DISCUSSION PAPER

● *ABSTRACT*

The need for an integrated social constructivist approach towards the study of science and technology is outlined. Within such a programme both scientific facts and technological artefacts are to be understood as social constructs. Literature on the sociology of science, the science-technology relationship, and technology studies is reviewed. The empirical programme of relativism within the sociology of scientific knowledge and a recent study of the social construction of technological artefacts are combined to produce the new approach. The concepts of 'interpretative flexibility' and 'closure mechanism', and the notion of 'social group' are developed and illustrated by reference to a study of solar physics and a study of the development of the bicycle. The paper concludes by setting out some of the terrain to be explored in future studies.

The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other

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One of the most striking features of the growth of 'science studies' in recent years has been the separation of science from technology. Sociological studies of new knowledge in science abound, as do studies of technological innovation, but thus far there has been little attempt to bring such bodies of work together.[1] It may well be the

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case that science and technology are essentially different and that different approaches to their study are warranted. However, until the attempt to treat them within the same analytical endeavour has been undertaken we cannot be sure of this.

It is the contention of this paper that the study of science and the study of technology should, and indeed can, benefit from each other. In particular we will argue that the social constructivist view prevalent within the sociology of science, and which is also emerging within the sociology of technology, provides a useful starting point. We will set out the constitutive questions which such a unified social constructivist approach must address analytically and empirically. But our intention is not just to make a programmatic appeal: empirical examples, drawn from our own work on science and technology, will be used to illustrate the potential of our programme.[2]

The paper falls into three main sections. In the first part, we will outline various strands of argumentation and review bodies of literature which we consider to be relevant to our goals. We will then go on to discuss the two specific approaches from which our integrated viewpoint has developed: the Empirical Programme of Relativism[3] and a social constructivist approach to the study of technology.[4] In the third part, we will bring these two approaches together and give some empirical examples. We will conclude by summarizing our provisional findings, and indicate the directions in which we believe the programme can most usefully be pursued.

Some Relevant Literature

In this section we draw attention to three bodies of literature in science and technology studies. The three areas discussed are the sociology of science, the science-technology relationship, and technology studies. We will take each in turn.

Sociology of Science

It is not our intention to review in any depth developments in this field as a whole.[5] We are concerned here only with the recent emergence of the sociology of scientific *knowledge*.[6] Studies in this area take the actual content of scientific ideas, theories, and experiments as the subject of analysis. This contrasts with earlier work in the sociology of science which was concerned with science as an institution and the study of scientists' norms, career patterns, and reward structures.[7] One major — if not *the* major — development in the field in the last decade has been the extension of the sociology of knowledge into the arena of the 'hard sciences'. The need for such a 'strong programme' has been outlined by Bloor:[8] its central tenets are that, in investigating the causes of beliefs, sociologists should be impartial to the truth or falsity of the beliefs, and that such beliefs should be explained symmetrically. In other words, differing explanations should not be sought for what is taken to be a scientific 'truth' (for example, the existence of X-Rays) and a scientific 'falsehood' (for example, the existence of N-Rays). Within such a programme all knowledge and all knowledge-claims are to be treated as being socially constructed: that is to say, explanations for the genesis, acceptance and rejection of knowledge-claims are sought in the domain of the Social World rather than in the Natural World.[9]

This approach has generated a vigorous programme of empirical research and it is now possible to understand the processes of the construction of scientific knowledge in a variety of locations and contexts. For instance, one group of researchers has concentrated their attention on the study of the laboratory bench.[10] Another has chosen the scientific controversy as the location for their research and have thereby focussed on the social construction of scientific knowledge amongst a wider community of scientists.[11] As well as in 'hard' sciences, such as physics and biology, the approach has been shown to be fruitful in the study of fringe science,[12] and in the study of public-science debates, such as lead pollution.[13]

Although there are the usual differences of opinion among researchers as to the best place to locate such research (for instance, the laboratory, the controversy or the scientific paper), and there are differences as to the most appropriate methodological strategy to pursue,[14] there is widespread agreement that scientific knowledge can be, and indeed has been, shown to be thoroughly socially constituted. These approaches, which we shall refer to as 'social constructivist', mark an important new development in the sociology of science. The treatment of scientific knowledge as a social construction implies that there is nothing epistemologically special about the nature of scientific knowledge: it is merely one in a whole series of knowledge cultures (including, for instance, the knowledge systems pertaining to 'primitive' tribes).[15] Of course, the successes and failures of certain knowledge cultures still need to be explained, but this is to be seen as a sociological rather than an epistemological task.

The sociology of scientific knowledge promises much for other areas of 'science studies'. For example, it has been argued that the new work has relevance for the history of science,[16] philosophy of science,[17] and science policy.[18] The social constructivist view seems not only to be gaining ground as an important body of work in its own right: it also shows every potential of wider application. It is this body of work which forms one of the pillars of our own approach towards the study of science and technology.

Science-Technology Relationship

The literature on the relationship between science and technology, unlike that referred to above, is rather heterogeneous and includes contributions from a variety of disciplinary perspectives. We do not claim to present anything other than a very partial review, reflecting our own particular interests.

One theme which has been pursued by philosophers is the attempt to separate technology from science on analytical grounds. In doing so, philosophers tend to posit over-idealized distinctions, such as, for example, that science is about the discovery of truth whilst technology is about the application of truth. Indeed, the literature on the philosophy of technology is rather disappointing.[19] We prefer to suspend judgement on it until philosophers propose more realistic models of both science and technology.

Another line of investigation into the nature of the science-technology relationship has been carried out by innovation researchers. They have attempted to investigate empirically the degree to which technological innovation incorporates, or originates from, basic science. A corollary of this approach has been the work of some scholars who have looked for relationships in the other direction — that is, they have argued that pure science is indebted to developments in technology.[20] The results of the empirical investigations of the dependence of technology on science have been rather frustrating. It has been difficult to specify the interdependence. For example, Project Hindsight, funded by the US Defense Department, found that most technological growth came from mission-oriented projects and engineering R & D, rather than from pure science.[21] These results were to some extent supported by a later British study.[22] On the other hand, Project TRACES, funded by the NSF in response to Project Hindsight, found that most technological development stemmed from basic research.[23] All these studies have been criticized for lack of methodological rigour and one must be cautious in drawing any firm conclusions from such work.[24] Most researchers today seem willing to agree that technological innovation takes place in a wide range of circumstances and historical epochs and that the import which can be attached to basic science therefore probably varies considerably.[25] Certainly the view prevalent in the 'bad old days'[26] that science discovers and technology applies will no longer suffice. Simplistic models and generalizations have been abandoned. As Layton has remarked in a recent review:

> Science and technology have become intermixed. Modern technology involves scientists who 'do' technology and technologists who function as scientists ... The old view that basic sciences generate all the knowledge which technologists then apply will simply not help in understanding contemporary technology.[27]

Researchers concerned to measure the exact interdependence of science and technology seem to have asked the wrong question because they have assumed science and technology to be well-defined monolithic structures. In short, they have not grasped that science and technology are themselves socially produced in a variety of social circumstances.[28] It does seem, however, that there is now a move towards a more sociological conception of the science-technology relationship. For instance, Layton writes:

> The divisions between science and technology are not between the abstract functions of knowing and doing. Rather they are social...[29]

Barnes has recently described this change of thinking as follows:

> I start with the major reorientation in our thinking about the science-technology relationship which has occurred in recent years ... We recognize science and technology to be on a par with each other. Both sets of practitioners creatively extend and develop their existing culture; but both also take up and exploit some part of the culture of the other ... they are in fact enmeshed in a symbiotic relationship.[30]

Although Barnes may be overly optimistic in claiming that a 'major reorientation' has occurred, it can be seen that a social constructivist view of science and technology fits well with his conception of the science-technology relationship. Scientists and technologists can be regarded as constructing their respective bodies of knowledge and techniques with each drawing upon the resources of the other when and where such resources can profitably be exploited. In other words, science and technology are both socially constructed cultures and bring to bear whatever cultural resources are appropriate for the purposes at hand. In this view the boundary between science and technology is, in particular instances, a matter for social negotiation, and represents no underlying distinction: it then makes little sense to treat the science-technology relationship in a general unidirectional way. Although we will not pursue this issue further in this paper, the social construction of the science-technology relationship is clearly a matter deserving further empirical investigation.

Technology Studies

Our discussion of work under this heading is even more schematic. There is a very large amount of writing which falls under the rubric of 'technology studies'. It is convenient to divide the literature into three parts — innovation studies, history of technology, and sociology of technology. We will discuss each in turn.

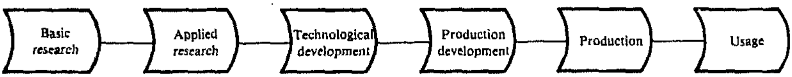
Most innovation studies have been carried out by economists looking for the conditions for success in innovation. Factors researched include various aspects of the innovating firm (for example, size of R & D effort, management strength and marketing capability) along with macro-economic factors pertaining to the economy as a whole. This literature is in some ways reminiscent of the early days in the sociology of science, when scientific knowledge was treated like a 'black box'[31] and, for the purpose of such studies, scientists might as well have produced meat pies. Similarly, in the economic analysis of technological innovation everything is included that might be expected to influence innovation, except any discussion of the technology itself. As Layton notes:

> What is needed is an understanding of technology from inside, both as a body of knowledge and as a social system. Instead, technology is often treated as a 'black box' whose contents and behaviour may be assumed to be common knowledge.[32]

Only recently have economists started to look into this black box.[33]

The failure to take into account the content of technological innovations results in the widespread use of simple linear models to describe the process of innovation. The number of developmental steps assumed in these models seem to be rather arbitrary (for an example of a six-stage process see Figure 1).[34] Although such studies have undoubtedly contributed much to our understanding of the conditions for economic success in technological innovation,[35] because they ignore the technological content they cannot be used as the basis for a social constructivist view of technology.

FIGURE 1
A Six-Stage Linear Model of the Innovation Process



This criticism cannot be levelled at the history of technology, where there are many finely crafted studies of the development of particular technologies. However, for the purposes of a sociology of technology, this work presents two kinds of problem. The first is that descriptive historiography is endemic in this field. Very few scholars (but there are some notable exceptions — see below) seem concerned to generalize beyond historical instances, and it is difficult to discern any overall patterns upon which to build a theory of technology.[36] This is not to say that such studies might not be useful building bricks for a social constructivist view of technology — merely that these historians have not yet demonstrated that they are doing sociology of knowledge in a different guise.[37]

The second problem concerns the asymmetrical focus of the analysis. For example, it has been claimed that in 25 volumes of *Technology and Culture* only 9 articles were devoted to the study of failed technological innovations.[38] This contributes to the implicit adoption of a linear structure of technological development, which suggests that

> . . .the whole history of technological development had followed an orderly or rational path, as though today's world was the precise goal toward which all decisions, made since the beginning of history, were consciously directed.[39]

This preference for successful innovations seems to lead scholars to assume that the success of an artefact is an explanation of its subsequent development. Historians of technology often seem content to rely on the manifest success of the artefact as evidence that there is no further explanatory work to be done. For example, many histories of synthetic plastics start by describing the 'technically sweet' characteristics of Bakelite: these features are then used implicitly to position Bakelite at the starting point of the glorious development of the field:

> God said: 'let Baekeland be' and all was plastics![40]

However, a more detailed study of the developments of plastic and varnish chemistry following the publication of the Bakelite process in 1909[41] shows that Bakelite was at first hardly recognized as the marvellous synthetic resin which it later proved to be.[42] And this situation did not change very much for some ten years. During the first world war the market prospects for synthetic plastics actually grew worse. However, the dumping of war supplies of phenol (used in the manufacture of Bakelite) in 1918 changed all this,[43] and made it possible to keep the price sufficiently low to compete with (semi-)natural resins, such as celluloid. One can speculate over whether Bakelite would have acquired its prominence if it had not profited from that phenol dumping. In any case it is clear that a historical account founded upon the retrospective success of the artefact leaves much untold.

Given our intention of building a sociology of technology which treats technological knowledge in the same symmetrical, impartial manner that scientific facts are treated within the sociology of scientific knowledge, it would seem that much of the historical material does not go far enough. The success of an artefact is precisely what needs to be explained. For a sociological theory of technology it should be the *explanandum*, not the *explanans*.

Our account would not be complete, however, without mentioning some recent developments, especially in the American history of technology. These show the emergence of a growing number of theoretical themes upon which research is focussed.[44] For example, the systems approach towards technology,[45] and consideration of the effect of labour relations on technological development,[46] seem to herald departures from the 'old' history of technology. Such work promises to be very valuable for a sociological analysis of technology, and we shall return to some of it below.

The final body of work we wish to discuss is what might be described as 'sociology of technology'.[47] There have been some limited attempts in recent years to launch such a sociology, using ideas developed in the history and sociology of science — studies by, for example, Johnston[48] and Dosi,[49] who advocate the description of technological knowledge in terms of Kuhnian paradigms.[50] Such approaches certainly appear to be more promising than standard descriptive historiography, but it is not clear whether these authors share our understanding of technological artefacts as social constructs. For example, neither Johnston nor Dosi consider explicitly the need for a symmetrical sociological explanation which treats successful and failed artefacts in an equivalent way. Indeed, by locating their discussion at the level of technological paradigms it is not clear how the artefacts themselves are to be approached. As neither author has yet produced an empirical study using Kuhnian ideas, it is difficult to evaluate how the Kuhnian terms may be operationalized.[51] Certainly this has been a pressing problem in the sociology of science, where it has not always been possible to give Kuhn's terms a clear empirical reference.[52]

The possibilities of a more radical social constructivist view of technology have recently been touched upon by Mulkay.[53] He argues that the success and efficacy of technology could pose a special problem for the social constructivist view of *scientific knowledge*. The argument Mulkay wishes to counter is that the practical effectiveness of technology somehow demonstrates the privileged epistemology of science, and thereby exempts it from sociological explanation. Mulkay opposes this view, rightly in our opinion, by pointing out the problem of the 'science discovers, technology applies' notion implicit in such claims. In a second argument against this position, Mulkay notes (following Mario Bunge)[54] that it is possible for a false, or partly-false, theory to be used as the basis for successful practical application: the success of the technology would not then have anything to say about the 'truth' of the scientific knowledge upon which it was based. We find this second point not entirely satisfactory. We would rather stress that the truth or falsity of scientific knowledge are irrelevant to sociological analysis of belief: to retreat to the argument that science may be wrong but good technology can still be based upon it is to miss this point. Furthermore, the success of technology is still left unexplained within such an argument. The only effective way to deal with these difficulties is to adopt a perspective which attempts to show that technology, as well as science, can be understood as a social construct.

Mulkay seems to be reluctant to take this step because, as he points out, 'there are very few studies ... which consider how the technical meaning of hard technology is socially constructed'.[55] This situation however, is starting to change: a number of such studies have recently emerged. For example, Michel Callon, in a pioneering study, has shown the effectiveness of focussing upon technological controversies. He draws upon an extensive case-study of the electric vehicle in France (1960-75) to demonstrate that almost everything is negotiable: what is certain and what is not; who is a scientist and who is a technologist; what is technological and what is social; and who can participate in the controversy.[56] David Noble's study of the introduction of numerically-controlled machine tools can also be regarded as an important contribution to a social constructivist view of technology.[57] Noble's explanatory goals come from a rather different (Marxist) tradition,[58] and his study has much to recommend it: he considers the development of both a successful and failed technology, and gives a symmetrical account of both developments. Another intriguing study in this tradition is Lazonick's account of the introduction of the self-acting mule:[59] he shows that aspects of this technical development can be understood in terms of the relations of production rather than any inner logic of technological development. The work undertaken by Bijker, Bönig and Van Oost is another attempt to show how the socially constructed character of the content of some technological artefacts might be approached empirically:[60] six case studies were carried out, using historical sources.

In summary, then, we can say that the predominant traditions in technology studies — innovation studies and the history of technology — do not yet provide much encouragement for our programme. However, there are exceptions, and some very recent studies in the sociology of technology form promising starts upon which a unified approach could be built. We will now give a more extensive account of how these ideas may be synthesized.

EPOR and SCOT

In this part of the paper we outline in more detail the concepts and methods which we wish to employ. We start by describing the Empirical Programme of Relativism as it has been developed in the sociology of scientific knowledge. We then go on to discuss in more detail one approach taken in the sociology of technology.

The Empirical Programme of Relativism (EPOR)

The EPOR is an approach which has produced several studies demonstrating the social construction of scientific knowledge in the 'hard' sciences. This tradition of research has emerged from recent sociology of scientific knowledge. Its main characteristics, which distinguish it from other approaches in the same area, are the focus upon the empirical study of contemporary scientific developments, and the study, in particular, of scientific controversies.[61]

Three stages in the explanatory aims of the EPOR can be identified. In the *first stage* the interpretative flexibility of scientific findings is displayed — in other words, it is shown that scientific findings are open to more than one interpretation. This shifts the focus for the explanation of scientific developments from the Natural World to the Social World. However, although this interpretative flexibility can be recovered in certain circumstances, it remains the case that such flexibility soon disappears in science — that is to say, a scientific consensus will usually emerge as to what the 'truth' is in any particular instance. Social mechanisms which limit interpretative flexibility, and thus allow scientific controversies to be terminated, are described in the *second stage*. A *third stage*, which has not yet been carried through in any study of contemporary science, is to relate such 'closure mechanisms' to the wider social-cultural milieu. If all three stages were to be addressed in a single study, as Collins writes, 'The impact of society on knowledge "produced" at the laboratory bench would then have been followed through in the hardest possible case'.[62]

The EPOR represents a continuing effort by sociologists to understand the content of the natural sciences in terms of social construction. Various parts of the programme are better researched than others. The third stage of the programme has not yet even been addressed: but there are many excellent studies exploring the first stage. Most current research is aimed at elucidating the 'closure mechanisms' whereby consensus emerges (the second stage). Many studies within the EPOR have been most fruitfully located in the area of scientific controversy. Controversies offer a methodological advantage in the comparative ease with which they reveal the interpretative flexibility of scientific results. Interviews conducted with scientists engaged in a controversy usually reveal strong and differing opinions over scientific findings. As such flexibility soon vanishes from science, it is difficult to recover from the textual sources with which historians usually work. Collins has highlighted the importance of the 'controversy group' in science by his use of the term 'Core-Set'.[63] These are the scientists most intimately involved in a controversial research topic. Because the 'Core-Set' is defined in relation to knowledge production in science (the 'Core-Set' constructs scientific knowledge) some of the empirical problems encountered in the identification of groups in science by purely sociometric means can be overcome. And studying the Core-Set has another methodological advantage, in that the resulting consensus can be monitored. In other words, the group of scientists who experiment and theorize at the research frontiers, and who become embroiled in scientific controversy, will also reflect the growing consensus as to the outcome of that controversy. The same group of 'Core-Set' scientists can then be studied in both the first and second stages of the EPOR.[64]

Social Construction of Technology (SCOT)

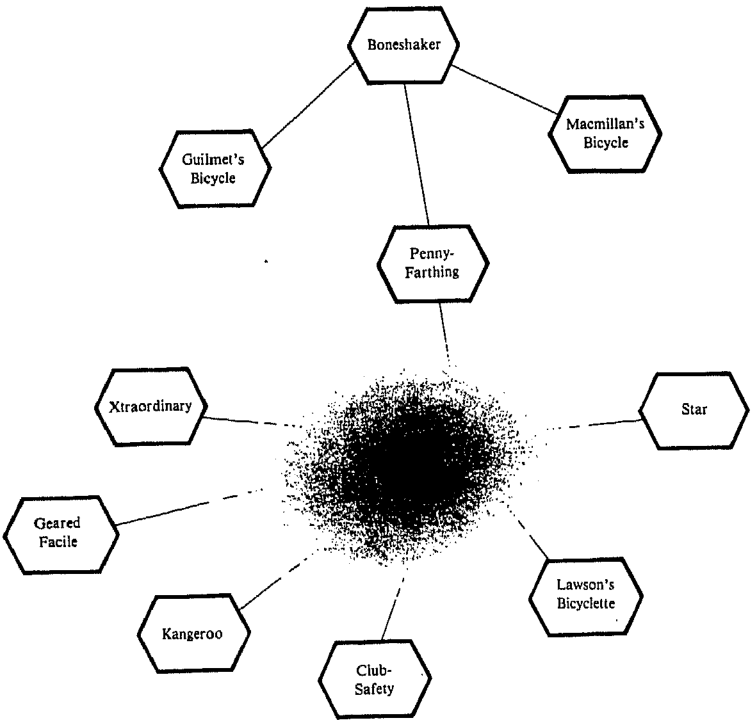
Before outlining some of the concepts found to be fruitful by Bijker and his collaborators in their studies in the sociology of technology, we should point out an imbalance between the two approaches (EPOR and SCOT) we are considering. The EPOR is part of a flourishing tradition in the sociology of scientific knowledge: it is a well-established programme supported by much empirical research. In contrast, the sociology of technology is an embryonic field with no well-established traditions of research: and the approach we draw upon specifically (SCOT) is only in its early empirical stages. Most readers, whilst having some familiarity with the EPOR, will probably be unaware of the concepts employed in SCOT. In bringing together a mature research tradition and an embryonic one, there is a danger that the reader will interpret the imbalance to the detriment of the 'younger' partner. In an attempt to engender a more sympathetic reading, we will devote considerable space to outlining some of the concepts and empirical material used in SCOT. Of course, the feeling of imbalance will persist, since it is a real imbalance. However, by being honest about the status of our studies we hope it will be seen more clearly what has and what has not been achieved.

In SCOT, the developmental process of a technological artefact is described as an alternation of variation and selection.[65] This results in a 'multi-directional' model, in contrast with the linear models used explicitly in many innovation studies, and implicitly in much history of technology. Such a multi-directional view is essential to any social constructivist account of technology. Of course, with historical hindsight, it is possible to collapse the multi-directional model onto a simpler linear model; but this misses the thrust of our argument that the 'successful' stages in the development are not the only possible ones.

Let us consider the development of the bicycle.[66] Applied to the level of artefacts in this development, this multi-directional view results in the description summarized in Figure 2. Here we see the artefact 'Ordinary' (or, as it was nicknamed after becoming less ordinary, the 'Penny-farthing'), and a range of possible variations. It is important to recognize that, in the view of the actors of those days, these variants were at the same time very different from each other and equally were serious rivals. It is only by retrospective distortion that a quasi-linear development emerges, as depicted in Figure 3. In this representation the so- called 'safety ordinaries' ('Xtraordinary' [1878], 'Facile' [1879], and 'Club Safety' [1885]) only figure as amusing aberrations which need not be taken seriously. Such a retrospective description can be challenged by looking at the actual situation in the 1880s. Some of the 'safety ordinaries' were produced commercially, whilst Lawson's 'Bicyclette', which seems to play an important role in the linear model, proved to be a commercial failure.[67]

However, if a multi-directional model is adopted, it is possible to ask why some of the variants 'die', whereas others 'survive'. To illuminate this 'selection' part of the developmental processes, let us consider the problems and solutions presented by each artefact at particular moments. The rationale for this move is the same as that for focussing upon scientific controversies within EPOR — in this way, one can expect to bring out more clearly the interpretative flexibility of technological artefacts.

FIGURE 2
A Multi-Directional View of the Developmental Process of the Penny-Farthing Bicycle



Note: the grey area has been filled in and magnified in Figure 7.

Key for Figures 2-7

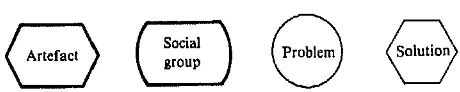
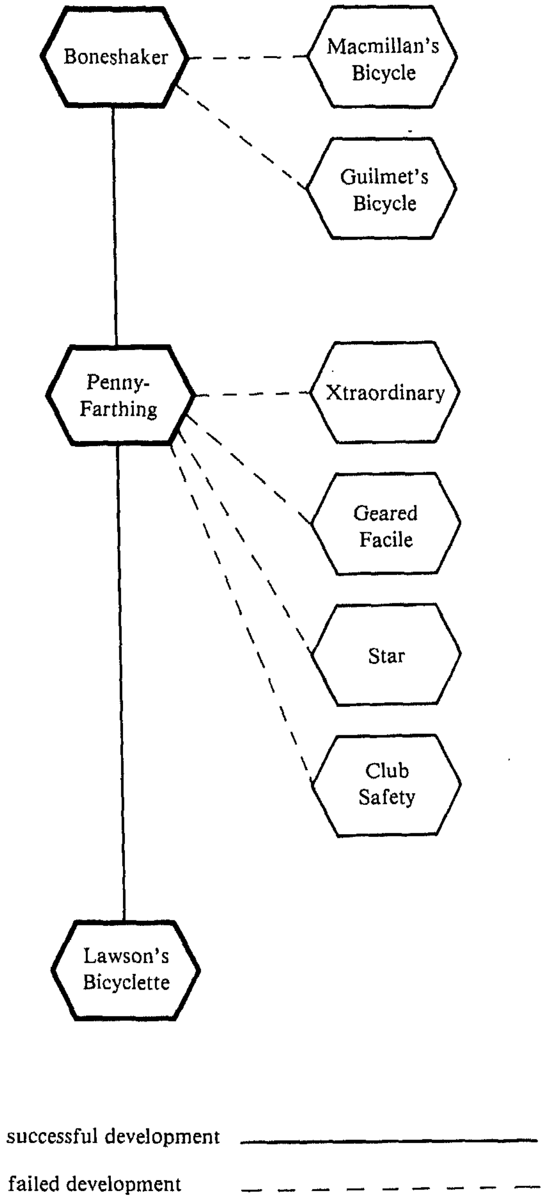


FIGURE 3
The Traditional Quasi-Linear View of the Developmental Process of the Penny-Farthing Bicycle



In deciding which problems are relevant, a crucial role is played by the social groups concerned with the artefact, and by the meanings which those groups give to the artefact: a problem is only defined as such, when there is a social group for which it constitutes a 'problem'.

The use of the concept 'relevant social group' is quite straightforward. The term is used to denote institutions and organizations (such as the military or some specific industrial company), as well as organized or unorganized groups of individuals. The key requirement is that all members of a certain social group share the same set of meanings, attached to a specific artefact.[68] In deciding which social groups are relevant, the first question is whether the artefact has any meaning at all for the members of the social group under investigation. Obviously, the social group of 'consumers' or 'users' of the artefact fulfils this requirement. But also less obvious social groups may need to be included. In the case of the bicycle, for example, one needs to mention the 'anti-cyclists'. Their actions ranged from derisive cheers to more destructive methods. For example, Rev L. Meadows White described such resistance to the bicycle in his book, *A Photographic Tour on Wheels*:

> ...but when to words are added deeds, and stones are thrown, sticks thrust into the wheels, or caps hurled into the machinery, the picture has a different aspect. All the above in certain districts are of common occurrence, and have all happened to me, especially when passing through a village just after school is closed.[69]

Clearly, for the anti-cyclists the artefact 'bicycle' had taken on meaning!

Another question we need to address, is whether a provisionally defined social group is homogeneous with respect to the meanings given to the artefact — or is it more effective to describe the developmental process by dividing a rather heterogeneous group into several different social groups? Thus, within the group of cycle-users, we discern a separate social group of women cyclists. During the days of the high-wheeled 'Ordinary', women were not supposed to mount a bicycle. For instance, in a magazine advice column (1885) it is proclaimed, in reply to a letter from a young lady:

> The mere fact of riding a bicycle is not in itself sinful, and if it is the only means of reaching the church on a Sunday, it may be excusable.[70]

Tricycles were the permitted machines for women. But engineers and producers anticipated the importance of women as potential bicyclists. In a review of the annual Stanley Exhibition of Cycles in 1890, the author observes:

> From the number of safeties adapted for the use of ladies, it seems as if bicycling was becoming popular with the weaker sex, and we are not surprised at it, considering the saving of power derived from the use of a machine having only one slack.[71]

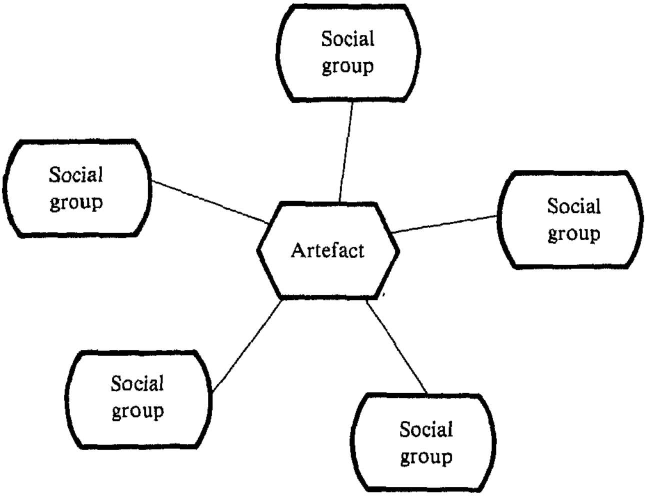
Thus some parts of the bicycle's development can be better explained by including a separate social group of feminine cycle-users. This need not, of course, be so in other cases: for instance, we do not expect it would be useful to consider a separate social group of women users of, say, fluorescent lamps.[72]

Once the relevant social groups have been identified, they are described in more detail. Although the only defining property is some homogeneous meaning given to a certain artefact, the intention is not just to retreat to worn-out, general statements about 'consumers' and 'producers'. We need to have a detailed description of the relevant social groups in order better to define the function of the artefact with respect to each group. Without this, one could not hope to be able to give any explanation of the developmental process. For example, the social group of cyclists riding the high-wheeled Ordinary consisted of 'young men of means and nerve: they might be professional men, clerks, schoolmasters or dons'.[73] For this social group, the function of the bicycle was primarily for sport. The following comment in the *Daily Telegraph* emphasizes sport, rather than transport:

> Bicycling is a healthy and manly pursuit with much to recommend it, and, unlike other foolish crazes, it has not died out.[74]

Let us now return to the exposition of the model. Having identified the relevant social groups for a certain artefact (Figure 4), we are especially interested in the problems each group has with respect to that artefact (Figure 5). Around each problem, several variants of solution can be identified (Figure 6). In the case of the bicycle, some relevant problems and solutions are shown in Figure 7, in which the grey area of Figure 2 has been filled. This way of describing the developmental process brings out clearly all kinds of

FIGURE 4
The Relationship between an Artefact and the Relevant Social Groups



conflicts: conflicting technical requirements by different social groups (for example, the 'speed' requirement and the 'safety' requirement); conflicting solutions to the same problem (for example, the Safety Low Wheelers and the Safety Ordinaries — this type of conflict often results in patent litigation); and moral conflicts (for example, women wearing skirts or trousers on a High Wheeler). Within this scheme, various solutions for these conflicts and problems are possible — not only technological, but also judicial, or even moral (for example, changing attitudes towards women wearing trousers).

Following the developmental process in this way, we see growing and diminishing degrees of stabilization[75] of the different artefacts. In principle, the degree of stabilization is different in different social groups. By using the concept of stabilization, the 'invention' of the Safety Bicycle is seen not as an isolated event (1884), but as a nineteen-year process (1879-98). For example, at the beginning of this period the relevant groups did not see the 'safety bicycle', but a wide range of bi- and tricycles — and, among those, a rather ugly crocodile-like bicycle with a relatively low front wheel and rear chain drive (Lawson's Bicyclette). By the end of the period, the word 'safety bicycle' denoted a low-wheeled bicycle with rear chain drive, diamond frame, and air tyres. As a result of the stabilization of the artefact after 1898, one did not need to specify these details: they were taken for granted as the essential 'ingredients' of the safety bicycle.

FIGURE 5
The Relationship between One Social Group and the Perceived Problems

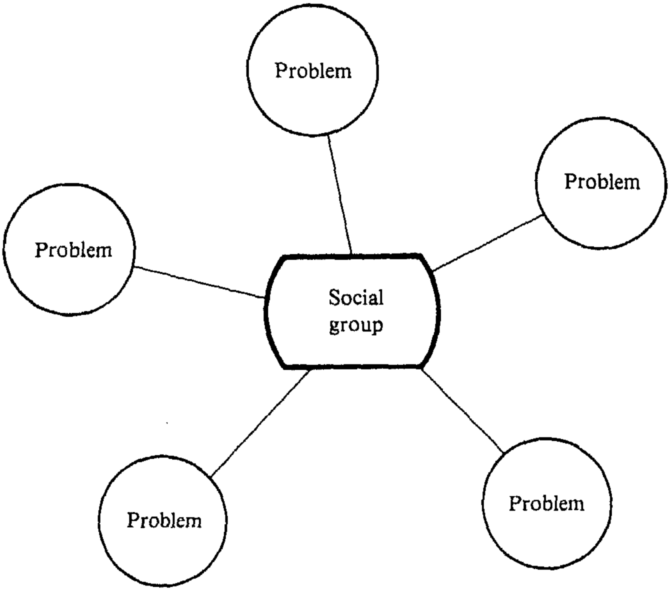


FIGURE 6
The Relationship between One Problem and its Possible Solutions

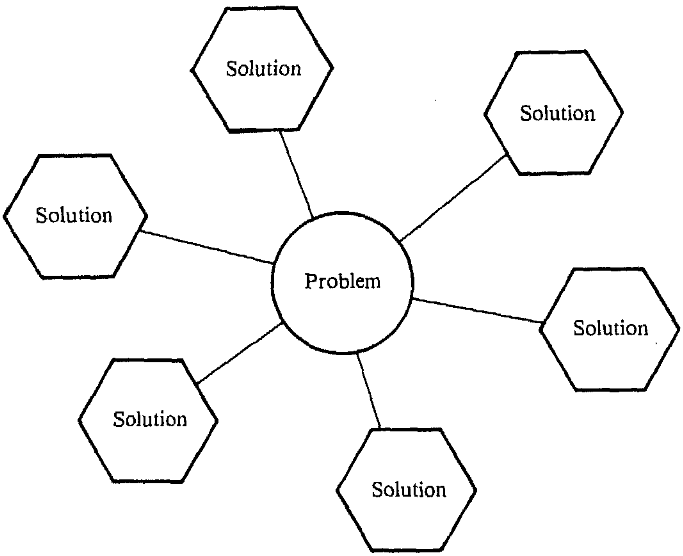
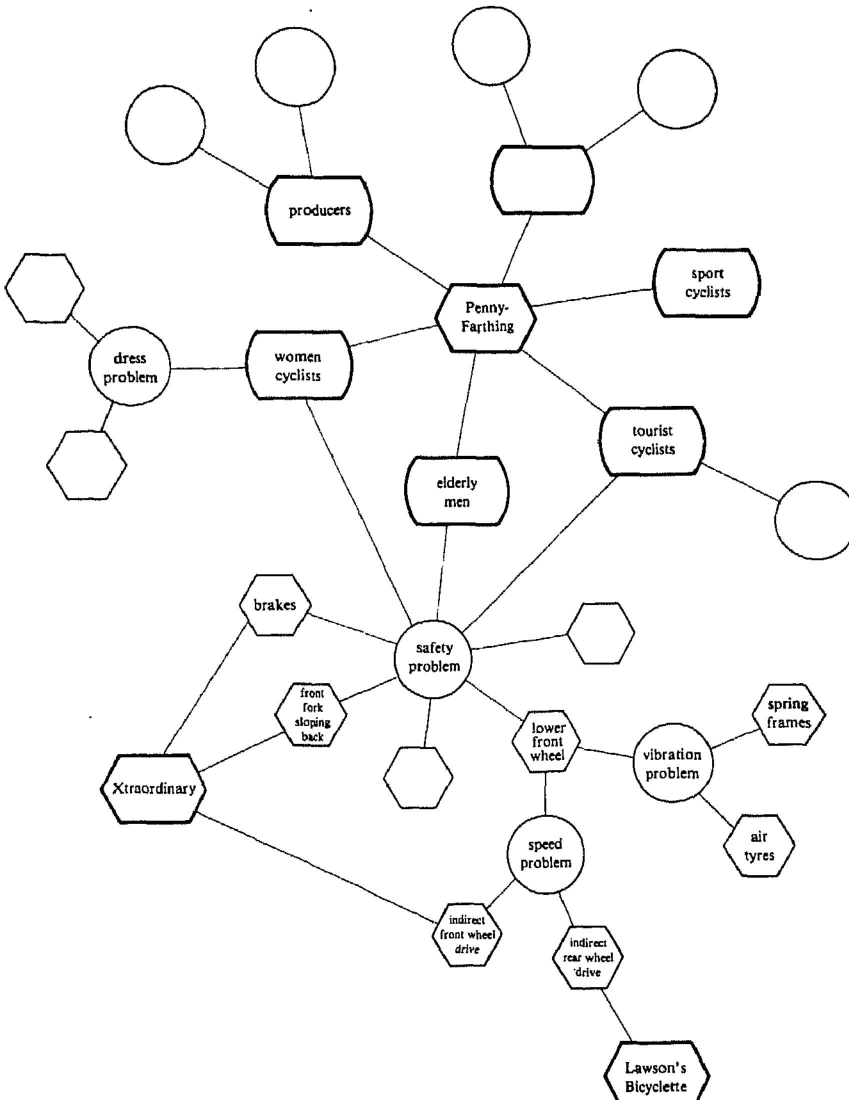


FIGURE 7
Some Relevant Social Groups, Problems and Solutions in the Developmental Process of the Penny-Farthing Bicycle



Note: because of lack of space, not all relevant social groups, problems and solutions are shown.

We want to stress that our model is not used as a mould, into which the empirical data have to be forced, *coûte que coûte*. The model has been developed from a series of case studies, and not from purely philosophical or theoretical analysis. Its function is primarily heuristic — to bring out all the aspects relevant for our purposes. This is not to say that there are no explanatory and theoretical aims, analogous to the different stages of the EPOR.[76] And indeed, as we have shown, this model already does more than merely describe technological development: it highlights its multi-directional character. Also, as will be indicated below, it brings out the interpretative flexibility of technological artefacts and the role which different closure mechanisms may play in the stabilization of artefacts.

The Social Construction of Facts and Artefacts

Having described the two approaches to the study of science and technology we wish to draw upon, we will now discuss in more detail the parallels between them. As a way of putting some flesh on our discussion we will, where appropriate, give empirical illustrations drawn from our own research.

The first stage of the EPOR involves the demonstration of the interpretative flexibility of scientific findings. To illustrate in more detail what we mean here, we will give a brief example.

Interpretative Flexibility — the Science Case

Our example is drawn from the solar-oscillation controversy. This controversy centred on measurements first made in 1975 by Henry Hill (a physicist at the University of Arizona, Tucson) which seemed to indicate that the Sun was oscillating at a number of different frequencies. Following Hill's report, several other groups of physicists and astronomers attempted to observe these oscillations, using a variety of techniques. By 1978, six groups had reported negative results at the frequencies claimed by Hill, thus casting doubt upon Hill's claims. The consensus amongst the solar physics community seems to be moving towards the view that the oscillations claimed by Hill do not exist.

To demonstrate the interpretative flexibility of scientific findings, the sociologist of science must show that differing interpretations of the natural world are available: in short, s/he must demonstrate that nature does not 'force the issue' of the existence or non-existence of some purported phenomenon, one way or the other. In this case, the purported phenomenon is the existence of solar oscillations. There is little difficulty in sustaining the interpretation that these oscillations do not exist, since most experimental results have not confirmed their existence. The difficulty, as always in such cases, is in recovering the plausibility of the rejected view that the oscillations *do* exist. Experimental evidence, in the shape of negative results, seems to be very compelling. The way that the plausibility of the rejected view can then be recovered has been demonstrated by Collins in his well-known study of the gravity wave episode.[77] In that case, an experimenter, Joseph Weber, was faced by several groups who failed to confirm his experimental claims to have detected large fluxes of gravitational radiation. By interviewing Weber and his critics, Collins was able to show that the negative results lacked compulsion because there was no agreement as to what counted as the 'same' experiment. It was possible to question whether the negative experiments had really been 'repeats' of Weber's original experiment. The thrust of the negative experiments could thus be diverted.

In the solar-oscillation case a similar methodology has been followed. Interpretative flexibility has been demonstrated by monitoring Hill's response to the negative results, which have been produced by different techniques to those used by Hill. To compare the differing techniques, some theoretical assumptions must be made about the physical processes occurring in the solar photosphere. Hill's response to the negative results has been to challenge the validity of these assumptions, and to claim that the different results actually indicate the poverty of the theoretical assumptions. He has thus been able to maintain that his oscillations are real but that, for various theoretical reasons, they do not show up in the other measurements. The following comment by Hill (in interview) illustrates the power of this approach towards criticism:

> My attitude is that Stanford is not wrong, Kitt Peak is not wrong, Eric Fossat is not wrong [these are all groups reporting negative experiments]. What happens is that there are certain measurements whose values are taken to be correct, and you take our numbers to be correct and ask what we can learn... I say look this is the

way science is supposed to be, you look at Nature and do observations and then I turn round and say what's wrong with the theory.

By suggesting that the theory was wrong, Hill was able to claim that all the experiments gave correct results, but that the results could not be straightforwardly compared. With new theoretical assumptions, he went on to show why these other techniques would not be sensitive to oscillations. Thus he was able to save the phenomenon. The critics' attitude was rather different. As one observer, who had failed to find oscillations, said:

> You should just take what we are observing, and using very straightforward assumptions..., you would have a big oscillation in our data but you don't.

For the critics, their negative results could straightforwardly be used to cast doubt upon Hill's measurements, implying that solar oscillations did not exist.

Of course, this example is merely illustrative, and to make a convincing case all the technical arguments over the reality of the oscillations, and the veracity of the various theoretical analyses, must be examined. However, we think this example will suffice for demonstrating how interpretative flexibility is to be shown in the science case.

Interpretative Flexibility — the Technology Case

In SCOT, the equivalent of the first stage of the EPOR would seem to be the demonstration that technological artefacts are culturally constructed and interpreted — in other words, the interpretative flexibility of a technological artefact must be shown. By this we mean, not only that there is flexibility in how people think of, or interpret, artefacts, but also that there is flexibility in how artefacts are *designed*. There is not just one possible way, or one best way, of designing an artefact. In principle this could be demonstrated in the same way as for the science case — that is, by interviews with technologists who are engaged in a contemporary technological controversy. For example, we can imagine that if interviews had been carried out in 1890 with the cycle engineers, we would have been able to show the interpretative flexibility of the artefact 'air tyre'. For some, this artefact was a solution to the vibration problem of small-wheeled vehicles:

> [The air tyre was] devised with a view to afford increased facilities for the passage of wheeled vehicles — chiefly of the lighter class such for instance as velocipedes, invalid chairs, ambulances — over roadways and paths, especially when these latter are of rough or uneven character.[78]

For others, the air tyre was a way of going faster (this will be outlined in more detail below). For yet another group of engineers, it was an ugly looking way of making the low-wheeler yet more unsafe (because of side-slipping) than it already was. For instance, the following comment describing The Stanley Exhibition of Cycles, is revealing:

> The most conspicuous innovation in the cycle construction is the use of pneumatic tires. These tires are hollow, about 2 in. diameter, and are inflated by the use of a small air pump. They are said to afford most luxurious riding, the roughest macadam and cobbles being reduced to the smoothest asphalte. Not having had the opportunity of testing these tires, we are unable to speak of them from practical experience; but looking at them from a theoretical point of view, we opine that considerable difficulty will be experienced in keeping the tires thoroughly inflated. Air under pressure is a troublesome thing to deal with. From the reports of those who have used these tires, it seems that they are prone to slip on muddy roads. If this is so, we fear their use on rear-driving safeties — which are all more or less addicted to side-slipping — is out of the question, as any improvement in this line should be to prevent side slip and not to increase it. Apart from these defects, the appearance of the tires destroys the symmetry and graceful appearance of a cycle, and this alone is, we think, sufficient to prevent their coming into general use.[79]

And indeed, other artefacts were seen as providing a solution for the vibration problem, as the following comment reveals:

> With the introduction of the rear-driving safety bicycle has arisen a demand for anti-vibration devices, as the small wheels of these machines are conducive to considerable vibration, even on the best roads. Nearly every exhibitor of this type of machine has some appliance to suppress vibration.[80]

Most solutions used various spring constructions in the frame, the saddle, and the steering-bar. In 1896, even after the safety bicycle (and the air tyre with it) achieved a high degree of stabilization, 'spring frames' were still being marketed.

It is important to realize that this demonstration of interpretative flexibility, by interviews and historical sources, is only one of a set of possible methods. At least in the study of technology, another method is applicable, and has actually been used. It can be shown that different social groups have radically different interpretations of one technological artefact. We call these differences 'radical' because the *content* of the artefact seems to be involved. It is something more than what Mulkay rightly claims to be rather easy — 'to show that the social meaning of television varies with and depends upon the social context in which it is employed'. As Mulkay notes, 'it is much more difficult to show what is to count as a "working television set" is similarly context-dependent in any significant respect'.[81]

We think that our account — in which the different interpretations by social groups of the content of artefacts lead via different chains of problems and solutions to different further developments — involves the content of the artefact itself. Our earlier example of the development of the safety bicycle is of this kind. Another example is variations within the high-wheeler. The high-wheeler's meaning as a virile, high-speed bicycle led to the development of larger front wheels — for, with a fixed angular velocity, the only way of getting a higher translational velocity over the ground was by enlarging the radius. One of the last bicycles resulting from this strand of development was the Rudge Ordinary of 1892, which had a 56-inch wheel and air tyre. But groups of women and of elderly men gave quite another meaning to the high-wheeler. For them, its most important characteristic was its lack of safety:

> Owing to the disparity in wheel diameters and the small weight of the backbone and trailing wheel, also to the rider's position practically over the centre of the wheel, if the large front wheel hit a brick or large stone on the road, and the rider was unprepared, the sudden check to the wheel usually threw him over the handlebar. For this reason the machine was regarded as dangerous, and however enthusiastic one may have been about the ordinary — and I was an enthusiastic rider of it once — there is no denying that it was only possible for comparatively young and athletic men.[82]

This meaning gave rise to lowering the front wheel, moving back the saddle, and giving the front fork a less upright position. Via another chain of problems and solutions (see Figure 7), this resulted in artefacts such as Lawson's Bicyclette (1879) and the Xtraordinary (1878). Thus, there was not *one* high-wheeler — there was the *macho* machine, leading to new designs of bicycles with even higher front wheels, and there was the *unsafe* machine, leading to new designs of bicycle with lower front wheels, saddles moved backwards, or reversed order of small and high wheel.[83] Thus the interpretative flexibility of the artefact 'Penny-farthing' is materialized in quite different design lines.

Closure and Stabilization

The second stage of the EPOR concerns the mapping of mechanisms for the closure of debate — or, in SCOT, for the stabilization of an artefact. We have already noted that the stabilization of an artefact is always a matter of degree: it seems that consensus in science can be described in similar terms. For example, Latour and Woolgar have shown that in the social construction of scientific facts modalities are attached or withdrawn from statements about facts in order to connote the degree of stabilization of the fact.[84] Thus the statements: 'The experiments claim to show the existence of X', 'The experiments show the existence of X', and 'X exists', not only exhibit progressively fewer modalities but also progressively greater degrees of stabilization of X.[85] In technological cases, we have also found varieties in the number of definitions, specifications, and elucidations attached to statements about the artefact. We can use this as a measure of the degree of stabilization which the artefact has achieved. However, there is a methodological problem in using language in this way. The need to add definitions and elucidations in order to be able to communicate about an artefact depends on more than just the degree of stabilization of that artefact in that social group; it will at least also depend on the context in which the statement is used (for example, a research paper, a patent, or a handbook).[86]

In considering the emergence of consensus and stabilization, one difference between science and technology arises. In the case of science, consensus formation can often be studied by focussing upon one group — the Core-Set — although the wider scientific community plays a specific role, especially in the case of a rhetorical closure (see below). But in the case of technology, it is typically necessary to analyze the stabilization of an artefact amongst more than one group, as we have shown above; and, since a variety of social groups must then be identified, it is impossible to carry out quite such neat case studies as can be achieved in science. This is partly a matter of strategy, for, in the study of science, it is sometimes necessary to follow more than one social group. For instance, in her study of a scientific laboratory, Knorr-Cetina argues that stabilization occurs amongst a variety of groups which she refers to as 'trans-scientific fields'.[87]

It seems that a variety of closure mechanisms play a part in bringing about both scientific agreement and the stabilization of an artefact. In some cases one particular mechanism may predominate; other cases may be resolved by other mechanisms or combinations of mechanisms. We will now illustrate what we mean by a closure mechanism by giving examples of two types which seem to have played a role in cases with which we are familiar. We refer to the particular mechanisms upon which we focus as 'rhetorical closure', and 'closure by redefinition of problem'. Firstly, we will discuss rhetorical closure in the science and technology cases. We will then go on to illustrate 'closure by redefinition of problem' for the technology case.

Rhetorical Closure — the Science Case

What we have in mind here is some 'crucial' experimental result, 'definitive' proof, or 'knockdown' argument which has the effect of closing the debate on some controversial issue. The character of such experimental results, proofs or arguments is that they are not convincing to the scientists most concerned with the debate — those scientists who form the Core-Set. Indeed, such scientists will usually be aware of the inadequacies of such 'crucial' experiments, 'definitive' proofs and 'knockdown' arguments, which usually have more influence in persuading the wider community than the Core-Set itself.

An example of an experiment which played this role was that carried out by one of the scientists involved in the gravity-wave dispute. This experiment (performed by 'Quest') did not have the requisite sensitivity to challenge Weber's results, but it played a significant role in their demise.[88] Another example might be the ritualistic recitation of von Neumann's hidden variable impossibility proof as a means of closing down the debate over David Bohm's 1952 hidden variable interpretation of quantum mechanics. This proof continued to be cited with little attempt being made to investigate its veracity.[89] A third example of rhetorical argument serving as a closure mechanism is documented by Pickering in his study of the reception of Price's magnetic monopole claim. A talk by Louis Alvarez which was full of rhetorical arguments (that is, not convincing to specialists) played an important role in the demise of the monopole claim amongst the high-energy physics community.[90]

To illustrate this type of mechanism further, we will turn to an example drawn from the solar-oblateness controversy.[91] This controversy began in 1967, and centred on Princeton physicist Robert Dicke's claims to have observed that the Sun was oblate — that is to say, he found the solar equatorial diameter to be larger than the solar polar diameter. This result was very important because it suggested an alternative explanation for the well-known anomaly in the perihelion advance of Mercury's orbit. This anomaly was widely believed to be explained by Einstein's general theory of relativity — and this was, indeed, one of the major successes claimed for Einstein's theory. However, a quadrupole moment from an oblate Sun (assuming the oblateness was produced by a rapidly rotating core) could account for enough of Mercury's perihelion to suggest that rival theories of gravity, such as the Brans-Dicke scalar-tensor theory, might provide the correct explanation.

When Dicke's oblateness results were published they immediately produced a storm of controversy. By 1974, eighteen major theoretical papers had been published challenging the results. Soon an experimental challenge also emerged. A new measurement of the oblateness was carried out by Henry Hill (who was also involved in the solar oscillation controversy). Hill's measurements contradicted Dicke's and furthermore suggested that Einstein's theory was largely correct. Hill's results received wide attention in the scientific press and in effect closed down the controversy. Whenever the oblateness issue threatened to raise its head again, Hill's result was ritually cited as having shown that Dicke was wrong. As is typical with rhetorical closure, none of the possible interpretative loopholes in Hill's experiment were explored (in contrast to his work on solar oscillations), and Hill's results were of more significance to the whole community than to the Core-Set.

Rhetorical Closure — the Technology Case

Closure in technology involves the stabilization of an artefact and the 'disappearance' of problems. To close a technological 'controversy' the problems need not be *solved* in the common sense of that word. The key point is whether the relevant social groups *see* the problem as being solved. In technology, advertising can play an important role in shaping the meaning which a social group gives to an artefact.[92] Thus, for instance, an attempt was made to 'close' the 'safety controversy' around the high- wheeler by simply claiming that the artefact was perfectly safe. An advertisement of the 'Facile' Bicycle (sic!) reads:

> Bicyclists! Why risk your limbs and lives on high Machines when for road work a 40 inch or 42 inch 'Facile' gives all the advantages of the other, together with almost absolute safety.[93]

This claim of 'almost absolute safety' was a rhetorical move, considering the height of the bicycle and the forward position of the rider, which were well known to engineers at the time to present problems of safety.

Closure by Redefinition of Problem — the Technology Case

We have already mentioned the controversy around the air tyre: for most of the engineers it was a theoretical and practical monstrosity. For the general public, in the beginning it meant an aesthetically awful accessory:

> . . . messenger boys guffawed at the sausage tyre, factory ladies squirmed with merriment, while even sober citizens were sadly moved to mirth at a comicality obviously designed solely to lighten the gloom of their daily routine.[94]

For Dunlop and the other protagonists of the air tyre, originally it meant a solution to the vibration problem. However, the group of sporting cyclists riding their high-wheelers did not accept that to be a problem at all. Vibration only presented a problem to the (potential) users of the low-wheeled bicycle. Three important social groups were therefore opposed to the air tyre. But then the air tyre was mounted on a racing bicycle. When, for the first time, the tyre was used at the racing track, its entry was hailed with derisive laughter. This was, however, quickly silenced by the high speed achieved, and there was only astonishment left when it outpaced all rivals.[95] Very soon handicappers had to give racing cyclists on high-wheelers a considerable start if riders on air-tyred low-wheelers were entered. After a short period no racer of any pretensions troubled to compete on anything else.[96]

What had happened? With respect to two important groups, the sporting cyclists and the general public, closure had been reached — but not by convincing those two groups of the feasibility of the air tyre in its meaning as an antivibration device. One can say, we think, that the meaning of the air tyre was translated[97] to constitute a solution to quite another problem: the problem of 'how to go as fast as possible'. And thus, by redefining the key problem with respect to which the artefact should have the meaning of a solution, closure was reached for two of the relevant social groups. How the third group, the engineers, came to accept the air tyre is another story, and need not be told here. Of course, there is nothing 'natural' or logically necessary about this form of closure. It could be argued that speed is not the most important characteristic of the bicycle, or that existing cycle races were not appropriate tests of a cycle's 'real' speed (after all, the idealized world of the race track may not match everyday road conditions, anymore than the formula-one racing car bears upon the performance requirements of the average family saloon). Still, bicycle races have played an important role in the development of the bicycle, and since racing can be viewed as a specific form of testing, this observation is very much in line with Constant's recent plea to pay more attention to testing procedures in studying technology.[98]

The Wider Context

Finally, we come to the third stage of our research programme. The task here in the area of technology would seem to be the same as for science — to relate the content of a technological artefact to the wider sociopolitical milieu. This aspect has not yet been demonstrated for the science case,[99] at least not in contemporaneous sociological studies.[100] However, the SCOT method of describing technological artefacts by focussing upon the meanings given to them by relevant social groups seems to suggest a way forward. Obviously, the sociocultural and political situation of a social group shapes its norms and values, which in turn influence the meaning given to an artefact. Since we have shown above how different meanings can constitute different lines of development, SCOT's descriptive model seems to offer an operationalization of the relationship between the wider milieu and the actual content of technology.[101]

Concluding Summary and Implications for Further Research

In this paper we have been concerned to outline an integrated social constructivist approach towards the empirical study of science and technology. We first reviewed several relevant bodies of literature and strands of argument. We indicated that the social constructivist approach is a flourishing tradition within the sociology of science, and that it shows every promise of wider application. We reviewed the literature on the science-technology relationship, and showed that here, too, the social constructivist approach is starting to bear fruit. And we reviewed some of the main traditions in technology studies. We argued that innovation studies, and much of the history of technology, were unsuitable for our sociological purposes. We discussed some recent work in the sociology of technology, and noted encouraging signs that a 'new wave' of social constructivist case studies was beginning to emerge.

We then outlined in more detail the two approaches — one in the sociology of scientific knowledge (EPOR), and one in the field of sociology of technology (SCOT) — upon which we base our integrated perspective. Finally, we indicated the similarity of the explanatory goals of the two approaches, and illustrated these goals with some examples drawn from science and technology.

Despite the imbalance between the two approaches — an imbalance largely due to the underdeveloped state of the sociology of technology — a number of important points of comparison have already emerged. Particularly, we have seen that the concepts of 'interpretative flexibility' and 'closure mechanism', and the notion of 'social group', can be given empirical reference in both the science and technology cases. A number of remarks stemming from our comparison are, however, appropriate.[102]

We have already indicated that the way in which interpretative flexibility has been demonstrated in the technology case, with the use of the descriptive model, is one of several possible methods. It would be fruitful to apply the 'controversy method', which has proved its success in the science case, and carry out a study of a contemporary controversy in technology.

More systematic studies of closure mechanisms in science and technology are also needed. We have looked at only two types of closure mechanism: others must be researched. It is possible that the type of closure mechanism predominant in science and the type predominant in technology are different. For example, we were not able to find a close analogue of 'closure by redefinition of problem' in the solar physics material. However, it would seem that phenomena such as 'ignoring a problem' or 'moving on', which are encountered in other areas of science, are related to 'closure by redefinition'. It can be speculated that the differences in types of closure mechanisms are linked to differences in the number of relevant groups involved in the developmental process. In science (at least in those parts of physics and biology which have so far been researched) it does seem to be the case that the social construction of scientific knowledge can be followed through by monitoring the activities of one dominant social group — the Core-Set. In technology it seems that there is no equivalent group, and that a number of social groups must be studied. Perhaps with more groups involved, 'redefinition of problem' is likely to be a more effective closure mechanism in technology than in science. The study of technology should thus give rise to a better analysis of closure mechanisms. This, in turn, should lead to a more systematic study of the functioning of various social groups, in science as well as technology. For instance, the role which rhetoric plays in the relation between the Core-Set and the wider scientific community seems to be analogous to the role played by advertising in the relation between the social group of producers and various groups of (potential) users.

The difference in the number of relevant social groups perhaps sheds some light on the different paths which sociology of science and technology studies have taken. The large number of social groups relevant to the technology case does present problems for the micro-study approach which has proved to be so fruitful in the sociology of scientific knowledge. As there does not appear to be any one key social group in technology, researchers may have been reluctant to look in too fine detail, afraid to miss key developments associated with unresearched social groups. The solution to this problem does not seem to us to be to go to the opposite extreme and use techniques of aggregation, because one then runs into the difficulty (which we have already noted) of ignoring the technological content of the artefact. We believe that the approach we have outlined — of identifying the relevant social groups — does provide a means of researching technology, but in such a way as not to lose sensitivity to the content of the artefact; in that way, it enables us to sail between the Scylla of the isolated artefact and the Charybdis of a mass aggregation.[103]

An alternative location for micro-studies of science — the ethnographic study of the scientific laboratory — has recently been advocated. Such a location has proved to be particularly useful for showing the interpretative flexibility of scientific knowledge.[104] However, the laboratory location is a rather poor place in which to study the formation of scientific consensus. This is because the processes of consensus formation are not usually to be found in a single laboratory. Unless one is prepared to use other data than purely ethnographic sources, it is difficult to study processes of consensus formation in individual laboratories.[105] The problem is even worse in the case of the study of the stabilization of technological artefacts. This is because there is *an even larger number of social groups* to study, and one is likely to obtain even less relevant data from the individual laboratory. It is important that an ethnographic study — say of an R & D lab — be carried out, but such a study will, it seems, be more useful for showing the interpretative flexibility of technological artefacts than for the study of closure mechanisms.

Studies of the influence of the wider cultural and social milieu are still few and far between, in both science and technology. The notion of 'relevant social group', as it is employed in SCOT, may, as we have indicated, provide a fruitful approach. Such studies are the most daunting to carry out, but we can only hope that they will be forthcoming, as they are an integral part of our social constructivist programme.

As we have noted throughout this paper, the sociology of technology is still underdeveloped, in comparison with the sociology of scientific knowledge. It would be a shame if the advances made in the latter field could not be used to throw light on the study of technology. On the other hand, in our studies of technology it appeared to be fruitful to include several social groups in the analysis, and there are some indications that this method may also bear fruit in studies of science. Thus our integrated approach to the social study of science and technology indicates how the sociology of science and the sociology of technology might benefit each other.

But there is another reason, and perhaps an even more important one, to argue for such an integrated approach. And this brings us to a question which some readers might have expected to be dealt with in the very first paragraph of this article — namely, the question of how to demarcate between science and technology. We think that it is rather unfruitful to make such an a priori distinction, using elaborate definitions. Instead, it seems worth while to start with commonsensical notions of science and technology, and to study them in an integrated way, as we have proposed. Whatever interesting differences may exist, will gain contrast within such a programme. This would constitute another concrete result of the integrated study of the social construction of facts and artefacts.

• *NOTES*

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1. The science-technology divorce seems to have resulted not so much from the lack of overall analytical goals within 'science studies', but more from the contingent demands of carrying out empirical work in these areas. To give an example: the new sociology of scientific knowledge, which attempts to take account of the actual content of scientific knowledge, can best be carried out by researchers who have some training in the science they study, or at least who are familiar with an extensive body of technical literature (indeed, many researchers are ex-natural scientists). Once having gained such expertise, there is a tendency to stay within the domain where that expertise can best be deployed. Similarly, R & D studies and innovation studies, in which the analysis centres on the 'firm' and 'market place', have tended to demand the specialist competences of economists. Such disparate bodies of work do not easily lead towards a more integrated conception of science and technology. One notable exception is J. R. Ravetz, *Scientific Knowledge and its Social Problems* (Oxford: Oxford University Press, 1971). This is one of the few works of recent science studies in which both science and technology and their differences are explored within a common framework.

2. The studies of science have been carried out by Pinch. The examples we draw upon in this paper come from his recent comparative study of four episodes of scientific controversy. For some of the provisional findings of this study, see T. J. Pinch, 'Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observation Reports in Physics', *Social Studies of Science*, forthcoming. In this paper we will use examples connected with the work of one scientist — Henry Hill.

The studies of technology have been carried out by Bijker. In all, six technological innovations were studied. See W. E. Bijker, J. Bönig and E. C. J. van Oost, 'The Social Construction of Technological Artefacts', paper presented to the EASST Conference (Deutschlandsberg, Austria: 24-26 September 1982). In this paper we mostly use examples drawn from the case-study of the bicycle.

3. H. M. Collins, 'Stages in the Empirical Programme of Relativism', *Social Studies of Science*, Vol. 11 (1981), 3-10.

4. Bijker et al., op. cit. note 2.

5. A comprehensive review can be found in M. J. Mulkay and V. Milič, 'The Sociology of Science in East and West', *Current Sociology*, Vol. 28 (Winter 1980), 1-342.

6. For a recent review of the sociology of scientific knowledge, see H. M. Collins 'The Sociology of Scientific Knowledge: Studies of Contemporary Science', *Annual Review of Sociology*, Vol. 9 (1983), 265-85.

7. For discussion of the earlier work (largely associated with Robert Merton and his students), see R. D. Whitley, 'Black Boxism and the Sociology of Science: A Discussion of the Major Developments in the Field', in P. Halmos (ed.), *The Sociology of Science, Sociological Review Monograph* No. 18 (Keele: University of Keele, 1972), 61-92.

8. D. Bloor, 'Wittgenstein and Mannheim on the Sociology of Mathematics', *Studies in History and Philosophy of Science*, Vol. 4 (1973), 173-91.

9. For more discussion, see B. Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge and Kegan Paul, 1974); M. Mulkay, *Science and the Sociology of Knowledge* (London: Allen and Unwin, 1979); Collins, op. cit. note 6; and B. Barnes and D. Edge (eds), *Science in Context* (Milton Keynes, Bucks.: The Open University Press, 1982). The origins of this approach can be found in L. Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv* (Basel: Benno Schwabe & Co., 1935; Frankfurt am Main: Suhrkamp, 1980); English edition: L. Fleck, *The Genesis and Development of a Scientific Fact* (Chicago: The University of Chicago Press, 1979).

10. See, for example, B. Latour and S. Woolgar, *Laboratory Life* (London and Beverly Hills, Calif.: Sage, 1979); K. D. Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford: Pergamon, 1981); M. Lynch, *Art and Artefact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* (London: Routledge and Kegan Paul, forthcoming, 1984); and S. Woolgar, 'Laboratory Studies: A Comment on the State of the Art', *Social Studies of Science*, Vol. 12 (1982), 481-98.

11. See, for example, H. M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, Or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205-24; B. Wynne, 'C. G. Barkla and the J Phenomenon: A Case Study of the Treatment of Deviance in Physics', *Social Studies of Science*, Vol. 6 (1976), 307-47; T. J. Pinch, 'What Does a Proof Do if it Does not Prove? A Study of the Social Conditions and Metaphysical Divisions leading to David Bohm and John von Neumann Failing to Communicate in Quantum Physics', in E. Mendelsohn, P. Weingart and R. Whitley (eds), *The Social Production of Scientific Knowledge* (Dordrecht and Boston, Mass.: Reidel, 1977), 171-215; and the studies by A. Pickering, B. Harvey, H. M. Collins, G. D. L. Travis and T. J. Pinch collected together in Collins (ed.), *Knowledge and Controversy, Social Studies of Science*, Vol. 11 (1981), 3-158.

12. H. M. Collins and T. J. Pinch, 'The Construction of the Paranormal: Nothing Unscientific is Happening', in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge, Sociological Review Monograph* No. 27 (Keele: University of Keele, 1979), 237-70; and Collins and Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge and Kegan Paul, 1982).

13. D. Robbins and R. Johnston, 'The Role of Cognitive and Occupational Differentiation in Scientific Controversies', *Social Studies of Science*, Vol. 6 (1976), 349-68. For a similar analysis of public science controversies, see B. Gillespie, D. Eva and R. Johnston, 'Carcinogenic Risk Assessment in The United States and Great Britain: The Case of Aldrin/Dieldrin', *Social Studies of Science*, Vol. 9 (1979), 265-301; and F. B. McCrea and G. E. Markle, 'The Estrogen Replacement Controversy in the USA and UK: Different Answers to the Same Question?', *Social Studies of Science*, Vol. 14 (1984), 1-26.

14. Some of the most recent debates can be found in K. D. Knorr-Cetina and M. J. Mulkay (eds), *Science Observed — Perspectives on the Social Study of Science* (London and Beverly Hills, Calif.: Sage, 1983).

15. As, for instance, argued by Barnes, op. cit. note 9, and Collins and Pinch, op. cit. note 12.

16. S. Shapin, 'History of Science and its Sociological Reconstructions', *History of Science*, Vol. 20 (1982), 157-211.

17. T. Nickles, 'ERISS and International Sociology of Science', presented to the Sixth Annual Meeting of the Society for Social Studies of Science (Atlanta: 5-7 November 1981), and Nickles, 'How Discovery is Important to Cognitive Studies of Science', paper presented to the Philosophy of Science Association Meeting (Philadelphia: 30 October 1982).

18. F. Healey, 'The Research Funding Organization As a Focus for Science Studies', paper presented to the Science Studies Conference (Oxford: 27-28 September 1982), and H. M. Collins, 'Scientific Knowledge and Science Policy: Some Foreseeable Implications', *EASST Newsletter*, Vol. 2 (November 1983), 5-8.

19. For a recent review of some of this literature, see Ron Johnston, 'Controlling Technology: An Issue for the Social Studies of Science', *Social Studies of Science*, Vol. 14 (1984), 97-112.

20. The *locus classicus* is the study by B. Hessen, 'The Social and Economic Roots of Newton's Principia', in N. I. Bukharin, A. F. Joffe, M. Rubinstein, B. Zavadovsky, E. Colman, N. I. Vavilov, W. Th. Mitkewich and B. Hessen, *Science at the Crossroads* (London: Frank Cass, 1931), 147-212.

21. C. W. Sherwin and R. S. Isenson, *First Interim Report on Project Hindsight: Summary* (Washington, DC: Office of the Director of Defense Research and Engineering, 1966); Sherwin and Isenson, 'Project Hindsight: A Defense Department Study of the Utility of Research', *Science*, Vol. 156 (23 June 1967), 1571-77.

22. J. Langrish, M. Gibbons, W. G. Evans and F. R. Jevons, *Wealth from Knowledge* (London: Macmillan, 1972).

23. Illinois Institute of Technology, *Technology in Retrospect and Critical Events in Science (TRACES)* (Chicago: IIT Research Institute, 1968).

24. See K. Kreilkamp, '*Hindsight* and the Real World of Science Policy', *Science Studies*, Vol. 1 (1971), 43-66, and D. C. Mowery and N. Rosenberg, 'The Influence of Market Demand upon Innovation: A Critical Review of Some Recent Empirical Studies', *Research Policy*, Vol. 8 (1979), 103-53.

25. See, for example, Derek J. deSolla Price, 'The Structure of Publication in Science and Technology', in W. H. Gruber and D. G. Marquis (eds), *Factors in the Transfer of Technology* (Cambridge, Mass.: MIT Press, 1969), 91-104; F. R. Jevons, 'The Interaction of Science and Technology Today, or, is Science the Mother of Invention?', *Technology and Culture*, Vol. 17 (1976), 729-42; and O. Mayr, 'The Science-Technology Relationship as a Historiographic Problem', *Technology and Culture*, Vol. 17 (1976), 663-73.

26. B. Barnes, 'The Science-Technology Relationship: A Model and a Query', *Social Studies of Science*, Vol. 12 (1982), 166-72.

27. E. Layton, 'Conditions of Technological Development', in I. Spiegel-Rösing and D. J. deSolla Price (eds), *Science, Technology, and Society* (London and Beverly Hills, Calif.: Sage, 1977), 210.

28. Mayr, op. cit. note 25.

29. Layton, op. cit. note 27, 209.

30. Barnes, op. cit. note 26, 166.

31. See Whitley, op. cit. note 7.

32. Layton, op. cit. note 27, 198.

33. See, for example, N. Rosenberg, *Inside the Black Box: Technology and Economics* (Cambridge: Cambridge University Press, 1982).

34. Adapted from L. Uhlmann, *Der Innovationsprozess in westeuropäischen Industrieländern. Band 2: Den Ablauf industriellen Innovationsprozesse* (Berlin and München: Duncker and Humblot, 1978), 45.

35. See, for example, C. Freeman, *The Economics of Industrial Innovation* (Harmondsworth, Middx: Penguin, 1974), and Freeman, 'Economics of Research and Development', in Spiegel-Rösing and deSolla Price (eds), op. cit. note 27, 223-75.

36. John M. Staudenmaier, SJ, 'What SHOT Hath Wrought and what SHOT Hath Not: Reflections on 25 Years of the History of Technology', paper presented to the 25th Annual Meeting of SHOT, 1983. See, for a more extensive account, Staudenmaier, *Technology's Storytellers: Recovering the Human Fabric* (Cambridge, Mass.: MIT Press, forthcoming), based on his *Design and Ambience: Historians and Technology, 1958-1977* (unpublished PhD thesis, University of Pennsylvania, Philadelphia, 1980).

37. Shapin writes that 'A proper perspective of the uses of science might reveal that sociology of knowledge and the history of technology have more in common than is usually thought': S. Shapin, 'Social Uses of Science', in G. S. Rousseau and R. Porter (eds), *The Ferment of Knowledge* (Cambridge: Cambridge University Press, 1980), 93-139, at 132. Whilst we are sympathetic to Shapin's argument we think the time is now ripe for asking more searching questions of historical studies.

38. Staudenmaier, op. cit. note 36.

39. Eugene Ferguson, 'Toward a Discipline of the History of Technology', *Technology and Culture*, Vol. 15 (1974), 13-30, 19.

40. M. Kaufman, *The First Century of Plastics; Celluloid and its Sequel* (London: Plastics Institute, 1963), 61.

41. L. H. Baekeland, 'The Synthesis, Constitution, and Uses of Bakelite', *Industrial and Engineering Chemistry*, Vol. 1 (1909), 149-61; and Baekeland, 'On Soluble, Fusible, Resinous Condensation Products of Phenols and Formaldehyde', *Journal of Industrial and Engineering Chemistry*, Vol. 1 (1909), 545-49.

42. Manuals describing resinous materials do mention Bakelite, but not with the amount of attention which, retrospectively, we would think to be justified. Professor

Max Bottler, for example, devotes only one page to Bakelite in his 228-page book on resins and the resin industry: Bottler, *Harze und Harzindustrie* (Leipzig: Max Jänecke, 1924). Even when Bottler concentrates, in another book, on the *synthetic* resinous materials, Bakelite does not receive an indisputable 'first place'. Only half of the book is devoted to phenol/formaldehyde condensation products, and roughly half of the latter is devoted to Bakelite: Bottler, *Uber Herstellung und Eigenschaften von Kunstharzen und deren Verwendung in der Lack- und Firnisindustrie und zu elektrotechnischen und industriellen Zwecken* (München: J. F. Lehmanns Verlag, 1919). See also A. R. Matthis, *Insulating Varnishes in Electrotechnics* (London: John Heywood, approximately 1920).

43. W. Haynes, *American Chemical Industry*, Vol. 2 (New York: Van Nostrand, 1954), esp. 137-38.

44. Staudenmaier, op. cit. note 36. See also Thomas P. Hughes, 'Emerging Themes in the History of Technology', *Technology and Culture*, Vol. 20 (1979), 697-711.

45. See, for example, Edward W. Constant, II, *The Origins of the Turbojet Revolution* (Baltimore, Md.: Johns Hopkins University Press, 1980); Thomas P. Hughes, *Networks of Power: Electrification in Western Society: 1880-1930* (Baltimore, Md.: Johns Hopkins University Press, 1983); and John F. Hanieski, 'The Airplane as an Economic Variable: Aspects of Technological Change in Aeronautics, 1903-1955', *Technology and Culture*, Vol. 14 (1973), 535-52.

46. See, for example, David F. Noble, 'Social Choice in Machine Design: The Case of Automatically Controlled Machine Tools', in A. Zimbalist (ed.), *Case Studies on the Labor Process* (New York: Monthly Review Press, 1979), 18-50; Merritt Roe Smith, *Harpers Ferry Armory and the New Technology: The Challenge of Change* (Ithaca, NY and London: Cornell University Press, 1977); and W. Lazonick, 'Industrial Relations and Technical Change: the Case of the Self-Acting Mule', *Cambridge Journal of Economics*, Vol. 3 (1979), 231-62.

47. There is an American tradition in the sociology of technology. See, for example, S. G. Gilfillan, *The Sociology of Invention* (Cambridge, Mass.: MIT Press, 1935); W. F. Ogburn, *The Social Effects of Aviation* (Boston, Mass.: Houghton Mifflin, 1945); Ogburn and F. Meyers Nimkoff, *Technology and the Changing Family* (Boston, Mass.: Houghton Mifflin, 1955). See also R. Westrum, 'What Happened to the Old Sociology of Technology?', paper presented to the Eighth Annual Meeting of the Society for Social Studies of Science (Blacksburg, Virginia: 3-6 November 1983). A fairly comprehensive view of the present state of the art in German sociology of technology can be obtained from R. Jokisch (ed.), *Techniksoziologie* (Frankfurt: Suhrkamp, 1982). Several studies in the sociology of technology which attempt to break with the traditional approach can be found in W. Krohn, E. T. Layton and P. Weingart (eds), *The Dynamics of Science and Technology, Sociology of the Sciences Yearbook*, Vol. 2 (Dordrecht and Boston, Mass.: Reidel, 1978).

48. R. Johnston, 'The Internal Structure of Technology', in Halmos (ed.), op. cit. note 7, 117-30.

49. G. Dosi, 'Technological Paradigms and Technological Trajectories: A Suggested Interpretation of the Determinants and Directions of Technical Change', *Research Policy*, Vol. 11 (1982), 147-62. Dosi uses the concept of 'technological trajectory', developed by R. R. Nelson and S. G. Winter, 'In Search of a Useful Theory of Innovation', *Research Policy*, Vol. 6 (1977), 36-76.

50. Other approaches to technology based on Kuhn's idea of the community structure of science can be found in Constant, op. cit. note 45, and P. Weingart, 'Strukturen technologischen Wandels. Zu einer soziologische Analyse der Technik', in Jokisch (ed.), op. cit. note 47, 112-41.

51. One is reminded of the first blush of Kuhnian studies in the sociology of science. It was hoped that Kuhn's paradigm concept might be straightforwardly employed by sociologists in their studies of science. Indeed there were a number of studies in which attempts were made to identify phases of science, such as preparadigmatic, normal and revolutionary. It soon became apparent, however, that Kuhn's terms were loosely formulated, could be subject to a variety of interpretations and did not lend themselves to operationalization in any straightforward manner. It has also been argued that this description of science, especially when paradigms are interpreted as a form of sociometric network or 'invisible college', is not particularly radical. See, especially, T. J. Pinch, 'Kuhn — The Conservative and Radical Interpretations: Are some Mertonians "Kuhnians" and some Kuhnians "Mertonians"?', *4S Newsletter*, Vol. 7, No. 1 (1982), 10-25.

52. See, for example, the inconclusive discussion over whether a Kuhnian analysis applies to psychology in D. S. Palermo, 'Is a Scientific Revolution Taking Place in Psychology?', *Science Studies*, Vol. 3 (1973), 211-44. A notable exception is Barnes's recent contribution to the discussion of Kuhn's work: B. Barnes, *T. S. Kuhn and Social Science* (London: Macmillan, 1982).

53. M. J. Mulkay, 'Knowledge and Utility: Implications for the Sociology of Knowledge', *Social Studies of Science*, Vol. 9 (1979), 63-80.

54. M. Bunge, 'Technology as Applied Science', *Technology and Culture*, Vol. 7 (1966), 329-47.

55. Mulkay, op. cit. note 53, 77.

56. M. Callon, 'The State and Technical Innovation: A Case Study of the Electrical Vehicle in France', *Research Policy*, Vol. 9 (1980), 358-76. The bulk of the empirical material on the development of the electric vehicle has not yet been published. In a series of articles Callon uses examples from his study to illustrate his approach towards technology: see Callon, 'Struggles and Negotiations to Define what is Problematic and what is Not — the Socio-Logic of Translation', in K. D. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook*, Vol. 4 (Dordrecht and Boston, Mass.: Reidel, 1980), 197-219; and Callon, 'Pour une sociologie des controverses technologiques', *Fundamenta Scientiae*, Vol. 2 (1981), 381-99.

57. Noble, op. cit. note 40.

58. For a useful review of Marxist work in this area, see D. Mackenzie, 'Marx and the Machine', *Technology and Culture*, in press.

59. Lazonick, op. cit. note 46.

60. For a provisional report of this study, see Bijker et al., op. cit. note 2. The word 'artefact' denotes material products as well as production processes. The six artefacts which were studied are: aluminium, Bakelite, fluorescent lamp, safety bicycle, Sulzer loom and transistor.

61. Work which might be classed as falling within the EPOR has been carried out primarily at the Science Studies Centre, University of Bath, by Collins, Pinch and Travis, and at the Science Studies Unit, University of Edinburgh, by Harvey and Pickering. See, for example, the studies collected together in Collins (ed.), op. cit. note 11.

62. Collins, op. cit. notes 3, 7.

63. H. M. Collins, 'The Place of the Core-Set in Modern Science: Social Contingency with Methodological Propriety in Science', *History of Science*, Vol. 19 (1981), 6-19.

64. For the purposes of the third stage, the notion of Core-Set may be too limited.

65. See Constant, op. cit. note 45, for a similar evolutionary approach to the description of technological development. Both Constant's and our model seem to arise out of work in evolutionary epistemology: see, for example, S. Toulmin, *Human Understanding*, Vol. 1 (Oxford: Oxford University Press, 1972), and D. T. Campbell, 'Evolutionary Epistemology', in P. A. Schlipp (ed.), *The Philosophy of Karl Popper, The Library of Living Philosophers*, Vol. 14-I (La Salle, Ill.: Open Court, 1974), 413-63. For a review of evolutionary models of technical change, see Jon Elster, *Explaining Technical Change* (Cambridge: Cambridge University Press, 1983).

66. It may be useful to state explicitly that we consider bicycles to be as fully-fledged a technology as, for example, automobiles or aircraft. It may be helpful for readers from outside notorious cycle countries such as The Netherlands, France and Britain, to point out that both the automobile and the aircraft industries are, in a way, descendants from the bicycle industry. Many names occur both in the histories of the bicycle and in the histories of the autocar: Triumph, Rover, Humber, Raleigh, to mention but a few. The Wright brothers both sold and manufactured bicycles before they started to build their flying machines — mostly made out of bicycle parts. See C. F. Caunter, *The History and Development of Light Cars* (London: HMSO, 1957); Caunter, *The History and Development of Cycles (as illustrated by the collection of cycles in the Science Museum); Part I: Historical Survey* (London: HMSO, 1955); C. H. Gibbs-Smith, *The Aeroplane: An Historical Survey of its Origins and Development* (London: HMSO, 1960).

67. J. Woodforde, *The Story of the Bicycle* (London: Routledge and Kegan Paul, 1970).

68. There is no cookbook recipe for how to identify a social group. Quantitative instruments using citation data may be of some help in certain cases. More research is needed to develop operationalizations of the notion of 'relevant social group' for a variety of historical and sociological research sites.

69. Meadows, cited in Woodforde, op. cit. note 67, 49-50.

70. Cited in Woodforde, op. cit. note 67, 122.

71. 'The Stanley Exhibition of Cycles', *The Engineer*, Vol. 69 (7 February 1890), 107-08.

72. This is one of the technological artefacts mentioned in Bijker et al., op. cit. note 2.

73. Woodforde, op. cit. note 67, 47.

74. *Daily Telegraph* (7 September 1877), cited in Woodforde, op. cit. note 67, 42.

75. Two concepts have been used, which can be understood as two distinctive concepts within the broader idea of *stabilization*. *Reification* is used to denote 'social existence' — existence in the consciousness of the members of a certain social group. *Economic stabilization* is used to denote the 'economic existence' of an artefact — its having a market. Both concepts are used in a continuous and relative way; thus requiring phrases such as 'the *degree* of reification of the High Wheeler is *higher* in the group of "young men of means and nerve" than in the group of elderly men'. See Bijker et al., op. cit. note 2.

76. The notion of relevant social group may provide a starting point for formulating theoretical hypotheses about the occurrence of specific closure

mechanisms under certain conditions. For a tentative sketch of such a theoretical framework, see W. E. Bijker, 'Collectifs technologiques et styles technologiques: éléments pour un modèle explicatif de la construction sociale des artefacts techniques', paper presented to the Collogue STS 'Travailleur collectif et relations science — production', Lyon, France, 10-11 October 1983. In this paper, the ideas of Ludwik Fleck, op. cit. note 9, are used to relate the structure of social groups to the occurrence of specific closure mechanisms. The concepts 'technological community', 'technological style' and 'inclusion in a technological style' are introduced. See also W. Shrum, 'Scientific Specialties and Technical Systems', *Social Studies of Science*, Vol. 14 (1984), 63-90.

77. Collins, op. cit. note 11.

78. J. B. Dunlop, 'An Improvement in Tyres of Wheels for Bicycles, Tricycles, or other Road Cars', *British Patent No. 10607* (date of application: 23 July 1888).

79. Op. cit. note 71, 107.

80. 'The Stanley Exhibition of Cycles', *The Engineer*, Vol. 67 (22 February 1889), 157-58.

81. Mulkay, op. cit. note 53, 80.

82. W. Grew, *The Cycle Industry, its Origin, History and Latest Developments* (London: Sir I. Pitman & Sons, 1921), 8.

83. The American 'Star' (1881) had a small steering wheel in front of the big wheel.

84. Latour and Woolgar, op. cit. note 10.

85. The existence of different degrees of consensus has not yet been researched amongst Core-Set scientists.

86. See also Latour and Woolgar, op. cit. note 10.

87. Knorr-Cetina, op. cit. note 10.

88. H. M. Collins, 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', *Social Studies of Science*, Vol. 11 (1981), 33-62.

89. Pinch, op. cit. note 11.

90. Pickering has confirmed this point to us in personal communication. See also A. Pickering, 'Discovery Claims, Ad Hominem Arguments and Research Programmes: Case Studies in Elementary Particle Physics' (Edinburgh: Science Studies Unit, University of Edinburgh, September 1978, unpublished mimeo).

91. For further details of this controversy, see Pinch, op. cit. note 2. Again we would stress that the example is only illustrative.

92. Advertisements seem to constitute a large and potentially fruitful source for empirical social studies of technology. The considerations which professional advertising designers give to differences between various 'consumer groups' obviously fit our use of different relevant groups. For a fascinating account of the role of advertising in the development of household technology, see Ruth Schwartz Cowan, *More Work For Mother: The Ironies of Household Technology from The Open Hearth to the Microwave* (New York: Basic Books, 1983).

93. *Illustrated London News*, 1880, cited in Woodforde, op. cit. note 67, 60.

94. Woodforde, op. cit. note 67, 89.

95. See L. Croon, *Das Fahrrad und seine Entwicklung* (Berlin: VDI-Verlag, 1939).

96. See Grew, op. cit. note 82.

97. The concept of 'translation' is fruitfully used in an extended way in Callon, op. cit. note 56: see also M. Callon and J. Law, 'On Interests and their Transformation: Enrolment and Counter-Enrolment', *Social Studies of Science*, Vol. 12 (1982), 615-25, and B. Latour, 'Give Me a Laboratory and I will Raise the World', in Knorr-Cetina and Mulkay (eds), op. cit. note 14, 141-70.

98. E. W. Constant, 'Scientific Theory and Technological Testability: Science, Dynamometers, and Water Turbines in the 19th Century', *Technology and Culture*, Vol. 24 (1983), 183-98.

99. A model of such a 'stage 3' explanation is offered in H. M. Collins, 'An Empirical Relativist Programme in the Sociology of Scientific Knowledge', in Knorr-Cetina and Mulkay (eds), op. cit. note 14, 85-113, esp. 96-97.

100. Historical studies which address the third stage may be a useful guide here. See, for example, D. MacKenzie, 'Statistical Theory and Social Interests: A Case Study', *Social Studies of Science*, Vol. 8 (1978), 35-83, and S. Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', in Wallis (ed.), op. cit. note 12, 139-78.

101. Studies which seem to go some way towards meeting the explanatory goals of the third stage are those of Noble, op. cit. note 46, and Lazonick, op. cit. note 46. See also D. MacKenzie, 'Technology, the State and the Strategic Missile: A Preliminary Working Paper' (Edinburgh: Science Studies Unit, University of Edinburgh, 1983, unpublished mimeo).

102. For a more detailed discussion of some methodological and strategical conclusions, drawn from our comparative study of science and technology, see W. E. Bijker and T. J. Pinch, 'La construction sociale de faits et d'artefacts: Impératifs stratégiques et méthodologiques pour une approche unifiée de l'étude des sciences et de la technique', paper presented to 'L'atelier de recherche (III) sur les problèmes stratégiques et méthodologiques des sciences sociales en milieu scientifique et technique' (Paris: 2-3 March 1983).

103. Bruno Latour seems to follow a similar line of argument in his recent plea for what we may call a 'second generation of laboratory studies'; he proposes 'to stick with the methodology developed during laboratory field studies, focusing it not on the laboratory itself but on the construction of the laboratory and its position in the societal milieu': Latour, op. cit. note 97, 143. In order to do this, he has to consider more social groups than the laboratory tribe only. In his study of the Pasteur Laboratory, he actually describes the various roles played by statisticians, veterinarians, farmers, government officials, and the general public.

104. Latour and Woolgar, op. cit. note 10, and Knorr-Cetina, op. cit. note 10.

105. Latour and Woolgar appear to use other data in their study of the construction of TRF; Latour and Woolgar, op. cit. note 10, Chapter 3.

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