'Reasons for Studying Scientific and Science-based Controversies', pp. 177-192 in Brante, Fuller and Lynch (Eds.) *Controversial Science – From content to contention.* Albany: State University of New York Press
- Brodeur, P (1989) 'The hazards of electromagnetic fields', Parts I, II & III, *The New Yorker*, June 12th, 19th & 26th 1989
- Brown, B, Green, N and Harper, R (Eds.) (2002) Wireless World: Social and Interactional Aspects of the Mobile Age. London: Springer
- Brown, N and Michael, M (2002) 'From Authority to Authenticity: The Performance of Transparency in Biotechnology', *Health, Risk and Society*, 4, 3, pp. 259-273
- Brown, P (1987) 'Popular Epidemiology: Community Response to Toxic-Induced Disease in Woburn, Massachusetts', *Science, Technology and Human Values,* 12 (7), pp. 78-85
- Brown, P (1992) 'Popular epidemiology and toxic waste contamination: Lay and Professional Ways of Knowing', *Journal of Health and Social Behaviour*, 33 (September), pp. 267-281
- Brown, P (1997) 'Popular Epidemiology Revisited', Current Sociology, Vol. 45(3) 137-156
- BSA (2002) 'Statement Of Ethical Practice for the British Sociological Association' March, 2002 http://www.britsoc.co.uk/Library/Ethicsguidelines2002.doc?PHPSESSID=37a284523717297047d638c2d594e016, accessed 5th December 2003

- Burgess, A (2002) 'Comparing national responses to perceived health risks from mobile phone masts', *Health, risk and society*, 4 (2) pp. 175-188
- Burgess, A (2004) *Cellular phones, public fears and a culture of precaution*. Cambridge: Cambridge University Press
- Burns, R (2000) *Introduction to Research Methods*, International edition. London: Sage
- Callon, M (1999) 'The role of lay people in the production and dissemination of scientific knowledge', *Science, Technology and Society* Vol. 4 No.1 pp. 81-94
- Campbell, BL (1985) 'Uncertainty as Symbolic Action in disputes among Experts', Social Studies of Science Vol.15 pp. 429-453
- Carlo, G (ed.) (2001) Wireless Phones and Health II: state of the science. Dordrecht: Kluwer
- Chia, SE, Chia, HP, Tan, JS (2000a) 'Prevalence of headache among handheld cellular telephone users in Singapore: a community study', *Environmental Health Perspectives* 108 (11), pp. 1059-62
- Chia, SE, Chia, HP, Tan, JS (2000b) 'Health hazards of mobile phones. Prevalence of headache is increased among users in Singapore', *British Medical Journal*, 321 (7269), pp. 1155-6
- Collingridge, C (1980) *The Social Control of Technology*. London: Pinter.
- Collingridge, D and Reeve, C (1986) *Science Speaks to Power*. London: Frances Pinter
- Collins, H (1981) 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', *Social Studies of Science*, Vol. 11, p. 33-62
- Collins, H (1985) Changing Order: Replication and Induction in Scientific Practice. London: Sage
- Collins, H (1987) 'Certainty and the public understanding of science: Science on television', *Social Studies of Science* 17, pp. 689-714
- Collins H (1988) 'Public Experiments and Displays of Virtuosity: The Core-Set Revisited', *Social Studies of Science*, Vol. 18, No. 4, pp. 725-748.

- Collins, H (1995) 'Co-operation and the two cultures: Response to Labinger', *Social Studies of Science*, 25 (2), pp. 306-309
- Collins, H and Evans, R (2002) 'The Third Wave of Science Studies: Studies of Expertise and Experience', *Social Studies of Science*, 32 (2), pp. 235-298
- Collins, H and Pinch, T (1979) The construction of the paranormal: Nothing unscientific is happening, in Wallis, R (ed.) *On the Margins of Science: The social construction of rejected knowledge.* Keele, Staffs: Sociological review monograph no 27, pp. 237-70
- Collins, H and Pinch, T ([1993] 1998) *The Golem: what you should know about science* (Canto edition). Cambridge: Cambridge University Press
- Consumers' Association (2000) 'Ring of Truth', *Which? magazine*, 3rd April 2000
- Cozzens, S and Woodhouse, E (1995) Science, Government , and the Politics of Knowledge, in Jasanoff et al (eds.) *Handbook of Science and Technology Studies.* London: Sage
- Dawkins, R (1998) Unweaving the Rainbow: Science, delusion and the appetite for wonder. London: Penguin
- Dear, P, (1991) 'Narratives, anecdotes and experiments: turning experience into science in the seventeenth century', pp. 135-63 in Dear, ed. *The literary structure of scientific argument: historical studies.* Philadelphia: University of Pennsylvania Press
- Dimbylow, PJ and Mann, SM (1994) S'AR calculations in an anatomically realistic model of the head for mobile communication transceivers at 900 MHz and 1.8 GHz', *Physics in Medicine and Biology,* 39 pp.1537-1553
- Douglas, M (1992) Risk and Blame: Essays in cultural theory. London: Routledge
- Douglas, M and Wildavsky, A (1982) *Risk and Culture.* Berkley and Los Angeles: University of California press
- DTI (Department of Trade and Industry) (2000) SARtest report on hands-free kits, from www.dti.gov.uk
- Engelhardt, HT & Caplan, AL (1987) 'Introduction: Patterns of Controversy and Closure: The Interplay of Knowledge, Values and Political Forces', in

Engelhardt & Caplan (eds.) *Scientific Controversies.* New York: Cambridge University Press,

- EPA (1986) Guidelines for Carcinogen Risk Assessment, Published on September 24, 1986, Federal Register 51(185):33992-34003, Risk Assessment Forum, U.S. Environmental Protection Agency, Washington, DC
- Epstein, S (1996) *Impure Science.* Berkeley: University of California Press,
- Evans (2003) *Placebo: The Belief Effect.* London: Harper Collins
- Fadlon, J and Lewin-Epstein, N (1997) 'Laughter spreads: another perspective on boundary-crossing in the Benveniste affair', *Social Studies of Science*, 27, pp. 131-141
- Feyerabend, P ([1975] 1993) *Against Method* (Third edition). London: Verso
- Fiorino, DJ (1990) 'Citizen participation and environmental risk: A survey of institutional mechanisms', *Science, Technology and Human Values*, Vol. 15 no.2, 226-243
- Flyvbjerg, B (2001) Making Social Science Matter: Why Social Inquiry Fails and How it Can Succeed Again. Cambridge: Cambridge University Press
- Foster, K, Vecchia, P and Repacholi, M (2000) 'Science and the Precautionary Principle', *Science*, 288, pp. 979-981
- Foucault, M (1972) *The archaeology of knowledge*, (Translated from the French by A.M. Sheridan Smith). London: Tavistock Publications
- Fujimura, JH (1987) 'Constructing 'Do-able' Problems in Cancer Research: Articulating Alignment', *Social Studies of Science*, 17, pp. 257-93
- Fuller, S (1993) *Philosophy, Rhetoric and the End of Knowledge.* Madison: University of Wisconsin Press
- Funtowicz, S and Ravetz, J (1990) *Uncertainty and Quality in Science for Policy.* Dordrecht: Kluwer
- Funtowicz, S and Ravetz, J (1992) 'Three types of risk assessment and the emergence of post-normal science', in Krimsky, S and Golding, D (eds.) *Social theories of risk.* Westport, Connecticut: Praeger

- Geertz, C (1973) *The Interpretation of Culture.* New York: Basic Books
- Giddens, A (1990) *The Consequences of Modernity.* Cambridge: Polity
- Giddens, A (1996) *In Defence of Sociology.* Cambridge: Polity
- Gieryn, T (1983) 'Boundary Work and the demarcation of science from non-science: Strains and Interests in the Professional Ideologies of Scientists' *American Sociological Review* Vol. 48, December pp.781-795
- Gieryn, T (1995) 'Boundaries of science', in Jasanoff et al (eds.) *Handbook of Science and Technology Studies.* London: Sage
- Gilbert, GN (1976) 'The Transformation of Research Findings into Scientific Knowledge', *Social Studies of Science* Vol. 6 (3/4), pp. 281-306
- Gilbert, GN and Mulkay, M (1984) *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse.* Cambridge: Cambridge University Press
- Gillespie, B, Eva, D and Johnston, R (1979) 'Carcinogenic Risk Assessment in the United States and Great Britain: The Case of Aldrin/Dieldrin', *Social Studies of Science*, Vol. 9, No. 3. pp. 265-301.
- Glaser, BG and Strauss, AL (1967) *The Discovery of Grounded Theory: Strategies for Qualitative Research.* Chicago, Aldine
- Good, B (1994) *Medicine, Rationality and Experience.* Cambridge: Cambridge University Press
- Green Alliance (2001) *Decision-making under uncertainty: The case of mobile phones*, www.green-alliance.org.uk
- Hardell, L, Eriksson, M and Axelson, O (1998) 'Agent Orange in War Medicine: An Aftermath Myth', *International Journal of Health Services*, 28 (4) pp. 715-724
- Hardell, L, Nasman, A, Pahlson, A, Hallquist, A, Mild, KH (1999) 'Use of cellular phones and the risk for brain tumours', *International Journal of Oncology* 15, pp.113-116
- Hardell, L, Hallquist, A, Mild, KH, Carlberg, M, Pahlson, A, Lilja, A (2002) 'Cellular and cordless telephones and the risk for brain tumours', *European Journal of Cancer Prevention* 11, pp. 77-386

- Harraway (1991) *Simians, cyborgs and women.* New York: Routledge
- Harremoës, P, Gee, D, MacGarvin, M, Stirling, A, Keys, J, Wynne, B, Guedes Vaz, S (eds.) (2002) *Late lessons from early warnings: The precautionary principle 1896-2000,* European Environment Agency, Environmental issue report No 22,
- Health Council of the Netherlands (2002) *Mobile Telephones; An Evaluation of Health Effects.* The Hague: Health Council of the Netherlands; publication number 2002/01E
- Herman, JL (1992) *Trauma and Recovery.* New York: Basic Books
- Hilgartner, S (1990) 'The Dominant View of Popularization: Conceptual Problems, Political Uses', *Social Studies of Science*, Vol. 20, No. 3. pp. 519-539
- Hilgartner, S (2000) *Science on Stage: Expert advice as public drama.* Stanford, CA: Stanford University Press
- Hine, C (2000) *Virtual Ethnography.* London: Sage
- Hocking, B and Westerman, R (2003) 'Neurological effects of radiofrequency radiation', *Occupational Medicine*, 53, pp. 123-7
- Home Office (1960) Safety Precautions relating to intense radiofrequency radiation. London: HMSO
- House of Commons (1999) Select Committee on Science and Technology, Session 1998-99, Third Report, Scientific Advisory System: Mobile Phones and Health. London: HMSO
- House of Commons (2001) Select Committee on Trade and Industry, Session 2000-2001, Tenth Report, Mobile Phone Masts. London: HMSO
- House of Lords (2000) Science and Technology Select Committee, session 1999-2000, Third report, Science and Society. London: HMSO
- Hyland, G (2000) 'Physics and biology of mobile telephony', *The Lancet* 356: pp. 1833–36
- ICNIRP (1996) Health Issues related to the Use of Hand-held Radiotelephones and Base Transmitters, Health Physics, Vol. 70, no. 4, pp. 587- 593

- ICNIRP (1998) 'Guidelines for limiting exposure to time-varying electric, magnetic, and electromagnetic fields (up to 300 GHz)', *Health Physics,* 74 (4): pp. 494-522.

- IEGMP (2000) The Independent Expert Group on Mobile Phones report, Mobile phones and health (The Stewart report). Chilton: National Radiological Protection Board, at www.iegmp.org.uk

- Irwin, A and Wynne, B (1996) Misunderstanding Science?: The Public Reconstruction of Science and Technology. Cambridge: Cambridge University Press

- Irwin, A, Rothstein, H, Yearley, S and McCarthy, E (1997) 'Regulatory Science - towards a sociological framework', *Futures*, Vol. 29 (1) pp. 17-31

- Jansen, V, Stollenwerk, N, Jensen, H, Ramsay, M, Edmunds, W and Rhodes, C (2003) 'Measles Outbreaks in a Population with Declining Vaccine Uptake', *Science*, August 8, 301: p. 804 (in Brevia)

- Jasanoff, S (1987) 'Contested Boundaries in Policy-Relevant Science', *Social Studies of Science,* 17 pp. 195-230

- Jasanoff, S (1989), 'The problem of rationality in American health and safety regulation', in Smith, R and Wynne, B (eds.), *Expert Evidence: Interpreting science in the law.* London: Routledge

- Jasanoff, S (1990) *The fifth branch: Science Advisers as Policymakers.* Cambridge: Harvard University Press

- Jasanoff, S (1995) Science at the Bar: Law Science and Technology in America. Cambridge: Harvard University Press

- Jasanoff, S (1996) 'Beyond Epistemology: Relativism and Engagement in the Politics of Science', *Social Studies of Science*, 26, pp. 393-418

- Jasanoff, S (1997) 'Civilization and Madness: the great BSE scare of 1996', *Public Understanding of Science*, 6, pp. 221-232

- Jasanoff, S (2002) 'Science and the Statistical Victim: Modernizing knowledge in breast implant litigation', *Social Studies of Science* Vol. 31(1), pp. 37-69

- Jasanoff, S (2003) 'Breaking the waves in science studies, comment on HM Collins and Robert Evans, "The Third Wave of Science Studies"', *Social Studies of Science*, Vol. 33(3), pp. 389-400

- Jasanoff, S (2004a) 'Ordering Knowledge, Ordering Society', chapter two in Jasanoff, S (ed.), *States of Knowledge: The Co-Production of Science and Social Order.* London: Routledge, forthcoming

- Jasanoff, S (2004b) 'Afterword', in Jasanoff, S (ed.), *States of Knowledge: The Co-Production of Science and Social Order.* London: Routledge, forthcoming

- Jasanoff, S and Wynne, B (1998) 'Science and Decision-making' in Rayner, S and Malone, E (Eds.) *Human Choice & Climate Change, Volume 1– "The Societal Framework".* Columbus, Ohio: Battelle Press

- Kane, R (2001) Cellular Telephone Russian Roulette, A historical and scientific perspective. New York: Vantage Press

- Katz, J and Aakhus, M. (Eds.) (2000) *Perpetual contact: Mobile communication, private talk, public performance.* Cambridge: Cambridge University Press.

- Kerr, A, Cunningham-Burley, S and Amos, A (1998) 'The new genetics and health: mobilizing lay expertise', *Public Understanding of Science* 7, pp. 41–60.

- Keynes, JM (1936) *The general theory of employment, interest and money.* (1996 imprint), New York: Prometheus

- Knorr Cetina, K (1999) *Epistemic Cultures: How the Sciences Make Knowledge.* Cambridge: Harvard University Press

- Kuhn, T ([1962] 1996) *The Structure of Scientific Revolutions.* (Third edition), Chicago and London: University of Chicago Press

- Kuster, N and Balzano, Q (1997) 'Experimental and Numerical Dosimetry', in Kuster, Balzano and Lin (Eds.) *Mobile Communications Safety.* London: Chapman and Hall

- Kuster, N (2001) 'Latest Progress in Dosimetry and Near-Field Evaluations for Mobile Communications Devices', in Carlo (ed.), *Wireless Phones and Health II, State of the Science.* Dordrecht: Kluwer

- Lai, H and Singh, NP (1995) 'Acute low-intensity microwave exposure increases DNA single-strand breaks in rat brain cells', *Bioelectromagnetics*, 16(3) pp. 207-210
- Lai, H and Singh, NP (1996) 'Single- and double-strand DNA breaks in rat brain cells after acute exposure to radiofrequency electromagnetic radiation', *International Journal of Radiation Biology*, 69(4), pp. 513-521
- Latour, B (1983) 'Give Me a Laboratory and I will Raise the World', pp. 141-170 in Knorr-Cetina and Mulkay (eds.*), Science Observed: Perspectives on the Social Study of Science.* London and Beverly Hills: Sage
- Latour, B (1987) *Science in Action.* Cambridge: Harvard University Press
- Latour, B (1993) *We Have Never Been Modern.* Cambridge: Harvard University Press
- Latour, B (1999) Pandora's hope: Essays on the Reality of Science Studies. Cambridge: Harvard University Press
- Laudan, L (1998) 'Demystifying Underdetermination', pp. 320-353 in Curd and Cover (Eds.) *Philosophy of Science – The central Issues.* New York and London: WW Norton
- Leiss, W (ed.) (2001) *In the chamber of risks: Understanding risk controversies.* Montreal and Kingston: McGill-Queen's University Press
- Leiss, W and Paoli, G (2001), 'Cellular Telephones', in Leiss (ed.) op cit.
- Levidow, L (2001) 'Precautionary uncertainty: regulating GM crops in Europe', *Social Studies of Science* 31 (6), pp.842-74
- Lewenstein, B (1992) 'The meaning of 'public understanding of science' in the United States after World War II', *Public Understanding of Scienc*e 1, 45-68
- Locke, S (1999) 'Golem science and the public understanding of science: from deficit to dilemma', *Public Understanding of Science* 8 pp. 75–92.
- Locke, S (2002) 'The Public Understanding of Science – a Rhetorical invention', *Science, Technology and Human Values*, 27(1) pp. 87-111
- Longino, H (1990) *Science as Social Knowledge.* Princeton and Oxford: Princeton University Press

- MacCracken, G (1988) *The Long Interview*, Qualitative Research Methods Series (13), London: Sage

- MacKenzie, D (1990) Inventing Accuracy: a historical sociology of missile guidance. Cambridge and London: MIT press,

- McLeod, KS (2000) 'Our sense of Snow: the myth of John Snow in medical geography', *Social Science & Medicine*, 50, pp. 923-935

- Maisch, D (2000) 'Setting radiofrequency/Microwave (RF/MW) exposure guidelines to protect workers and the public: Russia and the West in major conflict', EMFacts consultancy, http://www.emfacts.com/papers/russia.html, accessed 18th September 2003

- Maisch, D (2001) 'Mobile Phone Use: It's time to take precautions', *Journal of the Australian College of Nutritional and Environmental Medicine* 20 (1) pp. 3-10

- Manning, M and Densley, M (2001) 'On The Effectiveness Of Various Types Of Mobile Phone Radiation Shields', SARtest report, 0113, June 2001, prepared by SARTest Ltd for the DTI

- Martin, B and Richards, E (1995) 'Scientific knowledge, controversy and public decision making' in Jasanoff et al. (eds.), *Handbook of Science and Technology Studies.* London: Sage

- Mercer, D (1996) 'Understanding Scientific/Technical Controversy', Science and Technology Policy Research Group, University of Woolongong, Occasional Paper No.1, November 1996, http://www.uow.edu.au/arts/sts/research/STPPapers/Occpaper-1.html

- Mercer, D (2001) 'Overcoming Regulatory Fear of Public Perceptions of Mobile Phone Health Risks,' *Radiation Protection in Australasia,* 18, pp.84-94

- Mercer, D (2002) 'Scientific Method Discourses in the Construction of 'EMF Science': Interests, Resources and Rhetoric in Submissions to a Public Inquiry', *Social Studies of Science*, 32 (2) pp. 205-33

- Merton, RK (1987) 'From a Sociologists notebooks (Specified Ignorance)', *Annual Review of Sociology*, 13, pp. 1-28

- Michael, M (1992) 'Lay discourses of science: Science-in-general, science-in-particular, and self', *Science, Technology & Human Values*, 17 (3) pp. 313-33
- Michael, M (1996) 'Ignoring science: Discourses of ignorance in the public understanding of science', in Irwin and Wynne (1996) op. cit.
- Michael, M and Birke, L (1994) 'Enrolling the Core Set: The Case of the Animal Experimentation Controversy', *Social Studies of Science* Vol. 24, pp 81-95
- Michaelson, SM and Elson, EC (1996) 'Interaction of nonmodulated and pulse modulated radiofrequency fields with living matter: experimental results,' in Polk and Postow (eds.), *Handbook of biological effects of electromagnetic fields* (2nd Edition), Boca Raton: CRC press
- Millstone, E and Van Zwanenberg, P (2001) 'Politics of Expert advice: lessons from the early history of the BSE saga', *Science and Public Policy*, 28(2) pp 99-112
- Moore, A (2003) 'Democracy and Risk: The Case of MMR', paper presented to the panel 'States of Uncertainty: The Governance of Risk' at the Political Studies Association conference, University of Leicester, 15-17 April 2003
- NCRP (1986) National Council on Radiation Protection and Measurement: Biological effects and exposure criteria for radiofrequency electromagnetic fields. NCRP report No. 86
- Nelkin, D (1975) 'The Political Impact of Technical Expertise', *Social Studies of Science*, Vol. 5, pp. 35-54
- Nelkin, D (1979) Controversy: the politics of technical decisions. Beverly Hills: Sage
- NIEHS (1999) Report on Health Effects from Exposure to Power-Line Frequency Electric and Magnetic Fields, NIH Publication No. 99-4493
- NRPB (1982) Proposals for the Health Protection of Workers and Members of the Public against the Dangers of Extra Low Frequency, Radiofrequency and Microwave Radiations: A Consultative Document, Chilton, NRPB-CD82
- NRPB (1986) Advice on the Protection of Workers and Members of the Public from the Possible Hazards of Electric and Magnetic Fields with Frequencies Below 300 GHz: A Consultative Document, Chilton, NRPB-CD86

- NRPB (1989) Guidance as to Restrictions on Exposure to Time Varying Electromagnetic Fields and the 1988 Recommendations of the International Non-Ionizing Committee, Chilton, NRPB-GS11

- NRPB (1992) Electromagnetic Fields and the Risk of Cancer: Report of an Advisory Group on Non-Ionising Radiation, *Documents of the NRPB*: Vol. 3, Number 1

- NRPB (1993) Restrictions on Human Exposure to Static and Time-Varying Electromagnetic Fields and Radiation, *Documents of the NRPB*: Vol. 4, No. 5

- NRPB (1999) 1998 ICNIRP Guidelines for Limiting Exposure to Time-Varying Electric, Magnetic and Electromagnetic Fields (up to 300 GHz): NRPB Advice on Aspects of Implementation in the UK, *Documents of the NRPB*, Vol. 10, No. 2

- NRPB (2003) Consultation Document – Proposals for limiting exposure to electromagnetic fields (0-300 GHz), 1st May 2003, Chilton: NRPB

- Park, R (2000) Voodoo Science: The Road from Foolishness to Fraud. Oxford: Oxford University Press

- Park, R (2003) 'The Seven Warning Signs of Bogus Science', *The Chronicle Review*, January 31st, 2003, Vol. 49, Issue 21, http://chronicle.com/free/v49/i21/21b02001.htm, accessed Sept 19th 2003

- Patton, C (1990) *Inventing AIDS*. New York: Routledge

- Paxman, J (2002) *The Political Animal – An Anatomy*. London: Penguin

- Philip, M (1990) 'Michel Foucault', in Skinner, Q (ed.), *The Return of Grand Theory in the Human Sciences,* (Canto edition). Cambridge: Cambridge University Press

- Phillips (Lord), Bridgeman, J and Ferguson-Smith, M (2000) The BSE inquiry: Report: evidence and supporting papers of the inquiry into the emergence and identification of Bovine-Spongiform Encephalopathy (BSE) and variant Creutzfeldt-Jakob Disease (vCJD) and the action taken in response to it up to 20 March 1996. London: HMSO

- Picart, CJS (1994) 'Scientific controversy as farce: The Benveniste-Maddox Counter Trials', *Social Studies of Science* 24, pp. 7-37

- Pickering, A (ed.) (1992) *Science as Practice and Culture*. Chicago: University of Chicago Press.
- Platt, J (1988) 'What can case studies do?', *Studies in Qualitative Methodology*, Volume 1, pp. 1-23
- Postow, E and Swicord, M (1996) 'Modulated fields and "window effects"' in Polk and Postow (eds.), *Handbook of biological effects of electromagnetic fields* (2nd Edition). Boca Raton: CRC press
- Preece, AW, Iwi, G, Davies-Smith, A, Wesnes, K, Butler, S, Lim, E and Varey A (1999) 'Effect of a 915-MHz simulated mobile phone signal on cognitive function in man' *International Journal of Radiation Biology*, 75, pp. 447-456
- Radder, H (1998) Responses and Replies, The Politics of STS, *Social Studies of Science*, Vol. 28, No. 2, pp. 325-331
- Ravetz, J (1987) 'Usable Knowledge, Usable Ignorance', in Munn, R and Clarke, W (eds.) *Sustainable Development of the Biosphere*. Cambridge: Cambridge University Press (1986), reprinted in *Knowledge*, 9 (1987), pp.87-116
- Repacholi, MH, Basten, A, Gebski, V, Noonan, D, Finnie, J and Harris, AW (1997) 'Lymphomas in *Eμ-Pim1* transgenic mice exposed to pulsed 900 MHz electromagnetic fields', *Radiation Research*, 147, pp. 631-640
- Richards, E (1988) 'The Politics of Therapeutic Evaluation: The Vitamin C and Cancer Controversy', *Social Studies of Science*, 18, pp. 653-701
- Richards, E (1996) '(Un)boxing the monster', *Social Studies of Science*, 26 (2), pp. 323-56
- Rier, D (1999) 'The versatile caveat section of an epidemiology paper', *Science Communication* 21(1), pp. 3-37
- Rip, A (1987) 'Controversies as informal technology assessment', *Knowledge*, 8, pp. 349-371
- Rip, A (2003a) Afterword, in Misa, T, Brey, P and Feenberg, A (eds.) *Modernity and Technology*. Cambridge and London: MIT Press
- Rip, A (2003b) 'Constructing Expertise: In a Third Wave of Science Studies', *Social Studies of Science*, 33 (3) pp. 419-434

- Royal Society (1985) *Public Understanding of Science.* London: The Royal Society
- RSC (1999) A Review of the Potential Health Risks of Radiofrequency Fields from Wireless Telecommunication Devices: An Expert Panel Report prepared at the request of the Royal Society of Canada for Health Canada, RSC.EPR 99-1
- Schneider, JW (1985) 'Social problems theory: The constructionist view', *Annual Review of Sociology*, 11 pp. 209-29
- Shackle, GLS (1955) *Uncertainty in Economics.* Cambridge: Cambridge University Press
- Shackley, S and Wynne, B (1996) 'Representing uncertainty in global climate change science and policy: boundary-ordering devices and authority', in *Science Technology and Human Values* 21(3) 275-302
- Shapin, S (1982) 'History of Science and its Sociological Reconstructions', *History of Science*, Vol. 20, pp. 157-211
- Shapin, S (1992) 'Why the public ought to understand science-in-the-making', *Public Understanding of Science*, 1, pp. 27-30
- Shapin, S (1995a) 'Here and Everywhere: Sociology of Scientific Knowledge', *Annual Review of Sociology*, Vol. 21, pp. 289-321.
- Shapin, S (1995b) 'Cordelia's Love: Credibility and the Social Studies of Science', *Perspectives on Science,* 3, pp. 255-275.
- Shapin, S and Schaffer, S (1985) *Leviathan and the Air Pump.* Princeton, New Jersey: Princeton University Press
- Shermer, M (1997) Why people believe weird things: Pseudoscience, superstition and other confusions of our time. New York: Freeman
- Shrader-Frechette, KS (1995) 'Evaluating the Expertise of Experts', *Risk: Health, Safety and Environment* [Risk] 6, pp. 115-126
- Slovic, P (1987) 'The perception of risk', *Science* 236, 17 April 1987, pp. 280-285
- Slovic, P (1992) 'Perception of risk: Reflections on the psychometric paradigm' in Krimsky, S and Golding, D (eds.) *Social theories of risk.* Westport, Connecticut: Praeger

- Smithson, M (1989) *Ignorance and Uncertainty: Emerging Paradigms.* New York: Springer-Verlag
- Solomon, SH and Hackett, EJ (1996) 'Setting Boundaries between Science and Law: Lessons from Daubert v. Merrell Dow Pharmaceuticals, Inc.', *Science, Technology, & Human Values*, Vol. 21, No. 2, pp. 131-156
- Star, SL (1985) 'Scientific Work and Uncertainty', *Social Studies of Science*, 15, pp. 391-427
- Star, SL and Griesemer, JR (1989), 'Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology (1907-39)', *Social Studies of Science,* Vol.19 pp. 387-420
- Stilgoe, J (2000) 'Frankenstein Phones? – An investigation of the role of the media in the social construction of risk during the mobile phone debate', Unpublished MSc dissertation, University of Manchester
- Stilgoe, J (2001a) 'Some reservations about the gap concept', *Conservation Ecology* 5(2): r2. [online] URL: http://www.consecol.org/vol5/iss2/resp2
- Stilgoe, J (2001b) 'The Media and the Construction of Post-Normal Risk: The Health Effects of Mobile Phones', PREST Discussion paper series, Paper 01-06 http://les.man.ac.uk/PREST/Publications/DP_PDFs/PRESTDP01-06.pdf
- Stirling, A (1999) *On science and precaution in the management of technological risk*, An ESTO project report prepared for the European Commission – JRC Institute Prospective Technological Studies, Seville, IPTS
- Stocking, SH (1998) On drawing attention to ignorance, *Science Communication*, Vol. 20 (1) pp. 165-178
- Swiss Re (1996) *Electrosmog – A phantom risk.* Zurich: Swiss Reinsurance Company
- Szmigielski, S and Sobiczewska, E (2000) 'Cellular phone systems and human health – problems with risk perception and communication', *Environmental Management and Health*, 11(4), pp. 352-268
- TNO (2003) Effects of Global Communication System radio-frequency fields on Well Being and cognitive function of human subjects with and without

subjective complaints, TNO report, FEL-03-C148, http://www.ez.nl/beleid/home_ond/gsm/docs/TNO-FEL_REPORT_03148_Definitief.pdf

- Turner, V (1969) The Ritual Process, Structure and Anti-Structure. Chicago: Aldine
- Turney, J (1998) Frankenstein's Footsteps: Science, Genetics and Popular Culture. New Haven: Yale University Press
- Van der Sluijs, J, Van Eijndhoven, J, Shackley, S and Wynne, B (1998) 'Anchoring Device in Science for Policy: The Case of Consensus around Climate Sensitivity', *Social Studies of Science*, 28(2), pp. 291-323
- Van Zwanenberg, P and Millstone, E (2000) 'Beyond Skeptical Relativism: Evaluating the Social Constructions of Expert Risk Assessments', *Science, Technology and Human Values*, Vol. 25, no. 3, pp. 259-282
- Weinberg, A (1972) 'Science and Trans-science', *Minerva*, Vol. 10, pp. 209-22
- Wertheimer N, Leeper E (1979) 'Electrical wiring configurations and childhood cancer', *American Journal of Epidemiology*; 109, pp. 273-84
- Westrum, R (1978) 'Science and Social Intelligence about Anomalies: the case of meteorites', *Social Studies of Science*, 8, pp. 461-493
- WHO (2003) *Establishing a dialogue on risks from electromagnetic fields*, Geneva, World Health Organization publications, http://www.who.int/peh-emf/publications/EMF_Risk_ALL.pdf
- Wildavsky, A (1979) Speaking Truth to Power: The Art and Craft of Policy Analysis. Boston: Little Brown
- Winch, P (1958) *The Idea of a Social Science and Its Relation to Philo*sophy. London: Routledge and Kegan Paul
- Wolpert, L (1992) *The Unnatural Nature of Science.* London: Faber and Faber
- Woolgar, S (ed.) (1988) Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge. London; Sage

- Woolgar, S (1989) 'What is the Analysis of Scientific Rhetoric for? A Comment on the Possible Convergence Between Rhetorical Analysis and Social Studies of Science', *Science, Technology and Human Values*, Vol. 14, No. 1, pp. 47-49

- Woolgar, S. (1991) 'Configuring the User: The Case of Usability Trials', pp57-99 in Law, J. (Ed.), *A Sociology of Monsters: Essays on Power, Technology and Domination.* London: Routledge

- Wynne (1982) Rationality and ritual – The Windscale inquiry and nuclear decisions in Britain. BSHS Monographs, no. 3

- Wynne, B (1987) 'Uncertainty – Technical and Social' pp. 95-115 in Brooks, H and Cooper, CL (eds.), *Science for Public Policy.* Oxford: Pergamon Press

- Wynne, B (1988) 'Unruly Technology: Practical Rules, Impractical Discourses and Public Understanding', *Social Studies of Science*, Vol. 18, No. 1, pp. 147-167.

- Wynne, B (1989) 'Frameworks of rationality in risk management: Towards the testing of Naïve sociology', pp. 33- 47 in Browne, J (Ed.) *Environmental Threats, Perception analysis, and management.* London: Bellhaven.

- Wynne, B (1991) 'Knowledges in Context', *Science, Technology & Human Values*, 16(1), pp. 111-121.

- Wynne, B (1992a) 'Uncertainty and environmental learning- Reconceiving Science and Policy in the Preventive Paradigm', *Global Environmental Change* 2 (2), pp. 111-127

- Wynne, B (1992b) 'Carving Out Science (and Politics) in the Regulatory Jungle' (A review of "The 5th Branch" by Sheila Jasanoff), *Social Studies of Science*, Vol. 22, pp. 745-58

- Wynne, B (1993) 'Public uptake of science: a case for institutional reflexivity', *Public Understanding of Science* 2, 321-37

- Wynne, B (1995) 'Public understanding of science', in Jasanoff et al, (eds.) *Handbook of science and technology studies.* London: Sage

- Wynne, B (1996a) 'SSK's identity parade: signing-up, -off and –on', *Social Studies of Science*, 26, 357-391

- Wynne, B (1996b) 'May the sheep safely graze? A reflexive view of the expert-lay knowledge divide' in Lash, S, Szerzynski, B and Wynne, B (eds.) *Risk, Environment and Modernity.* London: Sage Publications

- Wynne, B (1996c) 'Misunderstood misunderstandings', in Irwin and Wynne (eds.) op. cit.

- Wynne, B (2001) 'Managing Scientific Uncertainty in Public Policy', presented at a conference "Biotechnology and Global Governance: Crisis and Opportunity", Cambridge, April 26-28th 2001, URL: http://www.wcfia.harvard.edu/conferences/biotech/Backgroundpapers.html

- Wynne, B (2003) 'Seasick on the Third Wave: Subverting the Hegemony of Propositionalism', *Social Studies of Science*, 33 (3) pp. 401-417

- Wynne, B and Mayer, S (1993) 'How science fails the environment', *New Scientist,* 5 June 1993, pp. 33-5

- Yearley, S (1994) 'Understanding Science from the Perspective of the Sociology of Scientific Knowledge: An Overview', *Public Understanding of Science*, 3 pp. 245-258

- Yearley, S (2000) 'Making systematic sense of public discontents with expert knowledge: two analytical approaches and a case study', *Public Understanding of Science*, 9(2) pp. 105-122

- Yin, R. K. (1994). *Case study research: Design and methods* (2nd edition.). London: Sage

- Zavestoski, S, Brown, P, Meadow, L, McCormick, S and Mayer, B (2002) 'Science, Policy, Activism, and War: Defining the Health of Gulf War Veterans', *Science Technology and Human Values*, Vol. 27 (2) pp. 171-205

- Zehr, S (1994) 'Flexible interpretations of acid rain and the construction of scientific uncertainty in political settings', *Politics and the life sciences* 13(2) pp. 205-216

- Zehr, S (1999) 'Scientists' Representations of Uncertainty', Chapter 1 in Friedman, SM, Dunwoody, S and Rogers, C (eds.) *Communicating Uncertainty: Media Coverage of New and Controversial Science.* Mahwah, NJ: Erlbaum.

- Zehr, S (2000) 'Public representations of scientific uncertainty about global climate change', *Public Understanding of Science*, 9(2), 85-103

- Zmirou, D (et al) (2001) Mobile Telephones, Base Stations and Health. Current State-of- Knowledge and Recommendations. A Report to the Director General of Health of France. Paris, http://www.sante.gouv.fr/htm/dossiers/telephon_mobil/resum_uk.htm (English summary)

Appendix 1: List of interviewees

Date	Name	IEGMP Role	MTHR Role	Other Info
22/10/2001	Dr Alan Preece	Verbal Evidence		
08/11/2001	Alasdair Philips	Verbal Evidence		Powerwatch
19/11/2001	Dr David De Pomerai	Verbal Evidence	Award-holder	
20/11/2001	Dr Sheila Johnston	Written Evidence		
28/11/2001	Dr John Tattersall	Verbal Evidence		
18/12/2001	Professor Michael Rugg	Member		
01/02/2002	Marie-Noelle Barton	Member[1]		
14/02/2002	Professor Anthony Swerdlow	Member	Award-holder	AGNIR member[2]
...and 12/08/2003	(repeat interview)			
19/02/2002	Professor Olle Johansson			
25/02/2002	Professor Kjell Hansson Mild		Committee Member	
17/04/2002	Professor Sir William Stewart	Chair	Committee Member/Chair[3]	
07/05/2002	Roger Coghill	Written Evidence		
21/05/2002	Dr John Stather	Secretariat		NRPB[4]
21/05/2002	Dr Mike Clark			NRPB[5]
21/05/2002	Dr Zenon Sienkiewicz		Committee Member	NRPB
29/05/2002	Alan Meyer	Written Evidence		Solicitor[6]
05/06/2002	Professor Colin Blakemore	Member	Committee Member	AGNIR member

Notes

1 Lay member of the IEGMP

2 ... and chair of AGNIR since 2003

3 Chair of MTHR committee until 2002, when he became chairman of both the NRPB board and the newly-formed Health Protection Agency.

4 NRPB deputy director

5 NRPB scientific spokesperson

6 Legal advisor and solicitor for anti-mobile phone groups, including Northern Ireland Families Against Telecoms Towers (NIFATT) and Mast Action UK

Date	Name	IEGMP Role	MTHR Role	Other Info
08/07/2002	Dr Gerard Hyland	Verbal Evidence		
05/09/2002	Graham Barber			IEE[7]
16/09/2002	Mike Dolan	Verbal Evidence		FEI[8]
04/10/2002	George Hooker and…	Observer	Observer	Department of Health
"	…Hilary Walker (Joint Interview	"	"	"
09/10/2002	Professor Paul Elliot		Award-holder	
30/10/2002	Professor Ted Grant		Committee Member	NRPB[9]
08/11/2002	Dr Simon Gerrard		Committee Member[10]	
15/01/2003	Dr Leika Kheifets			WHO[11]
18/02/2003	Les Wilson	Verbal Evidence		Microshield
27/02/2003	Professor Lawrie Challis	Member	Committee Member/ Chair[12]	
12/03/2003	Graham Worsley	Observer		Department of Trade and Industry
14/03/2003	Professor Sir Richard Doll			AGNIR chair[13]
14/05/2003	Peter Harrison	Verbal Evidence		Nokia[14]

[7] Institute of Electrical Engineers

[8] Spokesperson for the Federation of the Electronics Industry, representing mobile phone network operators (now IntellectUK)

[9] NRPB board member, 1989-1997

[10] Centre for Environmental Risk, University of East Anglia

[11] Took over from Michael Repacholi as director of the World Health Organisation's International EMF programme 2001-2003

[12] Chair of MTHR committee from November 2002

[13] AGNIR chair from 1990 to 2003 (retired)

[14] Head of EMF issues at Nokia and past chair of the Mobile Manufacturer's Forum

Appendix 2: Typical interview questions/topics

Introduce my study – what I want to investigate and why…

1) **General**
   - Could you explain how you became involved in the mobile phones health issue?
   - Could you give a brief summary of what you think about the issue and the way it has been handled?

2) **Science** – very open-ended
   - What research have you conducted/are you conducting, and what is its relevance to mobile phone use?
   - What led you to conducting research in this area?
   - Does your research suggest any possible health effects?
   - What role, if any, did you play in the IEGMP? (e.g. membership, providing evidence)
   - What, in your opinion, is the general feeling amongst scientists about the mobile phone debate?

3) **Uncertainty** – Introduce this: "Some commentators have pointed to significant scientific uncertainties in this area"
   - What is the extent of scientific uncertainty about health effects in the case of mobile phones?
   - How is this uncertainty dealt with?
     - (Can uncertainties such as this be adequately quantified?)
   - Are we more or less uncertain about the health effects of mobile phones than we were 10 years ago?
   - Were the precautionary recommendations of the Stewart report justified, in your opinion?
   - For industry… How does the mobile phone industry deal with such levels of scientific uncertainty?

4) **Policy for mobile phones**
   - Do you think that the current policy and recommendations for mobile phone use are adequate?
   - Do you think that the Stewart report represent a significant shift in policy? If so, how?
   - Who do you think should provide funding for research into mobile phones health effects? – Stewart report recommended the companies and public sector (50/50)

5) **Public participation**
   - How do you think the public should be informed about the mobile phones health issue?

- What role, if any, should the public play within expert investigations such as the IEGMP?
- How might greater public participation change how science is conducted, communicated etc.?

6) **Anecdotal evidence** – Introduce...

> **IEGMP paragraph: 1.70** We recommend that in a rapidly emerging field such as mobile phone technology where there is little peer-reviewed evidence on which to base advice, the totality of the information available, including n*on-peer-reviewed data and anecdotal evidence*, be taken into account when advice is proffered.

"The Stewart report recommended that anecdotal evidence be used in the provision of scientific advice. This seemed particularly interesting to me."

- What do you understand by the term 'anecdotal evidence' in science?
- What role do you think it plays in the course of normal scientific inquiry?
- How has this role changed over the last two decades (or so)?
  - Why? (Do you think this is because of recent controversies such as BSE, GM foods?)
- Do you agree that it should have been mentioned in the Stewart report?

As another prompt:

> **IEGMP paragraph: 1.58** We recommend that a substantial research programme should operate under the aegis of a demonstrably independent panel. The aim should be to develop a programme of research related to health aspects of mobile phones and associated technologies. This should complement work sponsored by the EU and in other countries. In developing a research agenda the peer-reviewed scientific literature, *non-peer reviewed papers and anecdotal evidence* should be taken into account.

Are there any final comments you would like to make about the issues we have discussed?

Appendix 3: NVivo coding scheme

Node Listing

Free Nodes
(1) Activism
(2) Compliance
(3) Electrosensitivity
(4) Electromagnetic Compatibility
(5) Funding
(6) History/Future of Debate
(7) Human Volunteers
(8) 'Independence'
(9) Industry
(10) Measurement/Dosimetry
(11) Media
(12) Other Actors
(13) Shields, Hands-free
(14) Technology (Inc TETRA, 3G)
(15) 'The Public' (Concern/Consultation)

Tree Nodes
(1) /Anecdotal Evidence
    (1 1) Definition
    (1 2) Importance
    (1 4) Epistemological Status
    (1 5) Social or Policy Status
    (1 6) Source
    (1 7) Subjective/Reported Symptoms

(2) /Ignorance, Uncertainty
    (2 1) For Policy
    (2 2) Certainty
    (2 3) Consensus

(3) /Dichotomies
    (3 1) Thermal/Non-Thermal
    (3 2) Dose Rate/Cumulative Dose
    (3 3) Distinction between Phones and Masts -
    (3 4) Distinction between Effects and Risk
    (3 5) Acute/Chronic Effects
    (3 6) Analogue/Pulsed, Digital Radiation
    (3 7) Humans/Animals

(4) /Policy
    (4 1) Precaution (Inc Risk Assessment/Management)
    (4 2) Opinion of MTHR, Other Research
    (4 3) SAR, Standards, Guidelines
    (4 4) Opinion of IEGMP

(4 4 1) Recommendation about Children
(4 5) Opinion of NRPB, AGNIR
(4 6) Context of Previous Policies, Issues (e.g. BSE, GMOs)
(4 7) Planning Issues
(4 8) Government
(4 9) Economic Implications
(4 10) Policies, Advice Abroad
(4 11) Labelling

(5) /Risk
(5 1) To Subgroups

(6) /Science (Definitions (What 'science' is), Differences between Physics, Biology etc.)
(6 1) Replication
(6 2) Mechanisms
(6 3) Epidemiology
(6 4) Peer Review