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Understanding scientific/technical controversy

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**Publication Details**

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Abstract
The material in this paper originates from a Doctoral thesis, I and lectures presented in the University of Wollongong Science and Technology Studies Department undergraduate subject 'Scientific Controversies' in 1993. During the preparation of this material, I noted that there was an absence of overviews of the literature, sufficiently clear or detailed, to guide the uninitiated through the subtle, but philosophically important, differences between the main currents of academic thought on Scientific and Technical Controversy (STC). The following 'map of the literature' has been designed to try to fill this gap and also to raise awareness of some of the valuable intellectual possibilities that can arise from a critical cross-fertilization of differing theoretical approaches, something which rarely occurs at present.

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1 Introduction: mapping the literature on scientific and technical controversy

The material in this paper originates from a Doctoral thesis, and lectures presented in the University of Wollongong Science and Technology Studies Department undergraduate subject 'Scientific Controversies' in 1993. During the preparation of this material, I noted that there was an absence of overviews of the literature, sufficiently clear or detailed, to guide the uninitiated through the subtle, but philosophically important, differences between the main currents of academic thought on Scientific and Technical Controversy (STC). The following 'map of the literature' has been designed to try to fill this gap and also to raise awareness of some of the valuable intellectual possibilities that can arise from a critical cross-fertilization of differing theoretical approaches, something which rarely occurs at present.

Taking as a relevant sample, the literature which specifically advertises that one of its main concerns is the study of STCs, six main approaches were identified:

- scientific controversy as political controversy
- technocratic politics
- historico/narrative
- facts/versus values
- closure studies
- contemporary sociology of scientific knowledge.

Studies were able to be pigeon-holed on the basis of their relationship to the following three criteria:

- the models and choice of analytical tools used to describe and explain STCs and why they occur;
- what broader principles the analyst hoped to draw to the readers attention from the study of STCs; and
- the actual choice of subject matter within any given STC or set of STCs, or what types of STCs come to be emphasised in actual studies.
It should also be acknowledged from the outset that this review has been shaped by a 'post-Kuhnian' orientation towards understanding science. Post-Kuhnian approaches can be characterised by their scepticism for 'grand' philosophical theories of scientific method and their concern with relating scientific knowledge to its social context. Of the approaches to STCs to be surveyed, 'Sociology of Scientific Knowledge' (SSK) -influenced approaches capture a post-Kuhnian ethos best. Because of this, I will discuss their strengths and weaknesses in more depth than other approaches to STCs. It should, however, be noted that this does not exempt SSK from criticism nor acknowledging the merits of alternative approaches.

The choice of broad multi-factorial criteria was made to avoid the tendency displayed by some post-Kuhnian orientated studies in STS to place too much emphasis on epistemological questions at the expense of investigating the broader links between the micro-social shaping of knowledge claims and macro-social contexts. Whilst any discussion of STC involves considering epistemological questions, focusing on epistemology exclusively can blur some important distinctions between the different political implications of various epistemologies and studies which may not explicitly address epistemological issues. The following commentary will examine the epistemological views of various approaches to STC but will place such discussion in the context of the broader categories noted above.

It is important to anticipate a criticism, following from the above, that such an approach is flawed because it makes comparisons between approaches with fundamentally different views about the epistemological status of science. In response, it could be argued that such a criticism takes the 'boundary maintenance' rhetoric used by competing approaches to STCs on face value. In fact, one of the sub-themes that appears in the following survey is the importance of evaluating approaches STCs according to both their programmatic rhetoric and also the types of issues and subject matter actually emphasised in practice.

Before proceeding with this review of the literature, it is worthwhile to briefly discuss one of the few recent attempts to overview the study of STCs: Martin and Richards (M & R), 'Scientific Knowledge, Controversy, and Public Decision-Making'. Despite their considerable status and important contributions to the field their paper displays a number of defects which weaken its value as an accurate and reliable overview of the literature.
M & R frame their overview in keeping with their aims of enhancing public participation in scientific decision-making and divide up the literature with a strong concern for the stated political aspirations of various studies of STCs. Four loosely bounded 'ideal type' categories are identified: Positivist; Group Politics approach; the Sociology of Scientific Knowledge (SSK) or Constructivist approach; and, the Social Structural approach. The implications for public decision-making concerning science and technology of these various approaches are compared and contrasted and an 'integrated approach' recommended.

From the start, this approach is weakened by a lack of discussion explaining the criteria by which their 'ideal' types have been chosen. This lack of theoretical reflexivity also helps explain a further problem which involves the mis-classification of some important contributions to the STC literature. Two important examples of this involve M & R's mis-classification of certain studies as 'positivist', and other studies as 'social structural'. In the first example, M & R take the work of Engelhardt and Caplan as an exemplar of Positivist approaches which supposedly separate the 'social' from the 'scientific' in the study of STCs.

A continuing controversy is considered actually to be two controversies cognitive controversy (a controversy over knowledge) and a social controversy (a controversy over non-scientific issues) (Engelhardt and Caplan, 1987). The cognitive controversy can be settled by the supposedly tried and true scientific method, whereas the social controversy may persist indefinitely.

This represents an over-simplification of Engelhardt and Caplan's position. Whilst Engelhardt and Caplan do display a positivist pre-occupation with scientific method (which I will comment on and critique at a later point) described under the heading of 'strict sound argument closure', their approach does offer some potential to consider the social contingencies involved in the construction of scientific knowledge claims. They do this by introducing the notion of 'broad sound argument closure': this involves acknowledgment by the analyst of the way participants in STCs frame what is 'social' as opposed to what is 'epistemic' (scientific). So whilst it is true that such approaches to STCs ultimately separate out the 'social' from the 'scientific' at the level of analysis of 'strict sound argument closure', there is considerable room at the level of analysis of 'broad sound argument closure' for studies of STCs to investigate many similar concerns to approaches such as SSK.
Another example of mis-classification involves the location of STC writers such as Dickson and Nelkin into the so-called 'group politics' approach:

This approach to scientific controversy concentrates on the activities of various groups, corporations, citizens' organizations, and expert panels. Essentially the controversy is dealt with as any other form of politics in the pluralist interpretation of liberal democracy: a process of conflict and compromise involving various groups contending in a political marketplace.  

This categorization is puzzling. In numerous places Nelkin explicitly discusses how STCs constitute a distinctive new form of politics. She is particularly preoccupied with reliance across the 'complete spectrum' of political views; with the legitimation of decision-making by technical experts; and with the effective decline in the scope of democratic decision-making as political issues are redefined as technical. Dickson, like Nelkin, is concerned with the rise of technocratic rationality and its shaping of STCs. Dickson quite openly acknowledges his debt to writers concerned with the distinctive shaping of modern politics by science and technology such as Mumford, Ellul and Habermas. Dickson has also regularly attempted to attach his concerns with technocracy to Marxist writing on 'social class' and 'the labour process'. M & R's characterization of Dickson as a political pluralist is clearly inadequate.

Apart from these problems an important question is: how well does M & R's own work fit in to their prescriptive overview of the literature? In the concluding part of their paper they describe the need to go beyond any of the single approaches. They identify and they prescribe the need to develop 'integrated approaches' to the analysis of STCs. Integrated approaches involve the mixing and matching of styles of analysis depending on the analysts' aims. Ironically, the approach adopted in most of M & R's own work follows a different pattern. Rather than pick and choose between approaches their work proceeds by first critiquing Positivist styles of analysis, taking SSK approaches as a pre-condition for their later consideration of broader politics. Both M & R (particularly Martin) have previously argued that the adoption of SSK-styled epistemology intrinsically challenges the dominant political power structures in contemporary western society and facilitates better public participation in STCs (see further discussion pp.50-51). This theme, quite central to their other work, is not explicated in relation to their promotion of so-called integrated approaches. As I will argue in my conclusion, integrated approaches may have certain benefits but
require a more consistent and carefully argued rationale than that offered by M & R.

Overall M & R's brief overview of the STC literature does not adequately capture the important differences between writers who problematize science and technology as raising novel political questions (technocratic politics) and those who emphasise continuity between politics of science and technology and politics more generally. Their approach also fails to credit the potential diversity between approaches which share positivist epistemologies and, ironically, fails to give adequate credit to the value of the contributions made to the study of STCs by their own predominantly SSK orientated case studies.¹⁰
Although disputes among experts sometimes expand to involve a more diverse mix of participants, often scientific and technical controversies are initiated by those without technical backgrounds. In these cases, technical experts may only be called late in the disputes when additional resources are sought to add strength to a position. The role played by other parties, such as government, is influenced by the subject of the controversy and the form the controversy takes.


In 'scientific controversy as political controversy' STCs are interpreted as the by-product of political conflict. In such approaches things like the micro-politics of the scientific sub-cultures are only given attention so far as they might reflect broader 'political realities' as the analyst deems fit to define them. Of the numerous possibilities for analysing STCs as political controversies, most studies can be considered as falling into three main categories:

- studies drawing from pluralist/liberal political models emphasising the importance of the formation and operation of social movements;

- Marxist and structural political models emphasising social class inequalities and the role of capitalist corporations, ideology, and the state;

- gender politics studies drawing from feminist studies of science which analyse controversies through the lens of gender inequalities and the status of science in a patriarchal society.

A good example of the first category can be drawn from the resource mobilization theory concerned with the study of the politics of social movements as put forward by Petersen and Markle. In a 1989 review article 'Controversies in Science and Technology' they argue both that
controversies are 'an inevitable product of Western democracy [and] that science and technology cannot neatly be demarcated from other social institutions.'

This vision of STCs as being essentially political in nature raises doubts about the pertinence of forms of analysis that emphasise controversies as mainly being the by-product of expert knowledge based disagreements. This approach also argues that there are limitations in approaches that view STCs as the by-product of social problems flowing from the impacts of inherent features of modern science and technology. In relation to the former point, Petersen and Markle highlight the importance of the initiation and shaping of disputes by non-experts:

> Although disputes among experts sometimes expand to involve a more diverse mix of participants, often scientific and technical controversies are initiated by those without technical backgrounds. In these cases, technical experts may only be called late in the disputes when additional resources are sought to add strength to a position. The role played by other parties, such as government, is influenced by the subject of the controversy and the form the controversy takes.

In relation to both points, but especially the latter, Petersen and Markle present an allegory to illustrate why it is plausible to consider political forces as the key to understanding why some scientific/technical issues become controversies rather than others. They ask the reader to imagine a Martian, trained in science policy, shortly after her arrival on earth. While taking a cab from the rocket port, she notes the lack of safety factors of the taxi-cab and other automobiles. She also observes the obvious social and environmental problems involved in the use of fossil fuels.

> Surely, she concludes, fossil fuels and auto-safety must be two of the most controversial science related issues in contemporary America.

Petersen and Markle point out that, of course, this is not the case. Yet both issues should, we might expect, be scientific/technical controversies. This leaves us with political questions about why controversies occur. It is not enough to look to intrinsic features of technology and science and their relationship to society.

> How did our expert go wrong? Why do some issues related to science and technology develop into public disputes while, others, of seemingly equal or even greater importance, do not?
In short, Petersen and Markle emphasise that STCs are best explained in political terms. They appeal to theories of 'resource mobilisation' within studies of the dynamics of social movements. In such an approach, it is assumed that in contemporary society there are numerous grievances: as such controversies could develop at almost any time and in relation to numerous issues. The reason that controversies do not arise this way, is that the vast majority of the populace lack resources to turn grievances into controversies. Because of the unequal distribution of social power in contemporary society, one of the few contexts where such resources are able to be attached to grievances is through the functions of social movements. In such contexts, of particular importance are the role of social movement professionals (or issue entrepreneurs) and the co-opting and influencing of elites. Under the pressure of political efficacy, social movement activities recast specific technical scientific disagreements in terms of social/political value questions. This suggests that STC is foremost political controversy, shaped, guided and instigated by social movements.

Petersen and Markle raise a number of examples of specific controversies to support their claims. They discuss, for instance, how cultural and political differences between the US and Great Britain led controversy over oestrogen replacement therapy to take on a totally different character in each respective national culture, the difference being both in the nature of the politics involved and the way knowledge claims were linked to them.¹⁶

Overall, whilst social movement approaches to controversy accept that the study of STCs can be valuable in exposing the social choices sometimes obscured in scientific decision-making and that controversies provide a form of technology assessment,¹⁷ they still maintain that STCs fit into a continuum of science, technology and society - that they are social political controversies ahead of scientific technical controversies sui generis.

As well as 'social movement politics', another example of 'controversy as politics' can be found in some Marxist orientated studies. Whilst there is no overarching programme of Marxist studies addressed directly to the problem of STC, some hints of what such an approach might look like can be found in the work of Levidow¹⁸, Figlio,¹⁹ Young,²⁰ and the Radical Science movement.²¹
Levidow, for instance, analyses the STC surrounding the nuclear accident at Three Mile Island. Beneath technical disagreement over what constituted safe exposure levels for 'fall-out', and contradictory interpretations of what levels local people were already exposed to, a more important force at play was the way 'authorities' came to define the crisis in the first place. Rather than acknowledge that, potentially, all radiation exposures were harmful to the public, debate came to be conducted in terms of potential exposures relative to other measurements, such as medical x-rays and forms of background radiation.

This fetishism of 'millirems per person', appealing to a negative 'possessive individualist' attitude towards possible damage, helped the state to perform its crucial tasks: to contain the immediate threat of popular disturbance, especially a break in production, as well as general distrust in the government itself; and to protect the nuclear industry from subsequent attack for the health hazards inherent in it. Thus the industry was able to congratulate itself afterwards for having prevented a core meltdown; in this way the state was able to protect capital's interest in nuclear power.\textsuperscript{22}

According to Levidow this so-called 'ideology of safe levels' operated at two political levels. At the first superficial political level, the state could misrepresent what actually constituted safe exposures. For instance, power companies experts argued that exposure from the accident was no more than a dental x-ray. This, of course, sparked a heated STC. There were a number of other experts willing to argue that exposure 'in reality' was more like 3 to 10 dental x-rays or greater. Whilst this challenged the trustworthiness of the authorities, this STC was still 'operating' at a superficial, politically-captured level.

Even that critical commentary, effectively calling the authorities liars, did so within the authorities' own fetishized terms of reference; it disputed quantitatively the government's ultimately meaningless comparison of two different processes: infrequent 30-second exposures to a section of the jaw, and an 'equivalent' total concentration of exposure to more cancer-prone parts of the body spread over several days. The official claims were not merely quantitatively false in content but also ideological in form representing the (largely hidden) organic damage as some finite, familiar thing popularly associated with good health ...\textsuperscript{23}

The second deeper (qualitative) political level that the controversy 'operated' at involved the symbolic power of the abstract category of 'safe
levels'. This category was constructed and deployed by the state and the forces of capital so as to shape the controversy to suit their own interests.

Through the scientific terms of reference it invoked, the state was able to represent the low-level radiation exposure as 'part of the facts of life'. This not-uncommon feature of the nuclear industry - an inherent contradiction between the economic profitability and public acceptance of nuclear power - thus became a merely contingent conflict: the monitoring of 'safe level'. In this way the state could appear to be protecting people from the excesses of the industry while really protecting capital's interests in nuclear power: a sophisticated exercise in managing the contradictions inherent in the nuclear industry.\textsuperscript{24}

In Marxist approaches then, STC can be analysed not merely in the narrow terms of the expert disagreements but, rather, in the context of the social relations of capitalism in which scientific/technical terms are negotiated and given practical meaning. As Levidow points out, quoting Marx from the Grundrisse:

... even the most abstract categories - despite their validity for all epochs (precisely because of their abstractness) - are nevertheless, in the specific character of this abstraction, themselves likewise a product of historical relations, and possess their full validity only for and within these relations.\textsuperscript{25}

This does not necessarily indicate that the natural world is some kind of social construct, but rather that the negotiations of the meanings and implications of scientific/technical knowledge will be refracted through the social reality of the class relations of bourgeois society. STC then can be read as one of the numerous forms of class politics.\textsuperscript{26}

In a similar vein, Figlio discusses the medico-legal debate surrounding the so-called 19th Century miners' disease, Nystagmus, putting the issue as follows:

I shall analyse the appearance of the observers of the disease and the socio-economic and political meaning of the disease, as mutually constitutive aspects of the same history. As a social event miners' Nystagmus has a history at one with the emergence of the social space within which its observation could occur. In this latter sense, the pre-existence of the disease as a 'natural' object which attracted medical scrutiny and characterisation - the growth of medical knowledge - is beside the point historically.\textsuperscript{27}
Similar to Marxist approaches discussed above, feminist orientated studies of STCs are not unified as to the specific details of how STCs should be best studied. Part of this diversity can be accounted for by the vigorous but recent flood of interest in 'the science question in feminism'. Feminist epistemologies of science reflect this diversity. Whilst some approaches have adopted biological essentialist perspectives emphasising the need to reshape science to incorporate the intrinsic biological cognitive attributes of women, others have incorporated sociological and biological ideas together into what Harding has described as 'feminist standpoint epistemology' which is based on the argument that men's dominating position in social life results in partial and perverse understandings, whereas women's subjugated position provides the possibility of more complex and less perverse understandings.

Other feminist positions have been more interested in locating and deconstructing the ways gender relations shape the epistemology of science in more specifically situated terms with care for the detailed historical and social contexts in which scientific knowledge is being constructed. Others yet again, have been less interested in the epistemological deconstruction of science, being more concerned with (liberal political) issues surrounding the levels of participation of women in science and ensuring a fairer representation of the historical contributions of women to science.

Apart from broader concerns with the degree to which, and in what ways, science can be seen as a form of patriarchal knowledge, and concern with the involvement by women in, and shaping by women of, science and technology, there have been a number of controversial areas of modern science and technology which have been subject to feminist styles of analysis ahead of others. Medical and psychiatric knowledge, in particular, have constituted sites where the construction of women's bodies and women's behaviour by patriarchal society could be made especially visible:

... the concern with gender analysis of scientific knowledge can be traced back to the women's health movement that developed in Britain and America during the 1970s. Regaining knowledge and control over women's bodies - their sexuality and fertility - was seen as crucial to women's liberation. Campaigns for improved birth control and abortion rights were central to the early period of second wave feminism. There was a growing disenchantment with male medical theories and practices.
Other areas of science such as new reproductive technologies, environmental debates and peace studies have also figured prominently in feminism/science discourse.35

Overall then, in 'controversy as politics' approaches to STC, disagreements can be accounted for, and explained, in numerous ways. Typically themes which arise involve:

- the shaping of controversies and their outcomes along lines of class, race, gender, etc.;

- the capture of technical and scientific experts into political programmes;

- the mobilisation of resources by social movements, etc.;

- analysis of the strategies which interest groups and participants adopt to attempt to 'win' controversies.

In such explanatory schemes epistemological issues surrounding science do not always need to take a 'high profile' nor be consistent between similar modes of political analysis. For instance, all of the following epistemological positions can be shown to be consistent with putting political analysis foremost in the manner described above:

- emphasise the shaping of knowledge claims as the by-product of 'political epistemological' factors, according to the analyst's version of political epistemology, i.e. Marxist or feminist, etc.;

- adopt a 'realist epistemology' but acknowledge the inter-weaving of science and political questions in practical contexts, i.e. the important subjects of controversies and disagreements are unlikely to be amenable to simple epistemological analysis by their very nature;

- re-define epistemological questions by putting them in to the background by emphasising, in the context in question, intrinsic problems of uncertainty. Best political practice, in the context of uncertainty, replaces epistemologically based concerns which are pre-occupied with 'knowledge closure' above practice.
Overall, in the 'STC as political controversy' approach, the key processes observed in scientific/technical disagreements will chiefly be those reflecting either the capture of scientists to pursue what are political aims of social interest groups, or the shaping of scientific/technical claims by the demands of class or gender forces. In these approaches, controversies are seen as the outcome of problems in social and political structures, hence they can be viewed in the light of being positive things bringing to air implicit political conflict that may otherwise be concealed.

From a 'post-Kuhnian' perspective, these approaches to STC display a weakness in frequently failing to evaluate the more detailed content of scientific disagreements. Scientific disagreements tend to be seen as symptoms of problems located 'elsewhere'. The politics of science are too easily 'read off' politics generally. This can mean that, at times, political analysis starts 'long after' a great deal of politics has already occurred concerning negotiating/constructing the content of scientific/technical knowledge claims. Whilst such approaches might capture political aspects of the knowledge claims in question in relation to, for instance, class and gender biases, they might easily miss the more micro-political shaping of claims specific to particular times and places, sub-cultures and knowledge contexts.
... a ubiquitous question woven throughout the variety of disputes over science and technology is who should control crucial policy choices? Reflected in this question is the fear that complex technological advances with far reaching consequences have changed the nature of the decision-making process, that scientific approaches to public policy have encroached on the traditional political models, and that deference to scientific knowledge threatens democratic principles reducing public control of crucial policy choices.


'Technocratic Politics' approaches identify scientific-technical disagreements as the by-products of politics, but not politics broadly understood. Rather, they are examples of the special political questions intrinsic to, and shaped by, qualities of modern science and technology. In particular, there is concern with the roles of scientific/technical knowledge in influencing what are seen as qualitative changes to the nature of modern social structures and politics. Within these approaches, the roots of most technical and scientific disagreements can be found in unresolved difficulties surrounding the attempt to define the 'legitimate' boundaries between science, technology and society.\(^{36}\)

The boundaries between science technology and society become blurred for numerous reasons:\(^{37}\)

- science is increasingly drawn upon as a source of rationality and legitimacy for decisions in modern society;\(^{38}\)
- science and technology have increasingly come to mediate day-to-day life experiences - as such there is a growing material dependence on the opinions of scientific and technical experts;\(^{39}\)
- there is a growing tendency for scientific-technical explanations and styles of reasoning to spread into previously intangible areas of human life;\(^{40}\)
• in political or social disputes there is an irresistible tendency for many disputes to come to be defined in technical terms with scientific 'facts' converging with political 'values' - this situation creates political complexities as attempts to separate scientific 'facts' from political 'values' founder because of the tendency for biases to be built into, or in fact even be necessary to, the process of collecting and interpreting data in science required for policy purposes.\textsuperscript{41}

Problems with defining the boundaries between science, technology and society are amplified by the emergence of persistent areas of conflict which are perceived to be intrinsic to modern science and technology.\textsuperscript{42} These include:

• Fear of Risk. The growth of invisible, uncertain, unfamiliar, as well as involuntary and 'delayed threat' risks. Recombinant DNA technology and nuclear power exemplify these new kinds of risks. Institutionalised scientific disagreement and uncertainty mean that increasing evidence, rather than allaying fears, promotes difficult value questions in relation to acceptable levels of risk, often challenging the trustworthiness and responsibility of public institutions.\textsuperscript{43}

• Fear of Misuse. The growth of applications of science with uses beyond their initial intended ones which may lead to harmful consequences, for instance, the potential for genetic engineering to be used in the hands of fascist governments to promote eugenics.\textsuperscript{44}

• Moral and Ethical Concerns (flowing from the first two points above). These are expressed in the classical Faustian-Frankenstein themes: should science research some areas at all? are there fundamental ethical concerns, natural laws, which should limit scientific research?\textsuperscript{45}

• Questions of Equity. A typical problem of new technologies is the redistribution of social costs and benefits. For instance, high tension powerlines or the construction of new airports may benefit large parts of the population but simultaneously impose costs on specific groups which may experience little or no direct share of these benefits. Also there is the related issue, given the huge social and economic costs of scientific research and development, of how goals and directions should be decided.\textsuperscript{46}
- Freedom of Choice and Individual Rights. These involve the tendency for decisions made about new technology to demand universal compliance. 'Everyone must experience the effects of the decision' - for instance, should individuals have the right to use banned or scientifically dubious medications?

Governments impose regulations on the assumptions that individual choices have social costs or that individuals may fail to make rational choices on their own behalf.47

The 'intrinsic' difficulties surrounding modern science and technology noted above, plus the legitimatory functions of science and technology noted earlier can come together to produce ambivalence and profound cognitive dissonance. Within STC, experts, and the knowledge they 'represent', quickly come to take on ambiguous roles. Nelkin describes the situation succinctly:

... for they [experts] serve as a source of authority in disputes that are partly based on mistrust of precisely the kind of expertise that they themselves have come to represent.48

STC studies working in this framework frequently set about exposing the problems involved in the so called blurring of scientific/technical and social values. This has often involved describing the ways scientific/technical decision-making disenfranchises those without scientific/technical expert representation or those unable to, or unwilling to package their responses to scientific and technical developments in scientific and technical terms. Attention has also been payed to the growth of 'state sanctioned' decision making processes such as public inquiries where scientific, technical and legal 'experts' reframe political decisions into scientific/technical ones.49

In short, a theme in many of these studies is the threat to democracy if the political values embedded in technical and scientific disagreements are not acknowledged. To quote Nelkin again:

... a ubiquitous question woven throughout the variety of disputes over science and technology is who should control crucial policy choices? Reflected in this question is the fear that complex technological advances with far reaching consequences have changed the nature of the decision-making process, that scientific approaches to public policy have encroached on the traditional political models, and that deference to scientific
knowledge threatens democratic principles, reducing public control of crucial policy choices.\textsuperscript{50}

The 'subject matter' of technocratic political' approaches tend to be things like the processes of scientific and technical decision making in controversial, frequently public, debates.\textsuperscript{51} The content of the respective positions in such debates may be analysed to explore the political implications of; the re-descriptions of 'political concepts' as 'scientific concepts'; the role in decision making of experts and expert bodies; the political agendas of expert groups; the capture of expert groups for political purposes; and the form and quality of public participation in such debates. Many of these subjects are similar to those examined in 'controversy as politics' approaches. However, unlike the 'controversy as politics' approaches, here political viewpoints are contextually filtered through the prior problematic of the 'technocratic societies' inevitable blurring of the boundaries of science and society. Hence the idea that such controversies are 'special'; that science and technology filter particular concerns in specific ways that need to be explored as valuable in their own right.

A variety of epistemological positions are possible in technocratic politics approaches:

- There can be a degree of indifference about entering into 'micro epistemological' debates concerning the nature of scientific knowledge. Such indifference can be justified by noting that more important issues surround the understanding of the complexity of the mixing of social/technical concerns and knowledges which are intrinsic to scientific/technical controversies. As touched on previously, such mixtures of science, politics, facts-values are not taken as anomalous. Rather they are seen as a part of the condition of modern scientific technical society, something requiring analysis in its own right and not re-description via the analyst.

- Some studies, notably the work of Wynne,\textsuperscript{52} Albury\textsuperscript{53} and Rip,\textsuperscript{54} do address epistemology squarely. Nevertheless, their emphasis is not on exploring the intricacies of scientific knowledge construction in situ alone, which is often done in more micro epistemologically preoccupied approaches, but rather to explore what should be the appropriate political practices involving scientific/technical disagreement, acknowledging from the outset, as central, the unique position and symbolic significance of modern science as a source of
epistemological authority for contemporary knowledge claims. The exploration of the construction, and implications, of scientific authority becomes as, or more important, than the preoccupation with the construction of specific knowledge claims at a micro level. Overall, in a sense then, the major epistemological preoccupations of 'technocratic politics' approaches could be described as 'macro epistemic', i.e. the broader significance of the way that the authority of science is used in the construction of knowledge claims.

From a 'post-Kuhnian' perspective, 'technocratic politics' approaches to STCs have a number of strengths and weaknesses. Their weaknesses involve the tendency for an uncritical dichotomy of technocratic versus humanistic to be used in analysis. In exploring the dilemmas in scientific controversy of the 'blurring of the boundaries' of the scientific/technical versus social/humanistic, 'technocratic politics' approaches ironically themselves often risk 'blurring the distinction' between the technocratic humanistic dichotomy as, on the one hand, a participant's resource, whose meaning is subject to negotiation according to the specific context of its use, and, on the other hand, an analyst's resource, as an over-arching theory of society and knowledge. This can mean that whilst technocratic politics approaches correctly identify the importance of technocratic rhetoric and the unique legitimatory role of science in contemporary western culture, there can be a risk of assuming an overly simplistic linkage between technocratic rhetoric and actual practices and knowledge claims, rather than carefully documenting such linkages as they unfold in situ. Further, the analyst may risk introducing, a priori, without the benefit of detailed explanation, theories of what constitute technocratic versus humanistic categories of thought.

Overall, the technocratic versus humanistic dichotomy may be useful to consider when analysing scientific controversies as long as it is taken as an important part of the rhetoric of participants in controversies. Whilst the way participants deploy such categories may in fact be one of the central forms of legitimatory rhetoric that can be observed in many scientific controversies, the analyst nevertheless should avoid the tendency of introducing 'unexplicated' theories involving humanistic versus technocratic world views against which to measure/judge participants' claims.
Aside from the difficulties noted above, technocratic politics approaches to controversy do provide a number of useful observations.

Most importantly they attempt to identify some of the features common to STCs. These involve specific observations about repeated patterns of social concern in relation to the introduction of new science and technology, such as fear of risk, problems of equity, etc., and the more general observations of the broader cultural significance of modern science as a belief system - that is, problems of cognitive dissonance when experts disagree, the use of scientific/technical rhetoric to legitimise political claims, etc. The importance of the broader cultural significance of science and its implication for the study of controversy is easily overlooked in some of our other approaches. For instance, in 'controversy as politics', consideration of the image of science as a special form of legitimation tends to be subordinated to considerations of more general politics. Similarly, in sociology of scientific knowledge approaches, the need to explain the heterogeneity and localised nature of the politics behind the construction of science as practice, risks losing sight of the more general homogeneous discourse of science as legitimatory representation.

It is extremely important to incorporate into the analysis of controversy a recognition of the broader cultural significance of science as a belief system. This is particularly true if we consider the growth of numerous knowledge-making contexts and 'hybrid experts' operating between esoteric specialist research sites and the public. These key sites are rich in the use of rhetoric and negotiation of both specific scientific knowledge claims and more general claims about science as a belief system.
Historico/narrative approaches

Told in the form of a riveting medical detective story, *Currents of Death* is a meticulously researched and dramatically written warning about the most pervasive - and covered up public Health Hazard Americans face: the pernicious effect of our continuous exposure to low-level alternating-current electromagnetic fields.


The category 'Historico narrative approaches' encompasses the numerous accounts of scientific disagreement which do not specifically locate themselves within any kind of obvious theoretical framework. This category includes things like journalistic exposés, some government reports and some quasi-academic histories. Obviously studies in this category do have implicit causal models and theoretical viewpoints. These will tend to be drawn from accessible intellectual resources relatively familiar to popular culture. Reflection on these 'intellectual resources' will not be one of the deliberate aims of such studies; rather such resources tend to be used as tools of convenience, a means to a narrative end.

There is usually less scope for investigation of the general nature of science or society in such studies. The specific controversy in question is the central subject. Such studies are frequently justified on the grounds of providing information to the public. Often, if there are lessons to be learned, they are equally specific, though in some journalistic contexts the lack of theoretical consideration of science tends to lead to the treatment of controversies as anomalies resulting from the breach of scientific norms, or forms of fraud. Whilst this genre of account of controversy is readily recognisable, it is difficult, as noted, to systematically analyse all the potential approaches offered. In short, philosophy and sociology of science and politics tend to take a 'back seat' relative to attempts to convey a sense of what happened and its significance.

Because such forms of analysis rely on the implicit ideal of 'telling it like it is', there is normally no acknowledgment of the implicit philosophy of science embodied in the analysis. It is not surprising then that so many of these types of studies do not adequately address the dimensions of scientific controversy which involve participants negotiating the meaning of science,
both in general and in the specific context of argument. Because these dimensions are ignored there is a tendency to proceed by supporting one side of a controversy and exploring the sources of 'bias' and 'error' in the other.

If there is any potential strength in such approaches it lies in their potential to focus on the dynamics of scientific controversy outside of narrow expert contexts, and in not being bound to theoretical interests that, in some of the other approaches, can lead to the elucidation of theoretical models taking precedence over the exploration of the specific historical features of any given controversy.
5 Fact/value approaches to controversy

... intelligent people on both sides of the controversy search enthusiastically for new problems diligently preparing charges and rebuttals, testing the strength of their arguments in open debate ... Unfortunately, technical controversies are usually chaotic and therefore we fail to achieve the potential benefits available to us ... in most controversies adversaries never confront one another.


In 'fact/value' studies the foremost concern is with the idea that technical and scientific controversies are pathological problems in modern society which need to be addressed and remedied. This is to be done by exploring ways of separating out the scientific from the political and social value components of controversies. Taking the work of Allan Mazur as representative of this approach, its key features can be identified as follows.57

Scientific and technical controversies have a number of background causes. Not unlike 'technocratic politics' approaches there is the acknowledgment that some intrinsic features of modern technology may stimulate controversy. For instance: strangeness of new technologies; problems of 'voluntarism' and 'compulsory adoption'; and the challenges of redistributions of costs and benefits.58 Whilst significant these factors, or any coherent anti-science sentiment, are not the most important sources for scientific controversies.59 More important sources for controversy can be located in the two following complementary scenarios:

- where immediate personal threats of negative by-products of new science or technology are perceived by groups or individuals (or less often, but still important, warnings about such threats are made within the ranks of scientific technical communities), and when such threats and warnings are brought to the broader public via the mass media;60

- full blown controversies occurring when the specific concerns noted above are linked to broader social and political issues and the political identifications of participants - to capture this last point, Mazur asks the
reader to 'visualise the nuclear power controversy as a surfer riding successive waves which are larger national issues'.

At a more specific level, there are a number of explanations given for the occurrence of controversies. Mazur explains this by examining the specific ways knowledge claims are politically shaped in the context of controversies. These explanations fall into two related categories:

- the conflation of facts and values; and
- ambiguity surrounding knowledge claims, i.e. the demand for certainty and the concomitant invasion of interests and biases when the area of science in question is, in reality, uncertain.

These two points may be expanded on as follows. There are a number of ways relevant facts and values are problematically mixed in scientific controversies:

- by the rhetorical device of stating factual hypotheses in irrefutable, untestable terms - for instance, the often asked question 'can it be proved something is not a risk?'; and
- by phrasing hypotheses in vague probabilistic terms.

The second theme, the role of ambiguity, is closely related to the difficulties of separating 'facts and values'. Ambiguities actually provide one of the entry points for confusion over 'facts and values' in debate in the first place. Ambiguities flow from situations involving complex problems of the state of the art which involve perceptions, which are difficult to articulate specifically, and questions which require judgement where there are no formal guides to procedure. These situations are typically like the following:

When it is necessary to make a simplifying assumption and many are reasonable, which simplifying assumption should be made? When data are lacking on a question, how far may one reasonably extrapolate from data of other sources? How trustworthy is a set of empirical observations?

Under these conditions of ambiguity, experts readily fall into the trap of using two polemical strategies to satisfy scientific peers, the public and government bodies:
the rejection of discrepant data: this involves the denial of the validity of an opponent's data often for pedantic reasons - rather than on strictly scientific grounds which might find the data reasonably acceptable;

the presentation of alternative interpretations: this occurs when both parties agree on the data but argue it may be interpreted in completely different ways with totally different decision making consequences - for example, the debate over 'linear' versus threshold doses for harmful effects of ionizing radiation.\textsuperscript{65}

Out of this polemic and failure to admit the limits of scientific knowledge - failure in a particular context to 'agree to disagree'\textsuperscript{66} - scientific positions tend to polarise as controversies proceed.\textsuperscript{67} In fact the positions of disputants take on the qualities of ideologies as they become entrenched, modified and related to particular decision making paths.

Added to the phenomena of polarisation and ideology formation, there are a number of other factors contributing to the failure of disputes to be resolved. These include:

- failure even to attempt to mediate a dispute to start with;

- the slowness of this process even if it is attempted;

- the pressure of non-experts (who fail to comprehend the real issues) on experts;

- lack of trust in political and administrative process;

- experts once involved needing to engage in 'face-saving', defending scientific positions which in other contexts would have been relinquished.\textsuperscript{68}

Like 'controversy as politics', fact/value approaches often study the dynamics of social movements and the motives of disputants. They also frequently make specific recommendations of how to 'resolve' controversies. This latter point has come to be a dominant theme in some studies. In fact it is often noted that, in abstract terms, 'controversies' have potentially positive aspects. For instance, Mazur argues that controversies,
carried out properly, can become an effective means of 'technology assessment'. Ideally, this would occur when

intelligent people on both sides of the controversy search enthusiastically for new problems diligently preparing charges and rebuttals, testing the strength of their arguments in open debate.69

Part of the aims of 'fact/value' analyses, then, is to suggest how the appropriate potential of controversy can be realised in the face of the real world situation where controversies all too readily become intransigent and harmful. As Mazur puts it:

Unfortunately, technical controversies are usually chaotic and therefore we fail to achieve the potential benefits available to us ... in most controversies adversaries never confront one another.70

Policy proposals, logically following from the above, involve suggestions of how to separate facts and values, how to specify the limits of scientific knowledge in any particular context, and how to answer any particular question and clarify particular scientific viewpoints.

This broad set of proposals works within the background assumption that there is an important division between scientists-experts and society generally. Ideally, scientists should be left to handle 'scientific/technical' questions and the public (society) 'value' questions. In regard to these issues, one pragmatic proposal Mazur feels should be developed is the use of science courts.71

Science courts were originally proposed in 1976 by a White House Task Force on anticipated advances in science and technology headed by Arthur Kantrowitz. They were originally designed to work through three phases of problem solving:

- identifying significant questions of science and technology associated with the controversial public policy issue in question, leaving ethical/political questions for subsequent consideration;

- establishing an adversary proceeding to be presided over by scientists/judges where scientific experts would testify and scientific advocates would cross examine them; and
the judges issuing their decision on the scientific facts pertaining to the disputed technical question.72

Overall then, the fact/value approach is pre-occupied with identifying and analysing the ways social/political values enter into science during STC and the way such values can be removed, i.e. clarification, testing, the creation of expert referees, the discipline of adversary procedure. Following from the assumption that the ultimate causes for controversy do not emanate from within science but, rather, from the supposed invasion of politics into science, fact/value approaches argue that in the appropriate environment science should be able to apply its own corrective, which is to let experts 'fight it out' amongst themselves. In such contexts the only 'real' role for the social scientist, or members of the public, is to make sure that scientists 'play' by their own stated rules.73

'Fact/value' approaches to understanding controversy would appear, then, to adopt a straight-forward 'realist' epistemology. Whilst this is correct in the main, there are, nevertheless, attempts in this literature to acknowledge, even if only to a subtle degree, the importance of social context in the construction of scientific claims. Mazur, for instance, anticipates the objections of some philosophers that 'fact/value' dichotomies imply a naive realism. The separation of 'fact/values', he argues, can be thought of in contextual pragmatic terms; it does not have to be pedantically pure.

All that is required is a separation of blatant evaluative and normative statements from statements of fact. Values which are shared by all the contending interest groups or values which are too subtle to affect practical decisions, may be intertwined in the statement of fact without causing a problem.74

In evaluating the strengths and weaknesses of 'fact/value' approaches, the questions surrounding the philosophical and practical viability of separating 'facts' from 'values' become crucial. From a post-Kuhnian vantage point, this desired separation of 'facts' and 'values' is philosophically implausible (see later discussion of SSK). Similarly, in 'technocratic politics' approaches, separating facts from values is also seen as a difficult if not impossible task because of the historical considerations of the legitimatory role of science and technology. Further, whilst some realist philosophers of science still argue for the possibility of the separation of 'facts' from 'values', this work normally relies on sophisticated, philosophical re-descriptions of activities
of scientists and not the types of primarily sociological/political analysis prescribed by scholars such as Mazur.

Not all aspects of 'fact-value' approaches can be seen as negative though. Some 'fact-value' approaches permit and encourage the analysis of the rhetoric participants use in their attempts to separate what, for them, are facts and values. Mazur's work on rhetoric and argument provides a number of valuable insights into the strategies used by competing groups in scientific controversies. 'Fact-value' studies have also provided insights into the ways competing social interest groups come to have different concepts of facts and values in the first place.
One of the tasks of this volume is to suggest distinctions among the various ways in which controversies are brought to a conclusion: by negotiation, by political procedure, or by the means supposedly more usual in scientific controversies, namely the appeal to facts and observations.


The fifth framework for understanding controversy could be described as 'closure' studies. This category captures the bulk of the work emanating from the so-called 'Hastings Project', an undertaking which involved more than 30 scholars, and addressed 'the character of scientific disputes with a heavy ethical or political overlay'.

Conferences and research groups in this Programme met between 1978 and 1982. Materials and papers from these meetings were re-worked between 1982 and 1984 and finally published as a series of essays edited by Engelhardt and Caplan, published in the large 1986 volume titled Scientific Controversies. Most of these essays were preoccupied with the context of closure. As Engelhardt and Caplan explain:

One of the tasks of this volume is to suggest distinctions among the various ways in which controversies are brought to a conclusion: by negotiation, by political procedure, or by the means supposedly more usual in scientific controversies, namely the appeal to facts and observations.

It is true that some of the essays from the Hastings project fall within the other categories, namely Mazur's 'fact/value' approach, Nelkin's 'technocratic politics', and Mendelsohn's approach, which comes close to the sociology of scientific knowledge. Nevertheless, most of the essays focus on closure in the same way, broadly sharing the following key assumptions.

According to closure studies, scientific controversies arise for a number of reasons. As a background assumption, it is acknowledged that controversies are typical in the history of science. This, in part, explains their usefulness as a site for addressing the more general question of evaluating conceptual
developments in science.\textsuperscript{80} Further, not unlike Popperian views of scientific discovery, the psychology of the origins of controversy are taken to be equally as varied.\textsuperscript{81} Building on this analogy it is argued that this is one of the reasons the study of closure is important. The act of attempting to falsify or test hypotheses can be seen as an integral part of the process of closure.

In spite of the variety of possible background causes for controversy it is argued that there are four contexts in which controversies which resist closure have most commonly arisen. These contexts entail:

- different appreciations of the evidence at stake;
- the involvement of competing social groups with opposed political visions;
- large amounts of public involvement stimulating disputes to become more complex; and
- disputes having become highly differentiated, involving numerous sub-controversies.\textsuperscript{82}

Overall, as controversies are seen as a normal part of science, identifying their causes is valuable in allowing a better appreciation of

the ways in which the inner logic of scientific investigation, predicated upon a concern with evidence and reasoning on the basis of evidence involves values and political concerns.\textsuperscript{83}

and in providing a site to examine

the extent to which the movement of science to more ample and secure knowledge claims is shaped and directed, or impeded by forces external to science, such as the values and political forces of the scientists' cultural milieu.\textsuperscript{84}

Controversies occur as both a normal part of scientific development in the sense of the psychology of discovery, but are also the by-product of outside pressures.

At a more specific explanatory level, closure studies explore the following propositions:
• They stipulate that their main concern will be scientific debates with heavy political or ethical overlays. This stipulation does not limit study unduly, though, as many scientific controversies fit into this category, especially considering the contemporary importance of scientific legitimacy in public policy making.

• It is assumed that science, ethics and politics are marked by controversy with different patterns of reasoning and modes of resolution. For instance, it is argued that scientific/technical controversies are generally resolved by appeal to facts and reasoned arguments, while in the case of politics, negotiation is seen as central and with ethics there is a mixture of the above.

Flowing from this, the discussion of controversies involves separating out not only the various ingredients, i.e. forms of reasoning and resolution, but also the different sub-controversies involved in any given debate.

Following on this emphasis of individuating the specific controversies within any given debate, there is the need to map the distinctions between different ways such controversies are closed. In their opening essay to the collected Hastings papers, Engelhardt and Caplan attempt to synthesise the different approaches to closure suggested within the Hastings project. These various forms of closure are basically unintelligible unless they are understood in the light of the fundamental dichotomy between 'sound argument closure' and other forms of closure.

'Sound argument closure' primarily refers to closure on the basis of 'rational' argument. A degree of complexity enters into closure studies at this point: for whilst the dichotomy between sound argument and other forms of closure (not dissimilar to 'fact/value' distinctions noted earlier) is maintained, there is a strong aversion to turning controversy studies into a form of logical reconstruction of science on the basis of current scientific standards (whatever such things might be). Controversies should always be placed into their appropriate historical context.

The rules for acquiring evidence and drawing conclusions, however, change in science over time. One must, as a consequence, qualify scientific controversies with a socio-historical subscript to identify a scientific controversy with a particular scientific community, its rules for selecting evidence relevant to a debate, and its rules for reasoning on the basis of such evidence. That is, a scientific controversy becomes identified
with a particular scientific community, understood as a group of
stake holders in a scientific debate who at a particular point in
history share common rules of evidence and inference.\textsuperscript{85}

This concern with historical context should not, however, be confused with
the notion of historical context as it appears, for instance, in contemporary
sociology of scientific knowledge studies. Importantly, it is not taken to
imply the need for epistemological relativism, i.e. the socially contingent
basis of scientific knowledge claims. Historical context with simultaneous
scientific realism is given in closure studies by appealing to the idea of two
forms of sound argument closure: sound argument in the 'strict sense' and
sound argument in the 'broad sense'. The former sense is the ideal of 'what
would occur on the basis of rules of evidence and inference unconditioned
by the history or culture of the participants'.\textsuperscript{86} Engelhardt and Caplan hint
that such arguments would be those which could be imagined to come from
'the ultimate scientific community', something not necessarily achievable
but which could be appealed to as an 'ideal type' from which to judge
deviations against its norms and reasoning.

One appeals to that community as an intellectual possibility in
criticising actual communities and their rules of evidence and
scientific reference. The scientific community best able to make
true knowledge claims is thus an intellectual standpoint from
which one both justifies and criticises the deliverances of any
particular community.\textsuperscript{87}

The second sense of sound argument closure, in the 'broad sense', moves
from the 'ideal' to the 'pragmatic'. These arguments are ones that involve
appeals to rules of evidence and inference, that are 'as far as can be
determined by the participants, correct and undistorted by the
presuppositions of the participants.\textsuperscript{88}

Historically contextual arguments in relation to sound arguments in 'the
broad sense' can be made according to deviations observed in the behaviour
of participants measured against the participants own stated or constructible
rules. At the same time at something of a 'meta-level' the standards of the
participants themselves, in the first place, can be judged against ideal sound
arguments in the 'strict sense'. From the analyst's viewpoint, 'strict sound
argument closure' is seen as being prescriptive whereas 'broad sound
argument closure' is seen as descriptive.\textsuperscript{89}
'Bouncing off' these notions of sound argument closure, four other forms of closure are identified. These are all forms of closure other than those which appeal to reasoning on the basis of 'sound arguments'. These forms of closure can be briefly described as follows. They include:

- abandonment - when a controversy closes through lack of interest;

- force - where closure is achieved via coercion and there is no appeal to reasoning or negotiation, one side merely prevailing by political force;

- consensus (puzzlingly this is not to be confused with negotiation) - this is meant to capture shifts in belief systems with origins outside of the controversy which may lead to a controversy closing;

- negotiation closure - this is a procedure whose outcome is agreed to close an issue, aside from whether sound argument closure can be achieved - in a sense, this refers to consensus about the means to solve a controversy rather than over the conclusion of the means.\(^90\)

This rather convoluted schema of forms of closure is further complicated in that, in practice, in any given controversy, in keeping with the earlier observations that controversies comprise of sub-controversies, mixtures of forms of closure are possible.\(^91\)

The conceptual tools of closure studies are historical and sociological analysis refracted through the aims of identifying the different forms of controversy within a given debate, identifying the appropriate forms of closure and speculating on the reasons for the absence of closure. And as far as the latter is concerned, there will be overarching considerations of comparing sound argument closure to other forms of closure. The latter dichotomy is important in that the analyst must deploy judgements about what constitutes valid scientific reasoning. These judgements take two forms: in the case of sound argument closure in the broad sense, this is an historically informed judgement according to a reconstruction of the scientific standards of the participants; in the case of strict sound argument closure the judgement is of what makes for valid science in ideal, philosophical terms. These judgements lead the discussion of closure into a multi-layered process of a limited 'sociology of knowledge' and 'sociology of error'. A limited sociology of knowledge is possible in that the historical sources for knowledge claims, including valid ones, are acknowledged. It
should also be noted this sociology of knowledge is reminiscent of the limited sociology of knowledge of Merton - there being a pre-occupation with documenting the stimulus of social influence on the direction but, ultimately, not the content of science. A sociology of error is possible in that error and bias are investigated, both in the historical context of 'relative error bias' of 'broad sound argument closure'; that is, searching for inconsistencies within a reconstructed image of what participants should have argued within their own historically framed scientific beliefs; and, in the ahistorical context, of 'universal error' against images of 'strict sound argument closure', searching for inconsistencies in participants' beliefs relative to absolute images of scientific correct practice.

Closure studies are 'advertised' as contributing to academic discourse and public policy in the following ways:

First, at the broad level of theoretical discourse, closure studies are claimed to transcend preoccupations with 'internalist' and 'externalist' approaches to the study of science. The study of controversy via processes of closure is seen to provide a superior vehicle for understanding the processes of scientific change. The actual form of closure in any particular context is open to interpretation. Neither internalist nor externalist factors are of assumed a priori importance. Whilst sound argument closure should be the ultimate scientific arbitrator and is thus analogous to internalism, unlike internalism it is an ideal model of what scientists should have done, not one which assumes that this is what actually has occurred or will occur in any given context.

Second, it is claimed that closure studies 'expose' and highlight the multi-dimensional nature of scientific controversies: that scientific controversies are as complex as the human condition itself.

... scientific controversies are as complex as the weave of human interest and truth, fairness, and individual advantage. The understanding of controversies, and of their resolutions, requires what the humanities always require: a careful attention to the human condition.92

In short, the complexity of controversies encourages a need for methodological pluralism. Insights from philosophy, sociology and politics should all be used. 'A geography of scientific controversies and of the numerous pitfalls of closure must be multi-dimensional.'93
Third, it is argued that closure studies of scientific controversy offer valuable insights into society. This possibility is enhanced by the insistence of closure studies on the plurality and complexity of controversies.94

Finally, at a policy level, closure studies are claimed to provide an important instrument in clarifying the factual and ethical political dimensions of controversies, hopefully enhancing 'fairer' and 'more truthful' forms of closure. Through this appreciation of the interplay of factual and evaluative dimensions, Engelhardt and Caplan propose:

... we can better acknowledge what elements of such debates can be resolved through the discovery of new facts and what elements can only be resolved through developing means to create arbitrary standards that will fairly balance the interests of the stake-holders. There is an advantage in knowing when we can know more truly versus when we can hope to negotiate solutions more fairly.95

The objects of scrutiny in closure studies, in theory, are only limited by the historical circumstances of the given controversy under review. In practice, this pluralism does tend to be circumscribed. The importance of concepts such as sound argument closure refract the evaluation of the 'contents of science' in controversy through the lens of what the analyst regards as ideal science. As will be discussed shortly, more overtly 'symmetrical' types of analysis (those which assess all scientific knowledge claims as epistemologically equivalent) which do not presuppose strict sound argument closure allow a closer focus on the dynamics of the negotiations of the content of scientific knowledge claims.

Not unlike fact-value approaches, the weaknesses of 'closure studies' also revolve around their philosophy of science. The most important of these weaknesses involves the analyst's aim of specifying what the ideal scientific method standards should be, so as to identify what form of closure was or was not possible to achieve in a given debate. This difficulty becomes increasingly more acute when related back to things like 'strict sound argument closure' and 'sound argument closure'. The tension in such studies between attempting to locate controversies in their historical context whilst also trying to locate scientific method in an ahistorical sense invites contradiction. For example, it is easy to imagine the specific historical context of a controversy being lost and distorted in the process of the analyst deploying their own contemporary version of scientific method. Finally, if forms of closure are to be measured against the analyst's pre-existing model of scientific method, the methods identified as being important to
participants in a given controversy are unlikely to contribute to modifying
the analyst's theories of science. Closure studies, then, encounter difficulties
fulfilling one of their stated aims which is to contribute to building a
detailed image of the nature of science. At best, controversy studies can
provide confirmation of the analyst's pre-existing theories of science.

One potential strength of closure style studies, nevertheless, is that the idea
of exploring broad sound argument closure (involving the use of historical,
contextually sound arguments) can be a useful heuristic device to explore
differences between the stated values of participants and their actual
behaviour. Certainly from the perspectives of SSK (to be discussed shortly)
'sound argument closure' is a questionable device in that it involves the
analyst actively reconstituting science. Nevertheless, it would be fair to say
that in a less philosophically strict way, the study of STC frequently
involves some reconstruction by the analyst, no matter what her/his
chosen epistemological perspective, of what participants in a controversy
take as scientific.

A second positive feature of closure studies is in their acknowledgment of
the inter-play in any single debate of numerous sub-debates or sub-
controversies. It is obvious that any rich theoretical understanding of
controversy should involve identifying their multi-dimensionality. From a
pragmatic point of view, closure studies, in emphasising the plurality of
elements making up a controversy, help stand between the excessive
attention given to external political factors, as in controversy as politics
approaches, or internal forces, as in much contemporary sociology of
scientific knowledge analysis, with its concerns with the micro-construction
of scientific knowledge.
... 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.


The final category of scientific controversy studies we will consider is that of the contemporary sociology of scientific knowledge (SSK). One of the first major defining features of these studies is that they take as their 'point of departure' the related concepts of the 'under-determination' of scientific theories by the evidence and the 'theory-loading' of observation.

First, the under-determination thesis, or the Duhem-Quine thesis, can be considered. In brief, it proposes:

... any theory can be maintained in face of any evidence, provided that we make sufficiently radical adjustments elsewhere in our beliefs ... that no one single theory or theoretical hypothesis can ever be extricated from the ever present web of collateral assumptions so as to be open to conclusive refutation.96

A problem of fit between observation and theory can always be evaded by either incorporating adjustments to the theory or, conversely, observation in principle can always be explained by producing another theory. Basically, logically, evidence is always inconclusive and unable to be 'cleanly tested' against reality.

Complementing the under-determination thesis is the concept of the theory-loading of observation. This concept builds on Gestalt psychology and history of science from Koyre, Kuhn and Feyerabend and Hanson. It also draws on the ideas of language and life-worlds from the philosopher Wittgenstein.97 Theory-loading implies that observations of nature are prefigured by theories of measurement, psychologies of perception and
linguistic classifications. It is always open then for a scientist to challenge the validity of an observation on the basis that one of these auxiliary assumptions is mistaken. Further, a choice between theories can never be made simply on the basis of observation. This follows if we consider that observations have been made possible on the basis of auxiliary factors underlying theories from the outset.98

The acknowledgment in contemporary sociology of scientific knowledge studies of theory loading and under-determination has, as will be discussed shortly, not led to any simple or coherent approach to analysing science. It has, however, helped provide a resource for a relatively coherent set of methodological prohibitions, ways of criticising traditional approaches to understanding science or, to put it simply, proposals of how not to study science.99 These sorts of prohibitions could be listed as follows:

- The analyst should not take for granted the epistemological claims made by scientists. Whilst it is expected that scientists will describe their work in realist terms, the analyst should look 'behind' and 'beyond' such prescriptions. As a generalisation, scientists will tend to display a belief in 'natural realism' where the analyst will display a belief in 'social realism'.100

- Scientific progress should not be understood in terms of the progressive additions of 'bricks' to the growing wall of true knowledge of the natural word. If progress is to be understood at all it should be in terms of it being 'a participant's resource', or, as one might discuss music or art or other cultural traditions, knowledge and practice may become more sophisticated within certain parameters but such measures are incommensurable between different traditions.101

- Scientific discovery should not be viewed in the traditional way as the unveiling of a pre-existing state of nature, not in the common place sense of Christopher Columbus discovering the 'New World'. Rather, scientific discovery is seen as epistemologically problematic. Discoveries become acceptable or are rejected, shaped and defined via processes of social negotiation over time.102

- The history and social studies of science should not be the analysis of social factors accounting for scientific success or failure, or of 'demarcation criteria' between science and pseudo-science. Scientific
success or failure are participants' resources, not those to be used by the analyst. This leads to the so-called position of epistemological symmetry.\textsuperscript{103}

- The idea of a singular efficacious transferable scientific method is rejected out of hand. Rather there is talk of contingent skills and methods, heuristics and accounting processes, etc.\textsuperscript{104}

- Debates about the traditional significance of internal versus external factors and other dichotomies such as data and theory, objective and subjective, natural and social, good science and fraudulent science, have been transcended by the acknowledgment that such \textit{a priori} conceptual categories are too gross to capture the specific historically negotiated character of scientific knowledge.\textsuperscript{105}

- There is a rejection of the Mertonian ideal of science possessing a unique set of corrective social norms making science a unique form of social practice producing objective knowledge.\textsuperscript{106}

- Finally, a theme residing in the background to many of these propositions, there is the philosophical position that nature should not be taken as a causal factor in explaining the content of scientific knowledge.\textsuperscript{107}

In most areas of contemporary sociology of scientific knowledge underdetermination and theory-loading have not been taken as implying the need for psychological studies of knowledge creation or non-sociological, philosophical considerations of epistemological theory \textit{per se}.\textsuperscript{108} Predominantly there has been an emphasis on what could be described as sociological causal explanations. Such forms of explanation are extremely broad, drawing from the three sociological traditions of phenomenology, ethno-methodology and discourse analysis, and from other forms of interpretative sociology.

Pinch describes the broad sociological approach required in the following terms:

... the task for the sociologist is to try and recapture some of the life world of the scientist - the taken-for-granted practices and interpretations which make available the natural world. Of course, for the scientist nature appears as an independent objectively existing realm. On the other hand, for the sociologist,
nature can only be made available through discursive processes.\textsuperscript{109}

From this position, emphasising under-determination and the ubiquitous flexibility of interpretation, controversy as a subject is not something which needs to be thought of as in any way epistemologically special. Controversies rather, exemplify the contingent nature of all beliefs and the ever present possibility for the stability of beliefs about nature (knowledge) to be jeopardised by under-determination. In such models, it is just as important to seek sociological causes for accepted stable knowledge. As such, knowledge in controversy is in fact worthy of study, not because of special epistemological forces being at play but rather, because typical ones are being brought forward. The difference is one of degree not of kind.

Consideration of explanatory devices and models used in contemporary sociology of scientific knowledge studies is closely linked to the points made above. In theory, contemporary sociology of scientific knowledge approaches to controversy would include all the explanatory tools used to account for the construction of science whether or not in controversy. In practice, though, controversies have been favoured by some more than others as a useful site of investigation and illustration. Leaving this latter point aside momentarily, four main approaches and accompanying explanatory devices can be identified in contemporary sociology of scientific knowledge approaches to the construction of science which all have relevance to controversy studies. These approaches are as follows:

- the 'Strong Programme',\textsuperscript{110} or Edinburgh School, and the closely related Empirical Programme of Relativism (EPOR) from Bath;\textsuperscript{111}

- textual analysis, discourse analysis and experiments with new literary forms;\textsuperscript{112}

- actor-network and 'social world' studies;\textsuperscript{113} and

- laboratory studies and ethnographic approaches.\textsuperscript{114}

Of all the approaches, the first, Strong Programme and EPOR, is the one that most directly addresses the issue of STC as a key research site. The other approaches in question provide numerous tools which are relevant to understanding controversy. This is mainly in the context of the insights they
provide into the nature of scientific knowledge generally. There is also a strong degree of cross-fertilisation among various branches of contemporary sociology of scientific knowledge approaches. Strong Programme and EPOR approaches have frequently borrowed insights from other branches of contemporary sociology of scientific knowledge. If there is one broad dichotomy that should be noted in distinguishing Strong Programme and EPOR approaches from others it is this: whilst Strong Programme and EPOR criticise traditional forms of analysis of science and accept as a point of departure under-determination and theory-loading, they can nevertheless be seen as attempting to extend and build upon 'traditional' forms of sociological historical analysis. In some of the other contemporary sociology of scientific knowledge approaches the implications of theory loading and under determination are taken to imply the need for a break from traditional forms of sociological and historical theorising, choice of research sites and even in the ways the analyst writes about science and society.\textsuperscript{115}

The bulk of this discussion of contemporary sociology of scientific knowledge and scientific controversy then will focus primarily on the Strong Programme and EPOR approaches. Where it is useful, it will clarify and distinguish these from other contemporary sociology of scientific knowledge approaches, although it is beyond the scope of this piece of work to engage in any kind of detailed way in some of the more intricate debates within contemporary sociology of scientific knowledge.

The Strong Programme and the Empirical Programme of Relativism are closely related. Possibly the only main differences between them are ones of emphasis. The Strong Programme has tended to pursue more historically based studies, whereas EPOR has focussed on contemporary case studies. Further differences in emphasis flow from the greater degree of methodological internalism displayed generally in EPOR studies.\textsuperscript{116} As a general rule, whilst both areas have been preoccupied with documenting the sociological construction of the content of science, there has been a greater emphasis within Strong Programme studies on linking the content of science to macro-sociological structures.\textsuperscript{117} Within EPOR approaches there has been a focus on exposing the interpretative flexibility involved in the construction of scientific facts, the specific ways in which interpretative flexibility is displayed in specific contexts, and between contexts, and in explaining the ways interpretative flexibility is or has been curtailed or socially closed.\textsuperscript{118} Despite methodological concern with the way
interpretative flexibility and its closure is linked to broader social structures, EPOR approaches have paid it less attention in practice.\textsuperscript{119}

In Strong Programme and EPOR, scientific concepts are treated as analogous to any other form of cultural product. Charles E Rosenberg, in an essay review of the Strong Programme case studies in \textit{Natural Order}, described this approach succinctly:

Scientific concepts, like man's [sic] other cultural products, are fully understandable only in their 'context of use'. This context can reflect religious or regional identity, the needs of a particular class grouping, or a generational difference in perception. It can reflect as well the diversity of training, institutional affiliation, and aspiration within modern scientific disciplines. Scientific ideas are thus metaphor and tool, not simply ever closer simulations of a natural reality.\textsuperscript{120}

The forms of sociological explanation used in these approaches have been heavily influenced by three (not necessarily always acknowledged) analytical concepts:

- naturalism;
- social interests; and
- symmetry and reflexivity.

We can begin by looking at naturalism. Naturalism incorporates five interlocking themes.

- Whilst due caution should be exercised in making the analogy, the broad goals of explanation should not be dissimilar to the general aims and methods of science broadly understood.\textsuperscript{121}

- Accounts should be non-evaluative, description taking precedence over prescription. This is echoed in appeals to distinctions between epistemic versus judgemental relativism and the tendency to focus on, as Mulkay and Knorr-Cetina put it, the 'how' rather than the 'why' questions surrounding science.\textsuperscript{122} This distinction is also designed to separate the contemporary sociology of scientific knowledge from some traditional philosophies of science, which aim to judge correct or false approaches to generating scientific knowledge.
• Epistemological neutrality is sought in modes of explanation.¹²³

• Accounts should be non-teleological and non-anthropomorphic,¹²⁴ and there is a preoccupation with more concrete lower level theoretical concerns, rather than difficult-to-test grand theories.¹²⁵

• Accounts tend towards eclecticism and 'thick description'. There should be an attempt to avoid doing violence to the data.¹²⁶

This quasi-scientific ideal of naturalism is further fleshed out by adding the concept of social interests. Social interests ground concerns with explanation to sociological categories, as opposed to psychological or philosophical ones. The concept of social interests can be explained by making the following points:¹²⁷

• Social interests can be thought of by way of analogy with concepts such as social class or gravitational fields where causes are able to be inferred from effects.¹²⁸

• The identification and analysis of interests is dependent upon the analyst’s interpretation not that of those possessing the imputed interests.¹²⁹

• The epistemological status of the analyst’s use of social interests should be evaluated in instrumental terms: there is not necessarily any one account of interests involved in knowledge construction. Rather, an account of interests should be judged in terms of its usefulness and plausibility given certain explanatory aims.¹³⁰

• The ubiquitous interpretative flexibility of knowledge claims means social interests will be equally ubiquitous. Closure in the sense of certifying or stabilising knowledge claims is always an achievement tied to specific spaces and times. The attempt to achieve such forms of closure will reflect past social interests whilst certified knowledge claims in turn become tools to pursue further interests.¹³¹

• Social interests tend to be identified at key sites of the construction and certification of knowledge claims. This means that there has been a tendency in Strong Programme and EPOR to look at social interests
within sub-cultures, scientific communities or 'core-sets' of expert disputants. Broader social interests tend to be things that are expressed through the choices made at this more micro-level. The linkage between the expression of social interests in micro-sub-cultural settings and broader social contexts is taken as contingent on specific circumstances and open to specific investigation.\textsuperscript{132}

The third set of analytical concepts are symmetry and reflexivity. Symmetry stems from the awareness that the supposed 'truth' or 'falsity' of knowledge claims should not be used as an analyst's resource in understanding the construction of science. Rather, these are participants' resources.

Symmetry is a particularly relevant device for the study of scientific controversy.\textsuperscript{133} All relevant scientific positions should be able to be analysed with the same tools, and causal factors should be epistemologically equivalent. This latter equivalence, however, should not be confused with an argument that all knowledge claims are of equal value in practical contexts\textsuperscript{134} or in moral terms.\textsuperscript{135} Nor does it imply that knowledge claims should be interpreted outside of their historical context. For instance, an awareness of the status which participants give their claims is relevant in terms of identifying 'core-sets' and characterising the types of social interests which will be relevant.\textsuperscript{136} Reflexivity fits in closely with the concerns of symmetry. It requires that the analyst be aware of the epistemologically conditional status of his/her own knowledge claims. Hypothetically these claims should also be able to be analysed in sociologically causal terms, i.e. interests, etc. In short, symmetry and reflexivity follow from the aims of methodological epistemological neutrality. How possible such neutrality is and what forms symmetry and reflexivity should take have recently been open to considerable debate. This will be discussed shortly.

To recapitulate, in theory, within Strong Programme and EPOR approaches, the explanations for why controversies arise are exceptionally broad, being only implicitly limited by what are taken as valid forms of sociological explanation; this itself is informed by the concepts noted above of naturalism, social interests, symmetry and reflexivity. In practice, though, in actual Strong Programme and EPOR studies, examination of what could be described as causes of controversy has focussed rather narrowly on things such as difficulties surrounding attempts to generate new scientific research, or theoretical novelty within scientific sub-cultures, competition for scientific legitimacy and/or financial resources between competing scientific
sub-cultures, and different forms of educational institutional backgrounds shaping the scientific theories and observations of competing scientific sub-cultures. This has meant that whilst there have not been formal prohibitions to linking the causes of controversy to broader social pressures, 'methodological internalism' has held the greatest sway.

One might say that within a closure school vocabulary there has been a predisposition in contemporary sociology of scientific knowledge studies to examine forms of broad 'sound argument closure', to evaluate controversy at the level of the micro-construction of knowledge. This in part reflects the predisposition of EPOR and Strong Programme studies to be mainly concerned in practice with revealing and articulating the ideas of under-determination and interpretative flexibility in science, ahead of analysing scientific controversy as a general subject in its own right. As Pinch puts it, 'the analyst's interest in controversy extends only as far as it provides a location where claims about the natural world are disputed.'

To strengthen a point made earlier, this should not be taken to imply that controversy is unimportant. It is quite consistent within these approaches to regard controversy as one of the key sites for analysing the construction of science, yet to subordinate interest in controversy, as a general subject, to questions about the nature of science as a general subject. Of the numerous possible research sites for the contemporary sociology of scientific knowledge, controversy rates highly within EPOR and Strong Programme approaches. Pinch identifies the reasons for this. 'During the course of controversies we learn more about the critical processes of science which are not visible of other times.'

Pinch draws on an analogy from physics - on punching a system into a critical state so as to observe its key processes. Collins offers a further metaphor of controversies helping to show how ships - scientific facts - are placed in bottles:

... it is only be 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.

44
A further conceptual tool deployed in examining controversy in the EPOR approach, specifically, is the concept of 'core-sets'. Core-sets help to determine which actors will be studied in a controversy.

Core-sets, a phrase coined by Collins,\textsuperscript{141} can be explained by the following seven points:

- A core-set is constituted by the group of scientists immediately engaged in the scientific controversy over a particular fact or theory. There will be a key group of knowledge producers normally engaged at the frontiers of research.

- Core-sets may be constituted by scientists from varying disciplines and areas of expertise both theoreticians and experimentalists.

- Typically, core-sets in modern science are constituted of between ten to thirty scientists.

- These scientists should be easy to identify, as in controversy scientists will tend to conduct their battles in publicly accessible sites such as scientific literature and conferences.

- Normally such scientists should be able to be interviewed and access gained to their ideas.

- The core-set will be surrounded by scientific on-lookers who will follow debate and observe the formal pronouncements of the core-set. The potentially dual role of scientists as both members of core-sets or as onlookers has been described in terms of esoteric versus exoteric circles. It is likely that scientists will be involved in only one esoteric circle though it is possible for them to be part of a number of exoteric circles. It is also important to note that scientists may only, for a very short part of their careers, be involved in a core-set and in fact many scientists may never be part of a core-set.

- Finally, core-sets can be seen as a superior way of identifying a site for analysing science ahead of concepts such as schools, disciplines, invisible colleges; co-citation networks, paradigms, etc.
These concepts are seen to be flawed; because, as Pinch puts it, they rely on an 'artificial separation of scientific social relationships and cognitive relationships, they are unsuitable for capturing the new view of scientific knowledge - a view which sees the cognitive and the social as being irretrievably linked.'

In a broad sense then, the main conceptual tools of Strong Programme and EPOR controversy studies have been: 'naturalism', 'social interest' explanation, 'symmetry' and 'reflexivity' and, in EPOR specifically, these have been complemented by the key research site, which is the core-set.

In deploying the conceptual tools noted above, Strong Programme and EPOR studies of controversy have regularly emphasised the important roles in the construction of scientific knowledge claims played by such things as scientists' textual and rhetorical strategies, the embodiment and crystallisation of knowledge claims into instruments and systems of measurement, and institutional and educational factors. These points can be briefly expanded on as follows.

The study of scientists' textual/rhetorical strategies has fitted in with the more general preoccupations of much contemporary sociology of scientific knowledge with the role of text and language. These preoccupations have taken numerous forms. Whilst some studies have promoted the extreme position that scientists' discourse alone should be the subject of study, Strong Programme and EPOR studies have attempted to link text and language to social interests, the constraints on scientists being members of scientific subcultures and broader cultural knowledge making traditions, and the use of instruments. A number of patterns of rhetoric have been observed operating in scientific controversies. Most important has been the various ways scientists have used flexible evaluative repertoires, that is, the use of flexible vocabularies for describing their own work relative to their opponents' according to different social contexts and various social interests. For example, in evaluation of the rhetoric used by scientists in a controversy in biochemistry, Michael Mulkay observed a consistent pattern of a dual conception of what constituted a scientific 'fact'.

This strategy, adopted by both authors, seems to be related to the dual conception of scientific fact which has appeared in every letter so far. The interpretative conception of 'fact' is used in criticising one's opponent. The interpretative basis of the latter's views is made visible and emphasised as the author formulates the inconsistencies, uncertainties and mistakes perpetrated by his
opponent. It is always possible for the author to find such errors because the opponent's claims are inevitably assessed in relation to the authors' differing conception of the facts and their scientific meaning.

In contrast, when formulating his own views, each author minimises the interpretative work apparently involved. As a result, each author's position comes to appear in the text of each separate letter as indistinguishable from the observable realities of the bio-chemical world.\textsuperscript{146}

Similar flexibility has been observed in the way scientists evaluate ideals such as 'scientific norms'\textsuperscript{147} and 'scientific method' in practical historical contexts.\textsuperscript{148}

In basic terms, a tendency has been observed for scientists, in the setting of controversy, to deploy rhetoric to suggest how their scientific findings are isomorphic to nature, constituted by the application of 'appropriate' scientific practices (the so-called 'constitutive forum'), whereas rival scientific work can be explained as the by-product of social contingencies (the so-called 'contingent forum').\textsuperscript{149}

Dove-tailing with this concern with rhetoric and discourse have been observations of the important role played in controversies of the embodiment and solidification of knowledge claims into systems of measurement and technical instruments. Once scientific claims can be linked to such instruments and 'metrics' they can more easily be reproduced and provide opponents with less flexibility in proposing alternative scientific views. The role of the embodiment of claims into instruments is particularly significant if we acknowledge the importance, to contemporary science, of the rhetoric of testability, experimental replication and demonstration, and linkages to technological systems.\textsuperscript{150}

As a background to the above concerns, with forms of rhetoric and instruments, etc., it has also been frequently noted that things such as scientists' training and educational backgrounds play an important part in controversies. Following on from Kuhn's observations about the dogmatic nature of science education, theory loading of observation and ensuing paradigm incommensurability,\textsuperscript{151} there have been studies which have shown how institutional affiliation and training has shaped ways of evaluating and producing criteria of relevance for considering scientific
evidence. In some controversies, certain scientific questions will be conceptualised quite differently and protagonists arguments fail to meet.\textsuperscript{152}

Considering these various levels of explanation as a whole, the aims of Strong Programme and EPOR controversy studies could be described as identifying the specific contingencies involved in the construction of specific scientific knowledge claims whilst exemplifying or verifying more generally certain epistemological theories, i.e. 'theory-loading' and 'under-determination'. Accordingly, unlike some of the other branches of controversy study identified in this paper, little attention has been paid to making generalisations about the nature of controversy. This concern with the specific historically contingent nature of scientific knowledge has also discouraged the development, or utilisation, of broader political theories of science. This means the political focus will tend to be that located around the negotiation of specific knowledge claims. Whilst the study of the way scientific knowledge claims are utilised in broader contexts and reshaped, constructed and de-constructed, is not incompatible with contemporary sociology of scientific knowledge approaches (as previously noted) it has not been the dominant analytical interest. The practical difficulty of doing broader political analysis without broader theories of science and society such as those held implicitly or explicitly by approaches to controversy such as 'technocratic politics' or 'controversy as politics' has, no doubt, discouraged such projects being undertaken.\textsuperscript{153}

Within the contemporary sociology of science there have been a number of areas of debate and criticism of the adequacy of Strong Programme and EPOR approaches to evaluating scientific controversy. The following four questions capture the main parameters of this debate:

- Do Strong Programme and EPOR approaches use controversy as an exemplar to inform theories of science at the expense of proposing adequate theories of scientific controversy \textit{per se}?

- Do Strong Programme and EPOR approaches make unrealistic demands on the analyst in relation to the kinds of evidence required for the analysis of the detailed construction of the content of scientific knowledge?

- Do the requirements of reflexivity and symmetry require the analyst to (a) experiment writing with new literary forms; (b) engage in overt
criticism of modern science and the dominant political configurations of contemporary western society; and/or (c) become an overt participant in the controversy under analysis, or suffer from methodological inconsistencies?

- Do Strong Programme and EPOR approaches focus excessively on the construction of scientific knowledge in typical expert domains at the expense of investigating the broader sites in society where scientific knowledge might also be constructed?

The detailed dimensions of these questions can be expanded on in order.

In relation to the first question challenges have been made to the Strong Programme and EPOR claims that the processes involved in the construction of scientific knowledge in controversies are the same processes observed in the construction of science more generally - that in fact controversies merely amplify the normal processes which tend to otherwise be hidden in 'science as usual'. A difficulty here though is the fact that much of modern science proceeds without controversy. There is a possibility then that such studies may not be observing a representative sample in the normal processes involved in the construction of modern science.154

Expanding on the second question, Strong Programme and EPOR approaches to controversy normally demand a detailed investigation of the specific decisions, interests and intricate social historical context in which scientific knowledge claims are generated and certified. It can be asked, though, how realistic are these demands in practice? For instance, is it always possible to gain access to relevant scientists and their work at the level of intimacy and detail required? In historical case studies, these problems may become manifest in difficulties in obtaining accurate records of laboratories or being able to interview participants. In contemporary case studies, similar difficulties, but for different reasons, can be encountered. In politically sensitive controversies, participants may be unwilling to be interviewed or provide documents or they may even attempt to enrol or use the analyst for their own purposes. In the case of the latter, the mere presence of the analyst may even shape the knowledge claims and behaviour of participants. It is quite possible then that such evidentiary problems influence the kinds of case studies chosen by most Strong Programme and EPOR analysts. Politically sensitive science, with
uncooperative participants and complex patterns of evidence, may become effectively immune from scrutiny within these approaches.

This concern with the role of the analyst leads us to consider the details of our third question, i.e. the problems of managing the requirements of symmetrical analysis and reflexivity. There have been three main challenges to Strong Programme and EPOR management of symmetry and reflexivity:

- First there is the argument that reflexivity and symmetry demand the analyst explore new ways of writing and conducting research. From this perspective it has been argued that traditional forms of representation embody the kinds of views, i.e. the dichotomy between observer-observed, theory-data, author-subject, etc., prone to criticism if the implications of theory-loading and under-determination are accepted. To some critics it is 'ironic' and inconsistent that analysts deny their subjects' appeals to epistemological justifications for their knowledge claims, but then proceed with forms of argument reliant on de facto epistemological justifications themselves (e.g. empirical observations.) Mere disclaimers acknowledging the conditional status of the analyst's own knowledge claims are not deemed sufficient.

- Second, there are arguments that reflexivity and symmetry demand the analyst to self-consciously engage in political critique of contemporary science and society, something which is rarely done in the existing Strong Programme and EPOR literature. These arguments emphasise the importance to the institutions of modern science of their claims for epistemological authority, that is, their authority as the unique and definitive source of methods for gaining, and being the repository of, objective facts about nature. These epistemological claims provide a significant part of appeals by scientists for funding particularly in areas which are seen as pure research, without obvious and immediate 'spin-offs', and the drive to maintain autonomy over directions and methods of research. A further dimension of these arguments is the proposition that the epistemological authority of science also plays an important role in legitimating numerous broader political claims. This occurs both in relation to political claims in specific areas of public policy, such as health, education and environment, where the state needs to appear to be making decisions overtly in accord with contemporary scientific viewpoints and, at deeper levels, of attempting
to legitimate political process itself, i.e. that the character of particular political systems are in keeping with the methods and practices of science.\textsuperscript{158} The final dimension of these concerns is when the two issues noted above are considered operating in tandem. This involves the proposition that, as a 'by product' of science attempting to maintain its social status and funding, there has been a pervasive tendency for a reciprocal relationship to develop between the needs and interests of social elites and dominant social classes and the directions of scientific research, and representations of scientific methods and practices. Further, this reciprocal relationship has often been obscured by the epistemological authority of science.\textsuperscript{159}

According to these views then, symmetrical analysis and reflexivity and the necessary rejection of the epistemological authority of science, invariably involves the analyst in challenging one of the assumptions fundamental to the nature and maintenance of much of contemporary science and also the legitimacy of numerous areas of contemporary political life. As such the analyst's claim to political neutrality is inadequate, as SSK is, by its very nature, a powerful critique of science and the contemporary political status quo.\textsuperscript{160}

- The third and most radical approach to the problems of reflexivity and symmetry moves beyond the macro-political concerns noted above and argues for the active involvement of the analyst in the specific micro-settings where scientific knowledge is being produced. This argument, directed primarily at scientific controversy, works from the premise that as the analyst's work will be appropriated by the formal participants, no matter what the analyst desires, the analyst 'might as well' get involved deliberately, from the outset, in the construction of scientific knowledge claims.\textsuperscript{161}

The final question involving the adequacy of Strong Programme and EPOR approaches to controversy revolves around whether there has been a tendency for excessive attention to have been paid to the construction of scientific knowledge in typical expert domains such as laboratories, scientific journals and correspondences between scientists, at the expense of investigating the broader sites where scientific knowledge claims may also be constructed. This final question is worthy of some expanded discussion.
The weakness in excessive focus on the construction of scientific knowledge in the 'expert community' has been highlighted in a number of recent attempts to take insights from the sociology of scientific knowledge into the study of broader research sites such as the legal/regulatory environment and the mass media and scientific popularisation. Whilst no unified approach has yet emerged, a number of important theoretical propositions have been made. Some important examples of these include calls for the adoption of 'theoretical agnosticism' in defining the boundaries of science/society, the exploration of the processes of the transformation and stabilisation of scientific facts between and beyond expert contexts and the need to replace traditional views of scientific popularisation with concepts which acknowledge the epistemological importance of scientific communication outside of narrow expert contexts.

These three propositions can be expanded on briefly as follows. The ideal of agnosticism in relation to defining the boundaries of science and society is raised by Cozzens and Gieryn in an introductory essay to a recent collection of sociology of science papers, *Theories of Science and Society*.162 Whilst the authors acknowledge that defining the boundaries of science is still a philosophical issue of first-order importance, they argue that agnosticism mixed with a 'comfortable post-relativist epistemological attitude' is helping to generate fruitful empirical work and is an important ingredient in the shift of SSK out of its 'teenage' phase of methodological internalism.163 The extent to which sites of investigation of the construction of science can be removed from customary expert ones is exemplified in the collection in question in the study by Gieryn and Fiegert of the active, epistemological role of the media in constructing science in the context of the famous physicist Richard Feynman's 'discovery' of the cause of the explosion of the space shuttle Challenger.164

The call for the investigation of the construction of scientific facts in terms of their transformation and stabilisation between and beyond expert contexts can be found in the work of Callon and Latour, Radder and Mackenzie.165 In these studies, it is argued that the creation and acceptance of scientific/technical facts is reliant on the successful engineering and manipulation of complementary changes to the environments that 'scientific facts' are linked into and operate in.

This can involve Pasteur restructuring - or 'pasteurising' - French society to re-create his laboratory work in it166 or military missile guidance experts
transforming whole communities, over thousands of kilometres, into a giant laboratory. Considerations about scientific facts and, by implication, disputes about them must be linked to understanding the relevant networks in which they are embedded and which sustain them. As the analyst is pre-occupied with identifying and characterising the nature of these networks and the strategies used by actors within them and in manipulating their structure, traditional considerations of macro/micro, expert/non-expert context take on no a priori significance. As Callon and Latour put it:

A macro-actor, as we have seen, is a micro-actor seated on black boxes, a force capable of associating so many other forces that it acts like a 'single man'. The result is that a macro-actor is by definition no more difficult to examine than a micro-actor. Growth is only possible if one can associate long lasting forces with oneself and thereby simplify existence. Hence a macro-actor is at least as simple as a micro-actor since otherwise it could not have become bigger. We do not draw closer to social reality by descending to micro-negotiations or by rising towards the macro-actors. We must leave behind the preconceptions which lead us to believe that macro-actors are more complicated than micro-actors ... A macro-actor can only grow if it simplifies itself. As it simplifies its existence, it simplifies the work of the sociologist. It is no more difficult to send tanks into Kabul than to dial 999. It is no more difficult to describe Renault than the secretary who takes telephone calls at the Houston police station.

Mackenzie adds to this analysis the observation that STCs tend to provide a window into observing the otherwise concealed attempts by actors to achieve sustainability for their claims by enabling the movement of such claims between local and macro contexts.

... the significance of controversy is that in controversy scientists, mathematicians and technologists struggle over locality. They seek to show that their opponents claims are only local achievements. In the absence of controversy the move from the local to the non-local tends to go smoothly. It can appear as the operation of method, logic, rationality, considerations of technical efficacy, etc. Controversy opens it up to public view.

The call for replacement of traditional views of the popularisation of science, with concepts which recognise the epistemological importance of the actual construction of science in popular settings, has been made by writers such as Shinn and Whitley, Hilgartner and Wynne. For instance, in an introductory essay to an important collection of papers on scientific popularisation, Shinn and Whitley challenge traditional views in which
'scientific popularisation is irrevocably separated from the central core of scientific research and thus from the process of knowledge production.'

Shinn and Whitley recommend a number of shifts in conceptions. First, they argue that the concept of popularisation should give way to the alternative concept of exposition.

Exposition refers to the entire gamut of techniques and strategies that mark the formal exchanges that occur within the sphere of science where the latter is very broadly defined. Exposition, thus, embraces all the instruments for communicating results and ideas among an extended range of initiators and audiences. This includes specialists conducting research in the same field, specialists communicating across disciplinary boundaries, professors instructing students, scientists and journalists addressing the lay public, and civil servants and industrialists treating matters associated with science and technology. Put differently, exposition is defined here as a sort of continuum of methods and practices utilised both within research and far beyond, for purposes of conveying science-based information, whether as pure cognition, pedagogy, or in terms of social and economic problems.

Second, Whitley argues that there is often the direct involvement in certain areas of science of non-specialist audiences in the shaping of scientific knowledge claims.

Here, the process of communicating ideas and results to non-specialists and non-scientists can affect the direction of research as these different audiences respond in different ways and actively become involved in the research process. Non-specialist audiences thus are not always passive recipients of scientific knowledge in the contemporary, differentiated sciences but can be significant actors in intellectual development so that popularisation often has a direct impact upon what research is done, how it is done and how it is interpreted.

Third, complementing the need to consider the importance of non-expert audiences in science, they also argue that the not uncommon situation of a low degree of internal cohesion within certain specialised scientific fields means that expository practices are epistemologically important in themselves.

Recent work has shown that scientific fields vary considerably in their degree of internal cohesion, of intellectual pluralism, of standardisation of research procedures, of control over performance and significance standards and of formalisation of
symbols ... Thus the 'scientific community' is not a monolithic, stable and wholly autonomous entity but rather comprises a number of variously organised social structures, whose internal relationships change and whose connections with non-scientific groups are also varied and changeable. Furthermore, what constitutes scientific knowledge has changed and depends upon particular social relationships and collective judgements. 'Facts' are socially constructed cognitive objects, liable to reinterpretation and change, which become established through negotiations and extensive communication among scientists. The exposition of research results to scientific audiences is a crucial component of these processes which affects what comes to constitute knowledge in that field at that time. Expository practices are not epistemologically neutral.174

Building on these observations, Hilgartner acknowledges the weaknesses of traditional views of popularisation in their failure to account for:

- the importance of popular knowledge feeding back into the research process; that is,

  scientists learn about fields outside their immediate research areas from popular accounts and these shape their beliefs about the content and conduct of science.175

- the important role of simplification:

  ... simplification is important in scientific work, both within laboratory and in communicating with students, funding sources and specialists in adjacent fields.176

- popularisation as part of the process of transformation and stabilisation of scientific facts; that is, scientific facts do not immediately present themselves for acceptance. The process of stabilisation involves movement outside narrow expert contexts.

Hilgartner also adds to the other studies a cautionary note for the analyst to be aware of the role that the idea of popularised science can play in the rhetoric of scientists, particularly in controversy.

According to Hilgartner, the image of popularised/debased science (scientific knowledge produced at a distance from its purer site of construction) is deployed in two main ways.
First, the image of a debased currency of scientific knowledge can be used by scientists in juxtaposition to the correct pure science that has not been distorted by the path of popularisation, simplification or pressures of policy.

Secondly, the pejorative image of debased popular science is a flexible resource that need not always be deployed. By working from the ideal of the primacy of the esoteric base of knowledge scientists can demand the right to pronounce on whether or not a 'simplification' or 'popularisation' is an appropriate one. As Hilgartner puts it:

... scientific experts enjoy great flexibility in public discourse. On the one hand, when it suits their purposes, they can issue simplified representations for broader audiences; the notion of the appropriate simplification justifies this practice and enables scientists to invest these representations with the authority of the cultural symbol 'science'. On the other hand, scientists at all times can draw on the notion of distortion to discredit publicly available representations.177

It can be argued that these questions in relation to the adequacy of applying sociology of scientific knowledge approaches to controversy, whilst important to acknowledge, are not unmanageable.

The first question as to the adequacy of SSK approaches, that is, whether SSK approaches use controversy as an exemplar to inform theories of science at the expense of proposing adequate theories of scientific controversy per se, can partially be answered by acknowledging that such studies possess a three-way tension in their aims. This involves:

- using controversy studies to test and exemplify epistemological theories of the social construction of science;

- establishing more general theories about the nature of modern science; and

- documenting the specific features of the construction of science in a given controversy.

A failure to acknowledge that these aims, whilst complementary, involve separate detailed discussion, can lead to the problems identified earlier. As will be indicated in the eclectic model proposed at the end of this chapter, using SSK approaches to move beyond documenting specific interests in the
construction of specific knowledge claims, or merely reinforcing the possibility that scientific knowledge can be socially constructed (something assumed prior to engaging in analysis anyway), requires taking into account, and acknowledging, subsidiary arguments and theories relating to broader and more persistent features of science and society, in particular attention to the more homogeneous role of scientific representations which may cut across the more specific contingent nature of scientific practices.

The second question as to the adequacy of SSK approaches to controversy, that is, the practical problems of gaining the appropriate forms of evidence, can be answered in favour of SSK if the analyst is willing to take a lead from those studies mentioned above, such as those by Latour, Hilgartner and Wynne, which suggest the need to broaden the choice of research sites for the construction of scientific knowledge - in a sense developing some kind of sensitivity to technocratic politics types of concerns. Otherwise this problem remains, to some extent, with the SSK analyst. Nevertheless it is a problem shared by SSK and many other forms of sociological investigation attempting to explore contemporary society at a high degree of detail.

The questions posed by symmetry and reflexivity can also be seen as manageable. We can start by analysing the call for the use of new literary forms. New literary forms supposedly address requirements of reflexivity better than traditional ways of writing which grant the author power of producing an authoritative single account of a particular state of affairs. It can be argued though that existing experiments with new literary forms do not really overcome these problems. The use of devices such as multiple voices and continual self-reference by more fully anticipating the various ways a piece of work can be read, arguably, actually expand the power of the author rather than limit it.178 It would appear that traditional representational methods are no more authoritarian and, even if their use involves a degree of sociological irony, this can be seen as preferable to requiring the extensive overthrow of academic conventions or risking the subordination of the aims of understanding the construction of scientific knowledge to exploring the consequences of reflexivity as a philosophical problem in itself.

The other challenges raised by symmetry and reflexivity are no less manageable. Propositions that SSK analysis is flawed unless it engages in criticism of science and the political status quo can be argued to rely on an over-simplification of the role of the legitimatory functions of the
epistemological authority of science - both for science itself and the dominant political *status quo*. For instance, in the case of the former, it can be noted that the authority of contemporary science, more generally, rests on a variety of justifications -- not just its epistemological authority. With the growth of interdisciplinary goal-directed applied research, or what is sometimes described as 'techno-science', the high social status of science is more often than not justified on the basis of its economic, strategic, instrumental and cultural value. Whilst epistemological justifications are still important, and sometimes linked to the above, it remains far from obvious that the SSK analyst's denial of them entails criticisms of these other justifications, nor of scientific activity itself. Similarly, there are a number of important exceptions/complexities which must be considered before linking, by necessity, the epistemological authority of science to legitimation of dominant political authority in contemporary western society.

First, it can be argued that the above position does not adequately acknowledge the diversity, and divergence, of contemporary scientific activity and thus the political positions capable of being legitimised by it. For instance, the epistemological authority of science can be used both in attempting to challenge the dominant political *status quo* as well as criticise it. SSK challenges to the epistemological authority of science then not only potentially critique the existing political *status quo* but also positions opposed to it.

Second, it can be argued that the above position does not adequately acknowledge the potential for the SSK analyst his/herself to use general challenges to the epistemological authority of science to support various competing political positions -- not just critique of the political *status quo*. A good example of this can be found in the work of Douglas and Wildavsky. They attempt to undermine simple notions of the epistemological authority of science by emphasising the intrinsic social construction of knowledge claims about technological risks. They follow on from these observations to reinforce a conservative political position; they argue that the social construction of risk by mainstream social/political groups is preferable to the perceptions of technological risk as constructed by environmental groups, which they criticise for inconsistency in understanding the functioning of contemporary western societies.
Third, and finally, it can be argued there are instances where important political concerns rely on political epistemological justifications ahead of scientific political ones *per se*. This can be seen in the context of political concepts such as democracy and human rights. whilst important tensions between political epistemological and scientific epistemological justifications have been documented in some contemporary scientific controversies, such as the race, gender and IQ debates, maintenance of political authority has often relied on acting to limit the epistemological authority of science in such contexts.\(^{182}\)

Consequently, challenges to the epistemological authority of science do deny an important set of justifications for the elevation of the status of science in contemporary society and some political activities. The above examples, nevertheless, suggest that there can be problems in any kind of *a priori* stipulation that the SSK analyst should, by necessity, be a critic of contemporary science and politics. SSK studies will frequently open up the potential for criticism of science and contemporary society but certainly do not demand it.

This leaves us with the final, more micro-political, problem of symmetry and reflexivity, i.e. the question of the analyst’s involvement in a specific debate. Again, whilst it is no doubt correct that the analyst can easily become enrolled by the parties to a scientific controversy, it would seem that this does not imply the need for the analyst to actually engage in the production of scientific knowledge within a controversy. Part of the difficulties of the proposition that the analyst has to become involved, stems from the conflation of the inability for the analyst to claim epistemological neutrality in any kind of absolute sense and still practise reflexivity and symmetry, and the idea that the analyst can claim to be neutral in the specific context of a given controversy. For instance, whilst an analyst’s account may well display their social interests in relation to being a sociologist of science, it would appear to be far too simplistic to move from the recognition of these interests to interests in the specific outcome of a given controversy. Of course, this may well be an empirical possibility but it would appear to be by no means a methodological necessity.

Finally, if an analyst is concerned with the use to which their findings may be put in a controversy, they do, of course, have the choice not to publish their findings or not to pursue work on topics where they feel their work will be 'misused'. This question is not unlike the ethical one which has been
put to scientists in areas where their research is difficult to monitor socially or is potentially environmentally damaging.\textsuperscript{183}

Overall, it would appear that the management of symmetry and reflexivity provoke important questions that all scholarship should ask in relation to the analyst's role. But it would seem to be inappropriate to turn the need to acknowledge this into an object of study itself at the expense of other research interests.

Not unlike the problems of symmetry and reflexivity, the problem of choice of research site is less one of methodological necessity than choice. The branching out of the sociology of scientific knowledge into broader research sites is an important challenge and could well be a stimulus for a range of future theoretical innovations, such as those observed in our earlier, more detailed discussion.

Keeping these questions in mind, SSK (EPOR and Strong Programme) approaches to controversy do provide numerous analytical possibilities and possess numerous philosophical strengths. The general philosophical arguments surrounding theory-loading and under-determination suggest that there are difficulties in many of the other approaches to controversy that either do not fully address the political shaping of the content of scientific knowledge; or, that are trapped, as are closure studies, into specifying what science is, in a positivist tradition, with all its incumbent difficulties. It would seem, then, that the numerous SSK tools for analysing the construction of science are crucial to appreciating fully the nature of modern scientific controversy.
Conclusions and overview

Considering the strengths and weaknesses of the approaches to controversy that have been noted in this paper, a selective eclectic model for analysing STC can be proposed.

The model proposes that the following twelve factors should be taken into account when analysing STCs.

From controversy as politics the following factors can be drawn:

(i) attention should be given to the broad political cultural factors which help promote scientific technical disagreements to come into the public eye (when controversies, as they frequently do, take on public dimensions);

(ii) an awareness of the more persistent political contingencies which may shape the negotiation of the practical meanings of scientific technical knowledge.

From technocratic politics the following factors can be drawn:

(iii) the acknowledgment of the special role of science as a source for legitimation in contemporary political culture;

(iv) attention to the analysis of how such representations of science are re-created and manipulated during controversies.

From fact/value approaches:

(v) investigation of the ways participants in controversy negotiate the boundaries between appropriate questions for scientific analysis versus political social analysis and the particular forms of rhetoric used in such processes.

From historico-narrative approaches:

(vi) an openness to the specific causal factors that will shape any particular controversy and give it its own unique historical character.
From closure studies:

(vii) investigation of the layering of controversies into linked sub-debates.

From contemporary sociology of scientific knowledge approaches (emphasising Strong Programme and EPOR perspectives):

(viii) incorporation of an understanding and willingness to explore the implications of under-determination and theory loading; treatment of all knowledge claims as symmetrical, open to explanation according to the same epistemological factors; not investigating the content of scientific knowledge from a vantage point of a sociology of error;

(ix) investigation of the various devices used by participants in controversy to try to limit interpretative flexibility in relation to their claims yet open interpretative inflexibility to the claims of rivals - such as flexible evaluative repertoires, images of scientific method and scientific norms;

(x) an openness to unpacking the diverse political/social interests embedded in, and necessary to, making up the content of scientific knowledge claims, such as the respective actors' goals, social locations and institutional or educational backgrounds;

(xi) reflexivity - acknowledging as far as possible the political-social-methodological context of one's own account.

Finally, from emerging critiques within the sociology of scientific knowledge:

(xii) an openness to the negotiation of the construction of the content of science in broader settings, such as legal/regulatory contexts and the mass media.

In simple terms this eclectic approach to controversy is strongly influenced by contemporary sociology of scientific knowledge tools of analysis and philosophical orientation to science. But it is also one that acknowledges the need to focus investigation, not only on the content of science, but on its symbolic and representational dimensions. It is with regard to these later
aims that much of the non-contemporary sociology of scientific knowledge, controversy literature is useful, although problematic in relation to its philosophical underpinnings.
Notes and References


2  It is important to acknowledge my awareness of the limitations of trying to provide an overview of such a large body of literature. For instance, with different analytical aims a different map could be derived. It should also be noted that some newer studies which may not yet represent a crystallized body of thought may have evaded my classification. I am also aware that classificatory exercises risk some degree of inherent philosophical circularity. These problems notwithstanding, the exercise can still be defended as an extremely valuable one for simplifying and guiding research.


5  For a more detailed discussion of Engelhardt & Caplan see discussion in Section 5 'Closure Studies'.


7  *ibid.*, p.511.

8  For a more detailed discussion of Nelkin's work see Section 3 'Technocratic Politics'.

9  e.g. in his widely quoted *Alternative Technology*, Fontana, London, 1974.

10 It should be noted however that in other places Martin & Richards have argued the importance of SSK analysis having a more overt political orientation. For a discussion and critique see Section 7 'SSK'.


12 *ibid.*, p.5.

13 *ibid.*, p.7.
14 *ibid.*, p.9.

15 *ibid.*, p.9.

16 *ibid.*, p.16.


23 *ibid.*, p.84.

24 *ibid.*, p.89.


26 The degree to which scientific/technical categories can be read off the politics of class relations is a complex debate within Marxist sociology of knowledge. For our purposes the way scientific/technical categories are given meaning in practical context is sufficient. For a consistent but deeper reading see Young, 1990, *op. cit.*, p.80, quoting Lukacs:

For the Marxist as an historical dialectician both nature and the forms in which it is mastered in theory and practice are social categories; and to believe that one can detect anything supra-historical or supra-social in this context is to disqualify oneself as a Marxist.


34 J Wajcman, *op. cit.*, p.5. See also E Showalter, *op. cit.*, and for feminist discourse on hysteria, Ehrenreich & English, *op. cit.*


39 See Nelkin, 1987, op. cit., where she uses the phrase (p.289) 'intellectual technocracy' and points out (p.291):

Disputes have broadened the scope of scientific research and created new positions in an increasing number of policy areas, providing material benefits to individuals through new consulting and advisory positions.

40 See discussion in ibid., p.292, quoting L Tribe, who talks of the application of scientific styles of reasoning to inappropriate contexts such as: '... fragile values ... non-quantifiable, intangible resistant to categorisation.'


44 Nelkin, 1979, op. cit., p.10.

45 ibid., p.10.

46 ibid., p.9.


49 See e.g. discussion in Wynne, 1982, op. cit.


51 e.g. Nelkin's collection, 1979, op. cit.; Barnes & Edge (eds.), op. cit.


53 Albury, op. cit.

54 Rip, op. cit.

55 See D Nelkin, Selling Science, WH Freeman, New York, 1987, ch.4 'The Perils of Progress', pp.53-69, on the tendency for difficult questions or contextual issues to be glossed over in media treatment of controversial science. These trends have also been documented in e.g. G Jones, J Connell & A Meadows, 'The Presentation of Science by the
Media', Leicester PCRC, University of Leicester, 1978. Part of these difficulties stems from the heavy reliance by those writing historico-narrative approaches on scientists for their definitions of science and scientific activity. Jones, Connell & Meadows described this reciprocity of perspective in the following terms (pp.44-45):

Such work [producing science programmes] requires the joint labours of journalists and scientists. It is work which is possible week after week, because the scientists occupy the same cultural/ideological space. Those antagonisms which do break out from time to time (especially between current affairs broadcasters and scientists) are seldom of a fundamental kind, in the sense that participants hold entirely different and contradictory views of the nature and scope of scientific work. Broadcasters employed in making programmes about science will normally hold essentially the same views and ideals about science as the scientists themselves. Even though not all broadcasters will articulate those views and ideals, they will be tacitly reproduced by them when they depict scientific practices as 'pure' and governed by a monolithic and unproblematic 'scientific method'. The critical approach that is sometimes adopted [in current affairs coverage] does not question the dominant views and ideals of science, but rather the uses of which pristine scientific knowledge has been put.

See also D Mercer, Myth and Contemporary Popular Images of Science/The Politics of Science in the Mass Media, unpublished BA (Hons) thesis, University of NSW Department of History & Philosophy of Science, 1985, ch.9 'Sources for Popular Images of Science: the Relation of Science to the Mass Media'

The types of literature in mind here would be journalistic exposés such as Brodeur, Zapping of America or Currents of Death, and reviews of controversy such as those found in journals like New Scientist.

56 The types of literature in mind here would be journalistic exposés such as Brodeur, Zapping of America or Currents of Death, and reviews of controversy such as those found in journals like New Scientist.

57 Mazur has written numerous articles on STCs: e.g. 'Disputes between Experts', Minerva, Vol.XI, 2 Apr 1973; 'The Rise and Fall of Public Opposition in Specific Social Movements', Social Studies of Science, Vol.10, 1980, pp.259-284; 'Scientific Disputes over Policy', in Engelhard & Caplan, 1987, op. cit., pp.265-282 Much of Mazur's approach is synthesised in his book The Dynamics of Technical Controversy, 1981. It is also useful to note the frequent implicit adoption of such a dichotomy in many policy settings, particularly in the field of risk assessment.


59 ibid., pp.120-124.


61 Mazur, 1981, op. cit., p.114. At this very general level of analysis, Mazur's work has some similarities with 'controversy as politics'. Unlike this approach, as the ensuing discussion with indicate, Mazur pays more attention to patterns of the construction of scientific claims within controversies.

62 ibid., p.267.

63 ibid., pp.34-41.

64 ibid., p.20.
ibid., pp.14-29.

ibid., p.23.

ibid., pp.27-29.


ibid., p.281.

ibid., p.281.


Mazur, 1987, op. cit., p.274, proposes:

... we never make a point of bringing housewives and blue-collar labourers into formal decisions about the prime interest rate or on whether or not to attack Iran, so why do it when evaluating nuclear power plants and recombinant DNA laboratories?


Mazur, 1981, op. cit., p.34. One of the weaknesses of overviews of controversy that focus excessively on epistemological positions of the studies they are analysing is that in policy discussions it is not uncommon to see a degree of inconsistency. Distinguishing facts from values is often seen as a pragmatic rather than a strictly epistemological exercise. For an example of an approach which attempts to maintain a realist epistemology of science but also acknowledge the pragmatic difficulties of separating facts from values, see discussion of the so-called 'neo-separationist' policy option in JD Graham, L Green & M Roberts, In Search of Safety: Chemicals and Cancer Risk, Harvard University Press, Cambridge MA 1988, pp.179-219 'Science and Policy Conflict', esp. 217-219.


Nelkin, 'Controversies and the Authority of Science', in Engelhardt & Caplan (eds.), op. cit.


ibid., p.10.
Other closure theorists, such as McMullen, differentiate these forms of sound argument closure thus: epistemic with strict closure equalling standard epistemic arguments; and broad sound argument closure equalling non-standard epistemic closure. E McMullen, 'Scientific Controversy and Its Termination', in Engelhardt & Caplan (eds.), op. cit., pp.49-91, esp. pp.60-64.


There are numerous overviews explaining the basic implications of theory-loading and under-determination for the social and historical study of science. See M Mulkay, 1979, op. cit., esp. chs.1 & 2; A Chalmers, What is this Thing Called Science?, University of Queensland Press, St Lucia, 1976; D Oldroyd, The Arch of Knowledge, NSW University Press, Kensington NSW, 1986.


See discussion in R Krohn, 'Towards the Empirical Study of Scientific Practice' in KD Knorr, R Krohn & R Whitley (eds.), The Social Process of Scientific Investigation, Reidel, Dordrecht, 1980, esp. p.xvii. It is important to note that this does not mean various of the competing approaches in SSK do not in themselves specify programmes of how to study science. See the later discussion of the Strong Programme and EPOR.

309. It is also useful to note that some studies working in strongly reflexive modes also transcend social realism. See the discussion later.


108 Knorr-Cetina & Mulkay, *op. cit.*, pp.3-4. However, many studies steer very close to this.


115 See discussion in Collins & Yearley, 1992, op. cit. A fruitful recent exchange on the tension between these different branches of contemporary sociology of science can be found in the collection edited by Pickering. See especially responses to Collins & Yearley's 'Epistemological Chicken': S Woolgar, 'Some Remarks about Positionism: a Reply to Collins and Yearley', p.327-342; M Callon & B Latour, 'Don't Throw The Baby Out With the Bath School!: A Reply to Collins and Yearley', pp.343-368; and HM Collins & S Yearley, 'Journey into Space', pp.369-389. See also discussion in A Pickering, 'From Science as Knowledge to Science as Practice', pp.1-26; Knorr-Cetina & Mulkay, 1983, op. cit., pp.9-11.

116 ibid., pp.6-9.


It is important for the sociology of scientific knowledge that external factors can be brought into the analysis. This would complete the third stage of EPOR ... Although this is the explanatory goal of the programme most studies, including the present work, focus more upon the social processes to be found within the scientific community.


121 Bloor, 1976, op. cit., p.144.

Bloor, 1976, op. cit., p.5.

ibid., p.6-13.


ibid., p.8.


ibid., pp.486-487.

ibid., pp.485. 'All that has gone before stands formally as a resource from which what is to come is developed.'


As Mulkay points out in 'Knowledge and Utility', Social Studies of Science, Vol.9, 1979, p.69, the failure or efficacy of knowledge claims in practical contexts should not be used to undermine epistemological symmetry, that is, under-determination implies epistemologically incompatible theories may well be capable of supporting equivalent practical outcomes.

Knorr-Cetina & Mulkay, 1983, op. cit., point out the need to distinguish between epistemic and judgemental relativism, pp.5-6.


Pinch, 1985, op. cit., p.29.

ibid.

ibid.


HM Collins, 'The Place of the "Core Set" in Modern Science: Social Contingency with Methodological Propriety in Science', History of Science, No.19, pp.6-19.
142 Pinch, in Olby & Cantor (eds.), op. cit., p.90.

143 Mulkay & Knorr-Cetina, 1983, op. cit.


145 Collins & Yearley, 1992, op. cit.

146 Mulkay, 1985, op. cit., p.43.

147 See discussion in Mulkay, 1976, op. cit.


153 Woodhouse reviews a number of recent studies attempting to branch out from the insights of the micro-construction of science to broader politics: E Woodhouse, 'The Turn Toward Society? Social Reconstruction of Science', Science, Technology and Human Values, Vol.16, No.3, Summer 1991, pp.390-404. The tension between the heterogeneity of science as practice versus the homogeneity of forms of scientific representation appears in a number of case studies linking in situ deconstruction of specific scientific knowledge claims with broader claims of deconstructing the authority of science in more general terms.

154 This problem is acknowledged in part by Pinch, in Olby & Cantor (eds.), op. cit.

155 For the purposes of this paper, the philosophical issue of 'infinite regress', that is, the implication that the analyst accepts his/her account can be explained by the same tools it uses to explain other accounts, can be taken as being adequately addressed in the existing literature. B Barnes & D Bloor, 'Relativism, Rationalism and the Sociology of Scientific Knowledge', in M Hollis & S Lukes (eds.), Rationality and Relativism, Blackwell, Oxford, 1982, pp.21-47; B Barnes, 'Sociological Theories of Scientific Knowledge', in Olby & Cantor (eds.), op. cit., pp.60-73.

156 S Woolgar, 'Irony in the Social Studies of Science', in Mulkay & Knorr-Cetina (eds.), op. cit., pp.239-256. See also S Woolgar, 'Some Remarks ...', 1992, op. cit, pp.337-42; Mulkay, 1985, op. cit. See also footnote 101.

157 See discussions of the so-called 'ideology of science' in Albury, 1983, op. cit.; Mulkay 1976, op. cit.; plus Ezrahi, op. cit.

See discussion in e.g. B. Martin, The Bias of Science, Society for Social Responsibility in Science, Canberra, 1979.


ibid.


Mackenzie, 1988, op. cit.


Ibid., p.viii.


Ibid., p.11.


Ibid.

Ibid.

Collins & Yearley, 1992, *op. cit.*


This can be shown clearly in public discourse in the so-called 'Two Cultures Debate' from the time of CP Snow's influential essay of the 1950s. Snow assumes the epistemological authority of science but largely argues for the superiority of science on its strategic economic instrumental value.


See discussion in Y Ezrahi, *op. cit.* and Edge & Cameron, *op. cit.* This argument is basically a variety of the so-called 'naturalistic fallacy' or the so-called 'is/ought' debate. For brief discussion of this debate see A Bullock & O Stallybrass (eds.), *Fontana Dictionary of Modern Thought*, 5th impression, Fontana, London, 1982, p.412.
