The Social Construction of Facts and Artifacts: 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 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 endeavor has been undertaken, we cannot be sure of this.

It is the contention of this chapter that the study of science and the study of technology should, and indeed can, benefit from each other. In particular we argue that the social constructivist view that is prevalent within the sociology of science and also emerging within the sociology of technology provides a useful starting point. We set out the constitutive questions that such a unified social constructivist approach must address analytically and empirically.

This chapter falls into three main sections. In the first part we outline various strands of argumentation and review bodies of literature that we consider to be relevant to our goals. We then discuss the two specific approaches from which our integrated viewpoint has developed: the "Empirical Programme of Relativism" (Collins 1981d) and a social constructivist approach to the study of technology (Bijker et al. 1984). In the third part we bring these two approaches together and give some empirical examples. We conclude by summarizing our provisional findings and by indicating the directions in which we believe the program 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 take each in turn.

Sociology of Science

It is not our intention to review in any depth developments in this field as a whole.[2] We are concerned here with only the recent emergence of the sociology of scientific *knowledge*.[3] 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.[4] 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: 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 (Bloor 1973). 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 program all knowledge and all knowledge claims are to be treated as being socially constructed; that is, 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.[5]

This approach has generated a vigorous program 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.[6] Another has chosen the scientific controversy as the location for their research and have thereby focused on the social construction of scientific knowledge among a wider community of scientists.[7] 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[8] and in the study of public-science debates, such as lead pollution.[9]

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 although there are differences as to the most appropriate methodological strategy to pursue,[10] there is widespread agreement that scientific knowledge can be, and indeed has been, shown to be thoroughly socially constituted. These approaches, which we 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 espistemologically 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) (Barnes 1974; Collins and Pinch 1982). Of course, the successes and failures of certain knowledge cultures still need to be explained, but this is to be seen as a sociological task, not an epistemological one.

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 (Shapin 1982), philosophy of science (Nickles 1982), and science policy (Healey 1982; Collins 1983b). The social constructivist view not only seems to be gaining ground as an important body of work in its own right but also shows every potential of wider application. It is this body of work that forms one of the pillars of our own approach to the study of science and technology.

Science-Technology Relationship

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

One theme that has been pursued by philosophers is the attempt to separate technology from science on analytical grounds. In doing so, philosophers tend to posit overidealized distinctions, such as that science is about the discovery of truth whereas technology is about the application of truth. Indeed, the literature on the philosophy of technology is rather disappointing (Johnston 1984). We prefer to suspend judgment 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.[11] 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 (Sherwin and Isenson 1966, 1967). These results were to some extent supported by a later British study (Langrish et al. 1972). On the other hand, Project TRACES, funded by the NSF in response to Project Hindsight, found that most technological development stemmed from basic research (Illinois Institute of Technology, 1968). All these studies have been criticized for lack of methodological rigor, and one must be cautious in drawing any firm conclusions from such work (Kreilkamp 1971; Mowery and Rosenberg 1979). 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 that can be attached to basic science therefore probably varies considerably.[12] Certainly the view prevalent in the "bad old days" (Barnes 1982a)—that science discovers and technology applies—will no longer suffice. Simplistic models and generalizations have been abandoned. As Layton 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. (Layton 1977, p. 210)

Researchers concerned with measuring the exact interdependence of science and technology seem to have asked the wrong question because they have assumed that science and technology are well-defined monolithic structures. In short, they have not grasped that science and technology are themselves socially produced in a variety of social circumstances (Mayr 1976). It does seem, however, that there is now a move toward 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. (Layton 1977, p. 209)

Barnes has recently described this change of thinking:

> 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. (Barnes 1982a, p. 166)

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 on the resources of the other when and where such resources can profitably be exploited. In other words, both science and technology are socially constructed cultures and bring to bear whatever cultural resources are appropriate for the purposes at hand. In his 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 do not pursue this issue further in this chapter, the social construction of the science-technology relationship is clearly a matter deserving further empirical investigation.

Technology Studies

Our discussion of technology studies work is even more schematic. There is a large amount of writing that 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 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 macroeconomic factors pertaining to the economy as a whole.[13] 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" (Whitley 1972) 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. (Layton 1977, p. 198)

Only recently have economists started to look into this black box.[14]

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 seems to be rather arbitrary (for an example of a six-stage process see figure 1).[15] Although such studies have undoubtedly contributed much to our understanding of the conditions for economic success in technological innovation, because they ignore the technological content they cannot be used as the basis for a social constructivist view of technology.[16]

This criticism cannot be leveled 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. Few scholars (but there are some notable exceptions) seem concerned with generalizing beyond historical instances, and it is difficult to discern any overall patterns on which to build a theory of technology (Staudenmaier 1983, 1985). This is not to say that such studies might not be useful building blocks 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.[17]

The second problem concerns the asymmetric focus of the analysis. For example, it has been claimed that in twenty-five volumes of *Technology and Culture* only nine articles were devoted to the study of failed technological innovations (Staudenmaier 1985). 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. (Ferguson 1974b, p. 19)

This preference for successful innovations seems to lead scholars to assume that the success of an artifact is an explanation of its subsequent development. Historians of technology often seem content to rely on the manifest success of the artifact 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" charac-

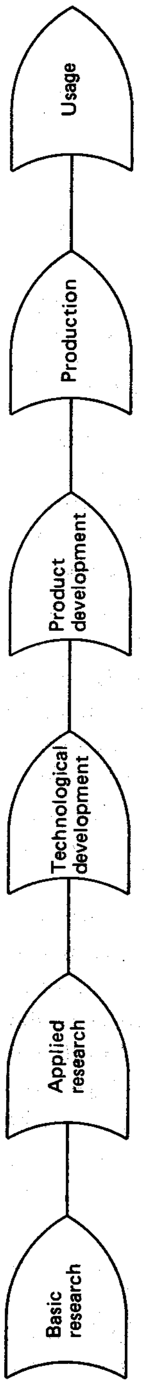

Figure 1
A six-stage model of the innovation process.

teristics 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! (Kaufman 1963, p. 61)

However, a more detailed study of the developments of plastic and varnish chemistry, following the publication of the Bakelite process in 1909 (Baekeland 1909c, d), shows that Bakelite was at first hardly recognized as the marvelous synthetic resin that it later proved to be.[18] And this situation did not change 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 (Haynes 1954, pp. 137–138) and made it possible to keep the price sufficiently low to compete with (semi-)natural resins, such as celluloid.[19] 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 on the retrospective success of the artifact leaves much untold.

Given our intention of building a sociology of technology that treats technological knowledge in the same symmetric, 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 artifact 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 on which research is focused (Staudenmaier 1985; Hughes 1979b). For example, the systems approach to technology,[20] consideration of the effect of labor relations on technological development,[21] and detailed studies of some not-so-successful inventions[22] seem to herald departures from the "old" history of technology. Such work promises to be valuable for a sociological analysis of technology, and we return to some of it later.

The final body of work we wish to discuss is what might be described as "sociology of technology."[23] 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 (1972) and Dosi (1982), who advocate the description of technological knowledge in terms of Kuhnian paradigms.[24] Such approaches certainly appear to be more promising than standard descriptive historiography, but it is not clear whether or not these authors share our understanding of technological artifacts as social constructs. For example, neither Johnston nor Dosi considers explicitly the need for a symmetric sociological explanation that treats successful and failed artifacts in an equivalent way. Indeed, by locating their discussion at the level of technological paradigms, we are not sure how the artifacts 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 utilized.[25] 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.

The possibilities of a more radical social constructivist view of technology have been touched on by Mulkay (1979a). 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 (1966)) 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 on which it was based. We find this second point not entirely satisfactory. We would rather stress that the truth or falsity of scientific knowledge is irrelevant to sociological analysis of belief: To retreat to the argument that science may be wrong but good technology can still be based on it is missing 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 that 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" (Mulkay 1979a, p. 77). 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 focusing on technological controversies. He draws on 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 (Callon 1980a, b, 1981b, and this volume). 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 (Noble 1984). Noble's explanatory goals come from a rather different (Marxist) tradition,[26] and his study has much to recommend it: He considers the development of both a successful and a failed technology and gives a symmetric account of both developments. Another intriguing study in this tradition is Lazonick's account (1979) of the introduction of the self-acting mule: 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 artifacts might be approached empirically: Six case studies were carried out, using historical sources.[27]

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 program. There are exceptions, however, and some recent studies in the sociology of technology present promising starts on which a unified approach could be built. We now give a more extensive account of how these ideas may be synthesized.

EPOR and SCOT

In this part we outline in more detail the concepts and methods that we wish to employ. We start by describing the "Empirical Programme of Relativism" as it was developed in the sociology of scientific knowledge. We then go on to discuss in more detail the approach taken by Bijker and his collaborators in the sociology of technology.

The Empirical Programme of Relativism (EPOR)

The EPOR is an approach that 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 on the empirical study of contemporary scientific developments and the study, in particular, of scientific controversies.[28]

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. Although this interpretative flexibility can be recovered in certain circumstances, it remains the case that such flexibility soon disappears in science; that is, a scientific consensus as to what the "truth" is in any particular instance usually emerges. Social mechanisms that 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" (Collins 1981d, p. 7).

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 program are better researched than others. The third stage of the program 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" (Collins 1981b). 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. For the purposes of the third stage, the notion of a core set may be too limited.

The 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 program 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 on specifically (SCOT) is only in its early empirical stages, although clearly gaining momentum.[29]

In SCOT the developmental process of a technological artifact is described as an alternation of variation and selection.[30] This results in a "multidirectional" model, in contrast with the linear models used explicitly in many innovation studies and implicitly in much history of technology. Such a multidirectional view is essential to any social constructivist account of technology. Of course, with historical hindsight, it is possible to collapse the multidirectional model on to 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.[31] Applied to the level of artifacts in this development, this multidirectional view results in the description summarized in figure 2. Here we see the artifact "Ordinary" (or, as it was nicknamed after becoming less ordinary, the "Penny-farthing"; figure 3) 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 quite 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 4. In this representation the so-called safety ordinaries (Xtraordinary (1878), Facile (1879), and Club Safety (1885)) figure only as amusing aberrations that need not be taken seriously (figure 5, 6, and 7). Such a retrospective description can be challenged by looking at the actual situation in the 1880s. Some of the "safety ordinaries" were produced commercially, whereas Lawson's Bicyclette, which seems to play an important role in the linear model, proved to be a commercial failure (Woodforde 1970).

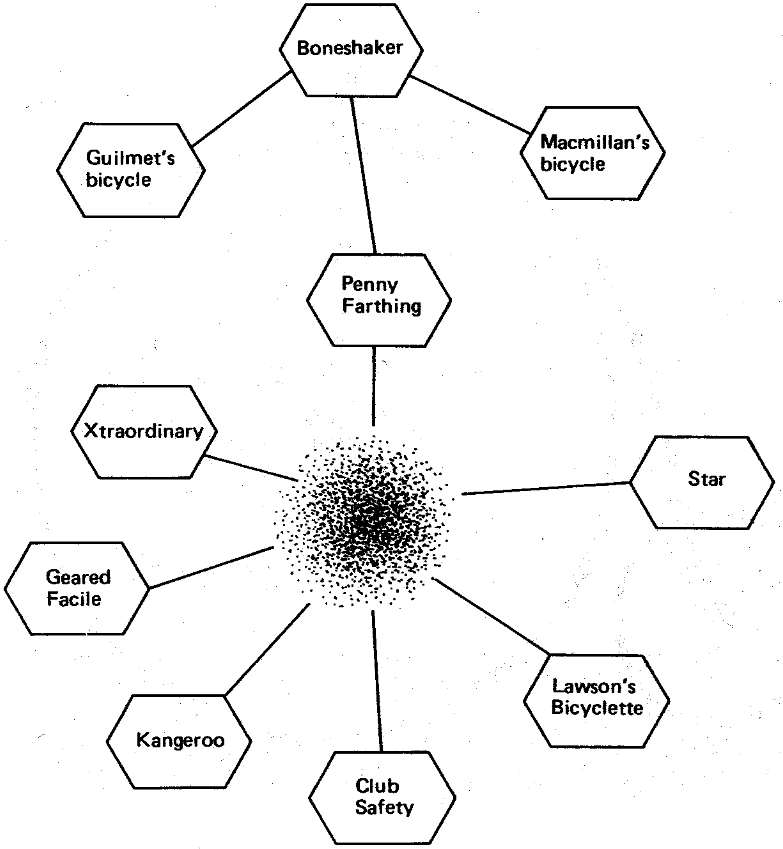

Figure 2
A multidirectional view of the developmental process of the Penny Farthing bicycle. The shaded area is filled in and magnified in figure 11. The hexagons symbolize artifacts.

However, if a multidirectional 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 artifact at particular moments. The rationale for this move is the same as that for focusing on scientific controversies within EPOR. In this way, one can expect to bring out more clearly the interpretative flexibility of technological artifacts.

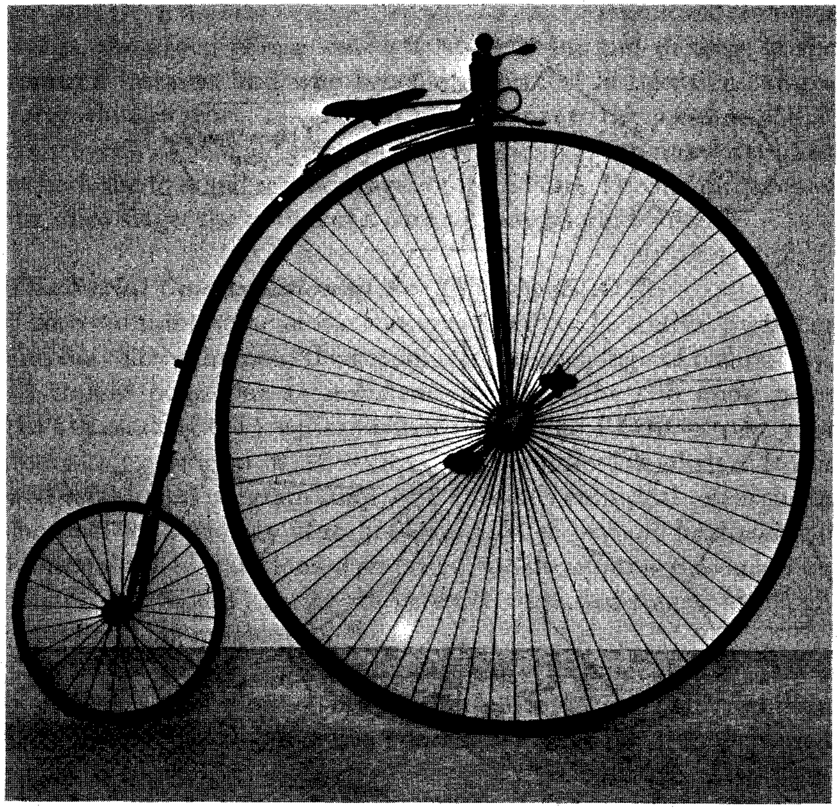

Figure 3
A typical Penny Farthing, the Bayliss-Thomson Ordinary (1878). Photograph courtesy of the Trustees of the Science Museum, London.

In deciding which problems are relevant, the social groups concerned with the artifact and the meanings that those groups give to the artifact play a crucial role: A problem is defined as such only when there is a social group for which it constitutes a "problem."

The use of the concept of a relevant social group is quite straightforward. The phrase 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 artifact.[32] In deciding which social groups are relevant, we must first ask whether the artifact has any meaning at all for the members of the social group under investigation. Obviously, the social group of "consumers" or "users" of the artifact fulfills this requirement. But also less obvious social groups

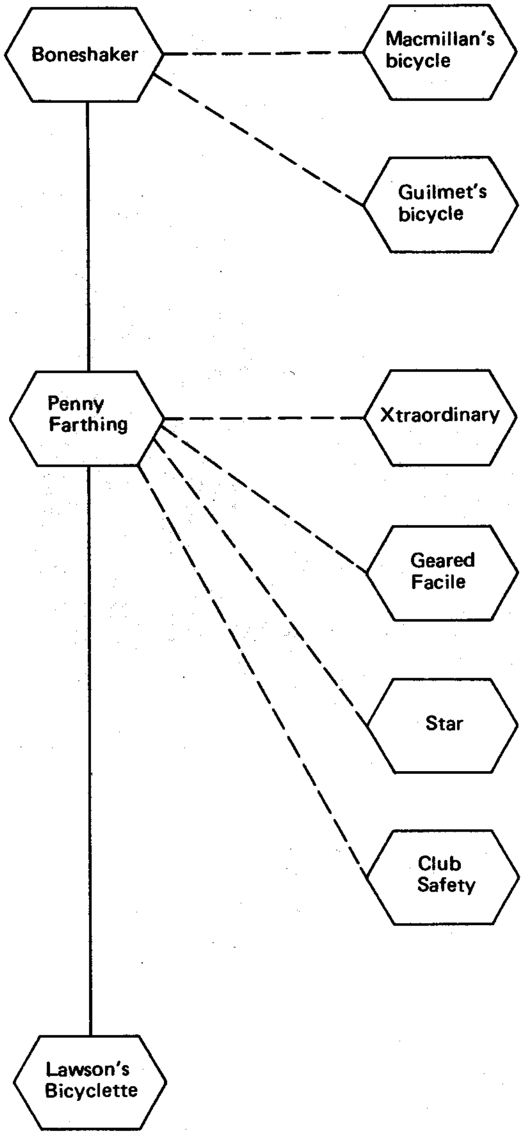

Figure 4
The traditional quasi-linear view of the developmental process of the Penny Farthing bicycle. Solid lines indicate successful development, and dashed lines indicate failed development.

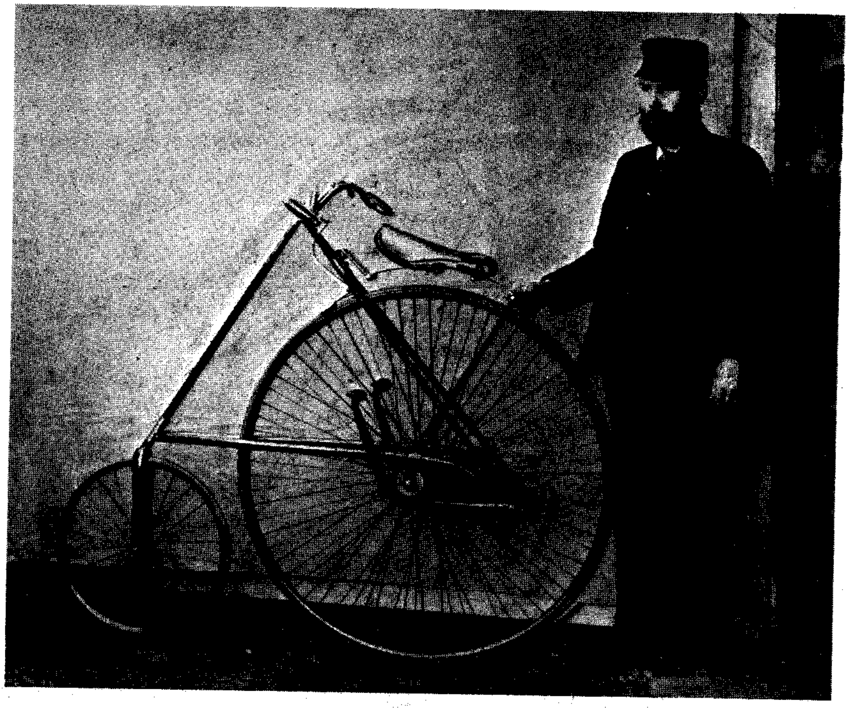

Figure 5
The American Star bicycle (1885). Photograph courtesy of the Trustees of the Science Museum, London.

may need to be included. In the case of the bicycle, one needs to mention the "anticyclists." Their actions ranged from derisive cheers to more destructive methods. For example, Reverend 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. (Meadows, cited in Woodforde 1970, pp. 49–50)

Clearly, for the anticyclists the artifact "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 artifact—or is it more effective to describe the developmental process by dividing a rather heterogeneous group into several

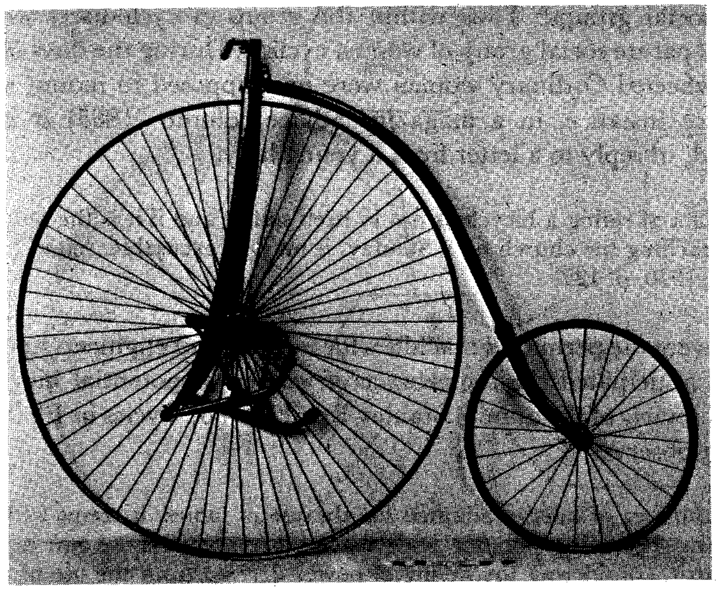

Figure 6
Facile bicycle (1874). Photograph courtesy of the Trustees of the Science Museum, London.

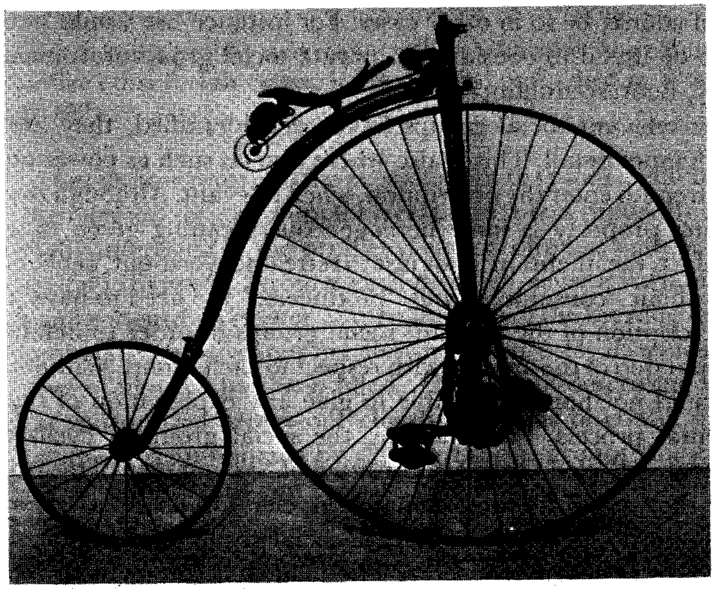

Figure 7
A form of the Kangaroo bicycle (1878). Photograph courtesy of the Trustees of the Science Museum, London.

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. (cited in Woodforde 1970, p. 122)

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. (Stanley Exhibition of Cycles, 1890, pp. 107–108)

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 would not expect it to be useful to consider a separate social group of women users of, say, fluorescent lamps.

Once the relevant social groups have been identified, they are described in more detail. This is also where aspects such as power or economic strength enter the description, when relevant. Although the only defining property is some homogeneous meaning given to a certain artifact, 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 to define better the function of the artifact 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" (Woodforde 1970, p. 47). For this social group the function of the bicycle was primarily for sport. The following comment in the *Daily Telegraph* (September 7, 1877) emphasizes sport, rather than transport:

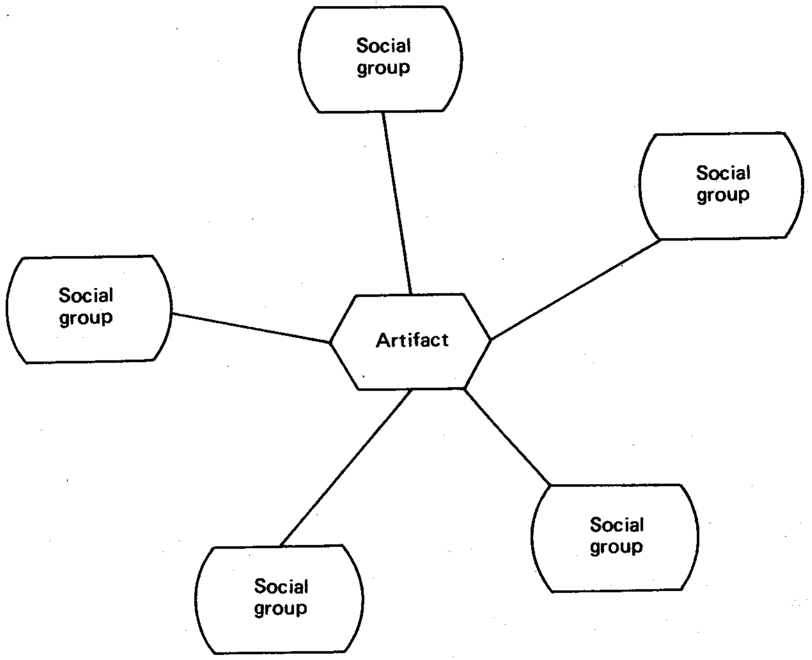

Figure 8
The relationship between an artifact and the relevant social groups.

> Bicycling is a healthy and manly pursuit with much to recommend it, and, unlike other foolish crazes, it has not died out. (cited in Woodforde 1970, p. 122)

Let us now return to the exposition of the model. Having identified the relevant social groups for a certain artifact (figure 8), we are especially interested in the problems each group has with respect to that artifact (figure 9). Around each problem, several variants of solution can be identified (figure 10). In the case of the bicycle, some relevant problems and solutions are shown in figure 11, in which the shaded area of figure 2 has been filled. This way of describing the developmental process brings out clearly all kinds of 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); and moral conflicts (for example, women wearing skirts or trousers on high-wheelers; figure 12). Within this scheme, various solutions to these conflicts and problems are possible—not

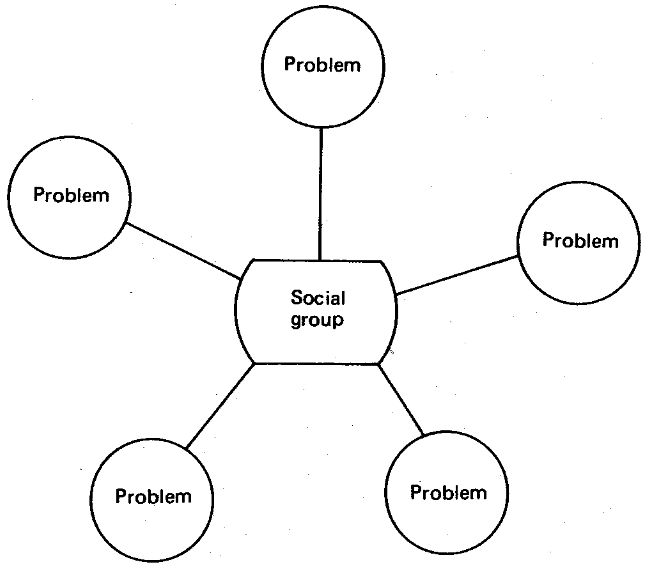

Figure 9
The relationship between one social group and the perceived problems.

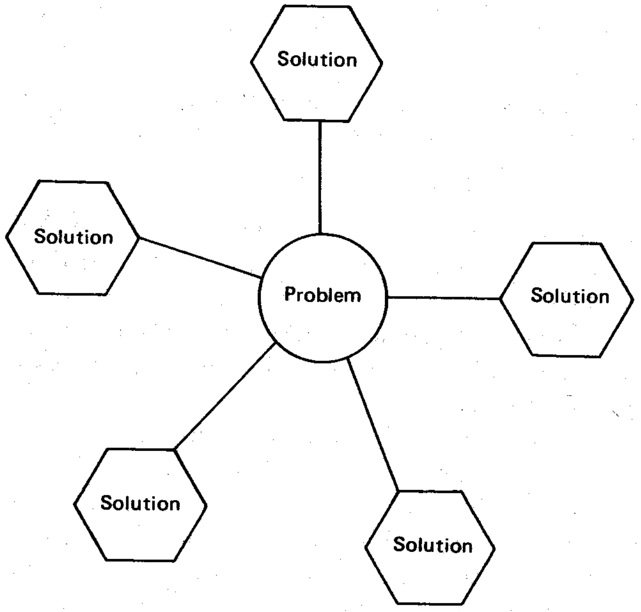

Figure 10
The relationship between one problem and its possible solutions.

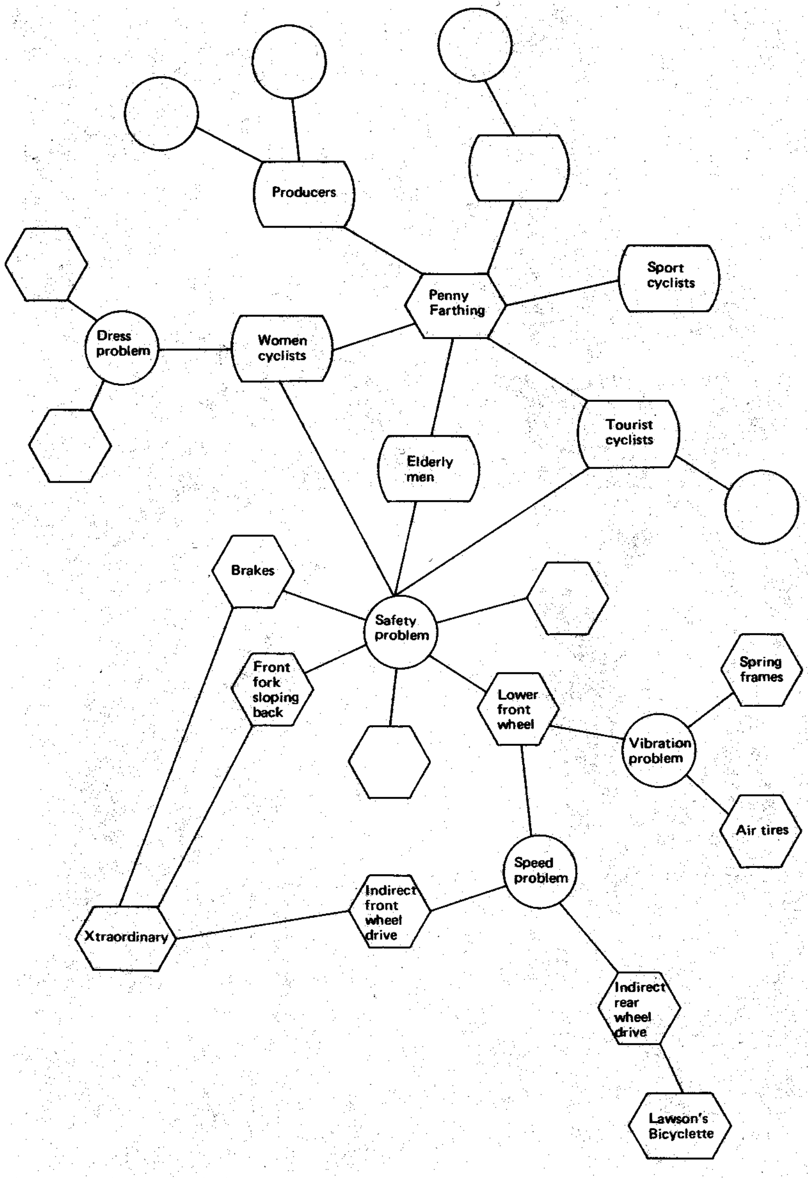

Figure 11
Some relevant social groups, problems, and solutions in the developmental process of the Penny Farthing bicycle. Because of lack of space, not all artifacts, relevant social groups, problems, and solutions are shown.

Figure 12
A solution to the women's dressing problem with respect to the high-wheeled Ordinary. This solution obviously has technical and athletic aspects. Probably, the athletic aspects prevented the solution from stabilizing. The set-up character of the photograph suggests a rather limited practical use. Photograph courtesy of the Trustees of the Science Museum, London.

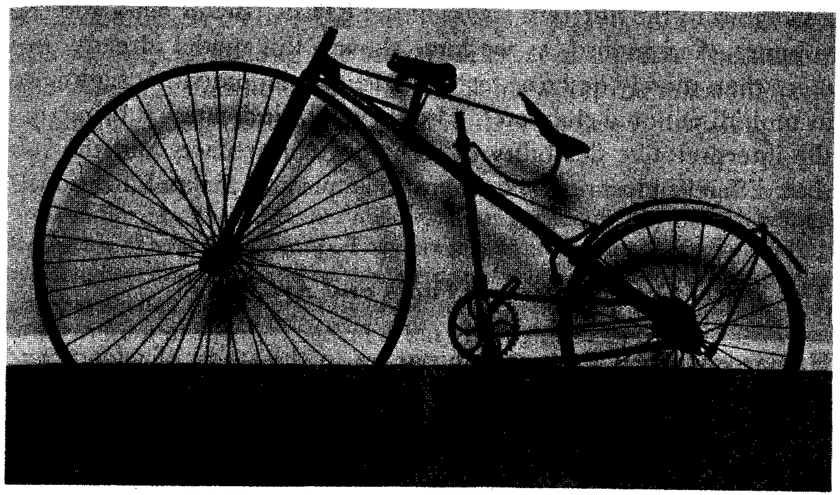

Figure 13
Lawson's Bicyclette (1879). Photograph courtesy of the Trustees of the Science Museum, London.

only technological ones but also judicial or even moral ones (for example, changing attitudes toward women wearing trousers).

Following the developmental process in this way, we see growing and diminishing degrees of stabilization of the different artifacts.[33] In principle, the degree of stabilization is different in different social groups. By using the concept of stabilization, we see that the "invention" of the safety bicycle was not an isolated event (1884), but 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 crocodilelike bicycle with a relatively low front wheel and rear chain drive (Lawson's Bicyclette; figure 13). By the end of the period, the phrase "safety bicycle" denoted a low-wheeled bicycle with rear chain drive, diamond frame, and air tires. As a result of the stabilization of the artifact after 1898, one did not need to specify these details: They were taken for granted as the essential "ingredients" of the safety bicycle.

We want to stress that our model is not used as a mold 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 to our purposes. This is not to say that there are no explanatory and theoretical aims, analogous to the different stages of the EPOR (Bijker 1984 and this volume). And indeed, as we have shown, this model already does more than merely describe technological development: It highlights its multidirectional character. Also, as will be indicated, it brings out the interpretative flexibility of technological artifacts and the role that different closure mechanisms may play in the stabilization of artifacts.

The Social Construction of Facts and Artifacts

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

Interpretative Flexibility

The first stage of the EPOR involves the demonstration of the interpretative flexibility of scientific findings. In other words, it must be shown that different interpretations of nature are available to scientists and hence that nature alone does not provide a determinant outcome to scientific debate.[34]

In SCOT, the equivalent of the first stage of the EPOR would seem to be the demonstration that technological artifacts are culturally constructed and interpreted; in other words, the interpretative flexibility of a technological artifact must be shown. By this we mean not only that there is flexibility in how people think of or interpret artifacts but also that there is flexibility in how artifacts are *designed*. There is not just one possible way or one best way of designing an artifact. 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 artifact "air tyre." For some, this artifact was a solution to the vibration problem of small-wheeled vehicles:

> [The air tire 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. (Dunlop 1888, p. 1)

For others, the air tire was a way of going faster (this is outlined in more detail later). For yet another group of engineers, it was an ugly looking way of making the low-wheeler even less safe (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. (Stanley Exhibition of Cycles, 1890, p. 107)

And indeed, other artifacts 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. (Stanley Exhibition of Cycles, 1889, pp. 157–158)

Most solutions used various spring constructions in the frame, the saddle, and the steering-bar (figure 14). In 1896, even after the safety bicycle (and the air tire 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 artifact. We call these differences "radical" because the *content* of the artifact 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

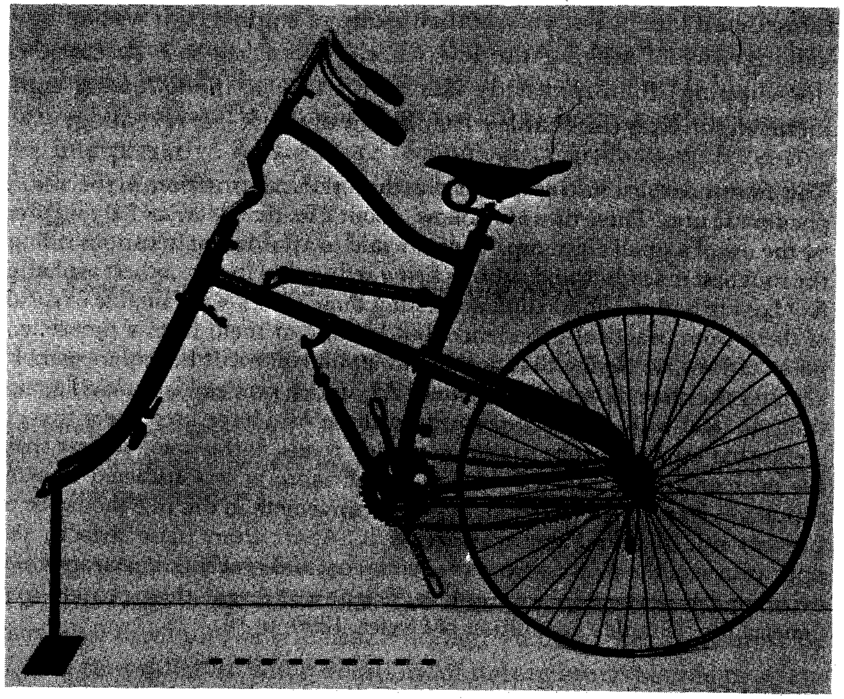

Figure 14
Whippet spring frame (1885). Photograph courtesy of the Trustees of the Science Museum, London.

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" (Mulkay 1979a, p. 80).

We think that our account—in which the different interpretations by social groups of the content of artifacts lead by means of different chains of problems and solutions to different further developments—involves the content of the artifact 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 one 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 tire. 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:

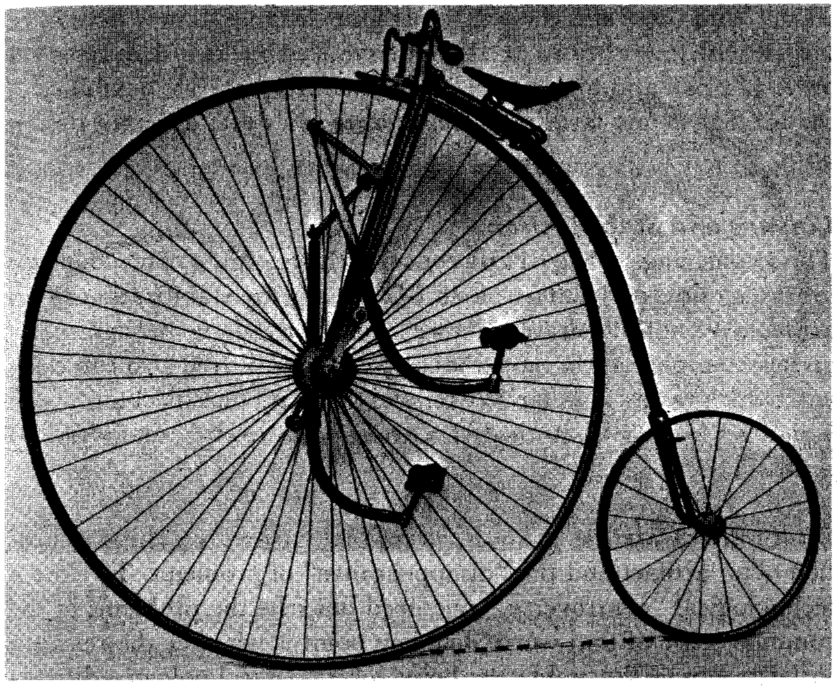

Figure 15
Singer Xtraordinary bicycle (1878). Photograph courtesy of the Trustees of the Science Museum, London.

> 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. (Grew 1921, p. 8)

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 artifacts such as Lawson's Bicyclette (1879) and the Xtraordinary (1878; figure 15). 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 backward, or reversed order of small and high wheel. Thus the interpretative flexibility of the artifact 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 artifact. We now illustrate what we mean by a closure mechanism by giving examples of two types that seem to have played a role in cases with which we are familiar. We refer to the particular mechanisms on which we focus as rhetorical closure and closure by redefinition of problem.

Rhetorical Closure Closure in technology involves the stabilization of an artifact and the "disappearance" of problems. To close a technological "controversy," one need not *solve* the problems 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 that a social group gives to an artifact.[35] Thus, for instance, an attempt was made to "close" the "safety controversy" around the high-wheeler by simply claiming that the artifact was perfectly safe. An advertisement for the "Facile" (*sic*!) Bicycle (figure 16) 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. (*Illustrated London News*, 1880; cited in Woodforde 1970, p. 60)

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 the Problem We have already mentioned the controversy around the air tire. 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. (Woodforde 1970, p. 89)

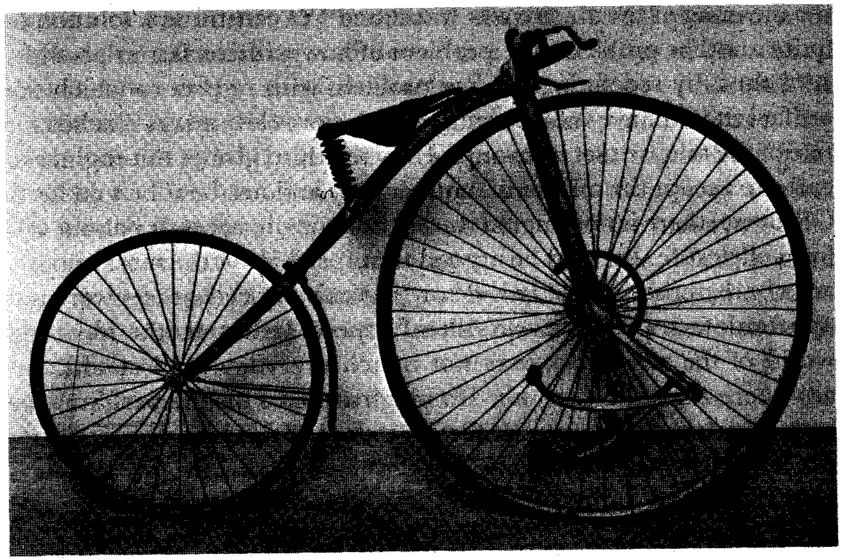

Figure 16
Geared Facile bicycle (1888). Photograph courtesy of the Trustees of the Science Museum, London.

For Dunlop and the other protagonists of the air tire, originally the air tire meant a solution to the vibration problem. However, the group of sporting cyclists riding their high-wheelers did not accept that as a problem at all. Vibration presented a problem only to the (potential) users of the low-wheeled bicycle. Three important social groups were therefore opposed to the air tire. But then the air tire was mounted on a racing bicycle. When, for the first time, the tire 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 (Croon 1939). Soon handicappers had to give racing cyclists on high-wheelers a considerable start if riders on air-tire low-wheelers were entered. After a short period no racer of any pretensions troubled to compete on anything else (Grew 1921).

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 tire in its meaning as an antivibration device. One can say, we think, that the meaning of the air tire was translated[36] 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 artifact 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 tire 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, any more than the Formula-1 racing car bears on the performance requirements of the average family sedan). Still, bicycle races have played an important role in the development of the bicycle, and because racing can be viewed as a specific form of testing, this observation is much in line with Constant's recent plea to pay more attention to testing procedures in studying technology (Constant 1983).

The Wider Context

Finally, we come to the third stage of our research program. The task here in the area of technology would seem to be the same as for science—to relate the content of a technological artifact to the wider sociopolitical milieu. This aspect has not yet been demonstrated for the science case,[37] at least not in contemporaneous sociological studies.[38] However, the SCOT method of describing technological artifacts by focusing on 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 artifact. Because we have shown 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. To follow this line of analysis, see Bijker (this volume).

Conclusion

In this chapter we have been concerned with outlining an integrated social constructivist approach to the empirical study of science and technology. We 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 are 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 is 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)—on 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 technology. In particular, we have seen that the concepts of interpretative flexibility and closure mechanism and the notion of social group can be given empirical reference in the social study of technology.

As we have noted throughout this chapter, 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 that some readers might have expected to be dealt with in the first paragraph of this chapter, namely, the question of how to distinguish science from technology. We think that it is rather unfruitful to make such an a priori distinction. Instead, it seems worthwhile to start with commonsense 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 program. This would constitute another concrete result of the integrated study of the social construction of facts and artifacts.

Notes

This chapter is a shortened and updated version of Pinch and Bijker (1984).

We are grateful to Henk van den Belt, Ernst Homburg, Donald MacKenzie, and Steve Woolgar for comments on an earlier draft of this chapter. We would like to thank the Stiftung Volkswagen, Federal Republic of Germany, the Twente University of Technology, The Netherlands, and the UK SSRC (under grant G/00123/0072/1) for financial support.

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 into account 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 by those who are familiar with an extensive body of technical literature (indeed, many researchers are ex–natural scientists). Having gained such expertise, the researchers tend to stay within the domain where that expertise can best be deployed. Similarly, R&D studies and innovation studies, in which the analysis centers on the firm and the marketplace, have tended to demand the specialized competence of economists. Such disparate bodies of work do not easily lead to a more integrated conception of science and technology. One notable exception is Ravetz (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. A comprehensive review can be found in Mulkay and Milič (1980).

3. For a recent review of the sociology of scientific knowledge, see Collins (1983c).

4. For a discussion of the earlier work (largely associated with Robert Merton and his students), see Whitley (1972).

5. For more discussion, see Barnes (1974), Mulkay (1979b), Collins (1983c), and Barnes and Edge (1982). The origins of this approach can be found in Fleck (1935).

6. See, for example, Latour and Woolgar (1979), Knorr-Cetina (1981), Lynch (1985a), and Woolgar (1982).

7. See, for example, Collins (1975), Wynne (1976), Pinch (1977, 1986), Pickering (1984), and the studies by Pickering, Harvey, Collins, Travis, and Pinch in Collins (1981a).

8. Collins and Pinch (1979, 1982).

9. Robbins and Johnston (1976). For a similar analysis of public science controversies, see Gillespie et al. (1979) and McCrea and Markle (1984).

10. Some of the most recent debates can be found in Knorr-Cetina and Mulkay (1983).

11. The *locus classicus* is the study by Hessen (1931).

12. See, for example, de Solla Price (1969), Jevons (1976), and Mayr (1976).

13. See, for example, Schumpeter (1928, 1942), Schmookler (1966, 1972), Freeman (1974, 1977), and Scholz (1976).

14. See, for example, Rosenberg (1982), Nelson and Winter (1977, 1982), and Dosi (1982, 1984). A study that preceded these is Rosenberg and Vincenti (1978).

15. Adapted from Uhlmann (1978), p. 45.

16. For another critique of these linear models, see Kline (1985).

17. Shapin writes that "a proper perspective of the uses of science might reveal that sociology of knowledge and history of technology have more in common than is usually thought" (1980, p. 132). Although we are sympathetic to Shapin's argument, we think the time is now ripe for asking more searching questions of historical studies.

18. Manuals describing resinous materials do mention Bakelite but not with the amount of attention that, 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 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 that part is devoted to Bakelite (Bottler 1919). See also Matthis (1920).

19. For an account of other aspects of Bakelite's success, see Bijker (this volume).

20. See, for example, Constant (1980), Hughes (1983), and Hanieski (1973).

21. See, for example, Noble (1979), Smith (1977), and Lazonick (1979).

22. See, for example, Vincenti (1986).

23. There is an American tradition in the sociology of technology. See, for example, Gilfillan (1935), Ogburn (1945), Ogburn and Meyers Nimkoff (1955), and Westrum (1983). A fairly comprehensive view of the present state of the art in German sociology of technology can be obtained from Jokisch (1982). Several studies in the sociology of technology that attempt to break with the traditional approach can be found in Krohn et al. (1978).

24. Dosi uses the concept of technological trajectory, developed by Nelson and Winter (1977); see also Van den Belt and Rip (this volume). Other approaches to technology based on Kuhn's idea of the community structure of science are mentioned by Bijker (this volume). See also Constant (this volume) and the collection edited by Laudan (1984a).

25. 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 in 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. See, for example, the inconclusive discussion over whether a Kuhnian analysis applies to psychology in Palermo (1973). A notable exception is Barnes's contribution to the discussion of Kuhn's work (Barnes 1982b).

26. For a valuable review of Marxist work in this area, see MacKenzie (1984).

27. For a provisional report of this study, see Bijker et al. (1984). The five artifacts that are studied are Bakelite, fluorescent lighting, the safety bicycle, the Sulzer loom, and the transistor. See also Bijker (this volume).

28. Work that might be classified as falling within the EPOR has been carried out primarily by Collins, Pinch, and Travis at the Science Studies Centre, University of

Bath, and by Harvey and Pickering at the Science Studies Unit, University of Edinburgh. See, for example, the references in note 7.

29. See, for example, Bijker and Pinch (1983) and Bijker (1984 and this volume). Studies by Van den Belt (1985), Schot (1985, 1986), Jelsma and Smit (1986), and Elzen (1985, 1986) are also based on SCOT.

30. Constant (1980) used a similar evolutionary approach. Both Constant's model and our model seem to arise out of the work in evolutionary epistemology; see, for example, Toulmin (1972) and Campbell (1974). Elster (1983) gives a review of evolutionary models of technical change. See also Van den Belt and Rip (this volume).

31. 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 Great Britain to point out that both the automobile and the aircraft industries are, in a way, descendants from the bicycle industry. Many names occur in the histories of both the bicycle and the autocar: Triumph, Rover, Humber, and Raleigh, to mention but a few (Caunter 1955, 1957). The Wright brothers both sold and manufactured bicycles before they started to build their flying machines—mostly made out of bicycle parts (Gibbs-Smith 1960).

32. 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. See also Law (this volume) on the demarcation of networks and Bijker (this volume).

33. Previously, two concepts have been used that can be understood as two distinctive concepts within the broader idea of stabilization (Bijker et al. 1984). *Reification* was used to denote social existence—existence in the consciousness of the members of a certain social group. *Economic stabilization* was used to indicate the economic existence of an artifact—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."

34. The use of the concepts of interpretative flexibility and rhetorical closure in science cases is illustrated by Pinch and Bijker (1984).

35. Advertisements seem to constitute a large and potentially fruitful data source for empirical social studies of technology. The considerations that professional advertising designers give to differences among various "consumer groups" obviously fit our use of different relevant groups. See, for example, Schwartz Cowan (1983) and Bijker (this volume).

36. The concept of translation is fruitfully used in an extended way by Callon (1980b, 1981b, 1986), Callon and Law (1982), and Latour (1983, 1984).

37. A model of such a "stage 3" explanation is offered by Collins (1983a).

38. Historical studies that address the third stage may be a useful guide here. See, for example, MacKenzie (1978), Shapin (1979, 1984), and Shapin and Schaffer (1985).

The Evolution of Large Technological Systems

Thomas P. Hughes

Definition of Technological Systems

Technological systems contain messy, complex, problem-solving components. They are both socially constructed and society shaping.[1] Among the components in technological systems are physical artifacts, such as the turbogenerators, transformers, and transmission lines in electric light and power systems.[2] Technological systems also include organizations, such as manufacturing firms, utility companies, and investment banks, and they incorporate components usually labeled scientific, such as books, articles, and university teaching and research programs. Legislative artifacts, such as regulatory laws, can also be part of technological systems. Because they are socially constructed and adapted in order to function in systems, natural resources, such as coal mines, also qualify as system artifacts.[3]

An artifact—either physical or nonphysical—functioning as a component in a system interacts with other artifacts, all of which contribute directly or through other components to the common system goal. If a component is removed from a system or if its characteristics change, the other artifacts in the system will alter characteristics accordingly. In an electric light and power system, for instance, a change in resistance, or load, in the system will bring compensatory changes in transmission, distribution, and generation components. If there is repeated evidence that the investment policies of an investment bank are coordinated with the sales activities of an electrical manufacturer, then there is likely to be a systematic interaction between them; the change in policy in one will bring changes in the policy of the other. For instance, investment banks may systematically fund the purchase of the electric power plants of a particular manufacturer with which they share owners and interlocking boards of directors.[4] If courses in an engineering school shift emphasis from the study of direct current (dc) to alternating current (ac) at about