biography of Huygens.

In such a biography the balance between the mathematical and the other aspects of Huygens' life and work will be different. In the present book, however, it is his mathematical genius that is central, rather than the person. And so I should not apologize about the mathematics, although I certainly hesitated to take up so much of the journal's space and the reader's time with the intricacies of small and smaller line segments at a parabola and a circle. But, as Yoder makes clear, masterly insights such as the one about the parabola and the circle are the essence of Huygens' mathematical genius. The insights gave him a sense of power, self-respect, prestige, and no doubt pleasure. Yoder clearly explains the power, makes us understand the self-respect and the prestige and enables us to re-experience the pleasure. Her beautiful book brings Huygens close to us, and that is a great achievement.

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Follow the Actors? Some Recent Dutch Studies on the Development of Science and Technology

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Review of: D. Stemerding, *Plants, Animals and Formulae. Natural History in the Light of Latour's Science in Action and Foucault's The Order of Things* (WMW Publicatie 4; Faculteit Wijsbegeerte en Maatschappijwetenschappen Universiteit Twente, Enschede, 1991; ISBN 90-365-0379-5), 202 pp.; W.E. Bijker, *The Social Construction of Technology* (Eijsden, 1990; ISBN 90-900-3723-3), 218 pp.; B. Elzen, *Scientists and Rotors. The Development of Biochemical Ultracentrifuges* (Enschede, 1988; ISBN 90-900-2289-9), 450 pp.

It all began with the discovery of symmetry. Science suddenly seemed to be less different from non-science than many people had believed. During the 1960s and 70s philosophers, sociologists, historians and antropologists vied with each other to show that science was actually not as 'rational' as it had been made out to be, or conversely, that other modes of thought were less 'irrational' than long had been assumed. Galileï and the Noble Savage appeared to be twins after all.

In keeping with these insights, David Bloor argued that there was no sound reason to exempt science from the sociology of knowledge. In his view, all knowledge claims should be explained in the same terms, regardless whether they were considered 'true' or 'false', 'scientific' or 'non-scientific'. Each of them should be equally submitted to an inquiry into its social origins. It has also been suggested to adopt this symmetrical approach to the study of the dissemination of scientific knowledge; in principle, the spread of scientific knowledge should be treated on a par with the diffusion of products from any other source of knowledge. The principle of symmetry has further been expanded to the realm of technology. Trevor Pinch and Wiebe Bijker claimed that the 'success' and 'failure' or the 'working' and 'non-working' of artefacts all should be explained by one and the same conceptual framework. Michel Callon and Bruno Latour even went so far as to propose that this methodological rule should also be applied to the roles of human and non-human actors, and that the construction of society should be understood in a similar way as that of the scientific/technical world.[5]

But if in the beginning all was symmetry, the key problem was of course to account for the presence of *asymmetry*. There was no denying that even though all knowledge claims were created equal some have ended up to be more equal than others. Science has come to enjoy a higher status in modern society than many other sources of knowledge, if not all. Some statements about the world have been certified as 'true', whereas others have been labelled as 'false', or have sunk into oblivion. Some artefacts have been accepted as 'working' or 'successful', while others have been dubbed as failures. How should this outcome be explained, if it can not just be ascribed to the verdict of Nature?

In theory, there are many different ways in which the issue might be solved. And in fact almost any answer that one can think of, has already been proposed, either separately or in combination with other solutions. The spectrum runs all the way from 'market'-like to 'command'-type explanations, or to put it differently: From arguments based on the postulate of free choice to explanations resting on the thesis that the shift from symmetry to asymmetry is ultimately a matter of coercion.

From the market side, it can be argued that the 'success' or 'failure' of particular statements, artefacts or modes of thought is directly related to the rate of return that their acceptance will yield to individual 'consumers' or 'investors'.

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[5] D. Bloor, *Knowledge and Social Imagery* (London, 1976), esp. pp. 2-5; C.A. Davids, *Zeewezen en wetenschap. De wetenschap en de ontwikkeling van de navigatietechniek in Nederland tussen 1585 en 1815* (Amsterdam/Leiden, 1986) (reviewed in *Tractrix*, vol. 1, 1989), pp. 25-31; Bijker, *The Social Construction of Technology*, pp. 172, 186-189.

The cost-benefit ratio of adopting them probably outweighed any advantage of accepting other items of knowledge. From the command side, it may be contended that adoption rather resulted from the fact that the individuals concerned had no other choice left; they were just forced to obey the rules laid down by large organizations or public authorities.

Between these extremes there exists of course a whole series of intermediate positions. It might be stated, for instance, that preferences are less determined by objective cost-benefits considerations than to the strength of the 'rhetoric' used to uphold or dispute the value of particular products of knowledge. Or it could be claimed that the scope for free choice was reduced by the growth of a whole network of accepted facts and artefacts, joined with patronage systems and supporting institutions, which diminish the power of individuals to challenge the validity of particular claims to knowledge. Or it could be asserted that the range of choice for single actors was restricted by the inertia of conventional wisdom or by the structure of some larger mental framework. But which of these solutions, or what mixture of them, is the most adequate to enhance our understanding of the shift from symmetry to asymmetry? And under what conditions do they in fact apply? These questions can only be answered by empirical research.

The issues of symmetry and asymmetry are the underlying theme of all three studies under review. All of them are Ph.D. dissertations prepared in the framework of science, technology and society program developed at the University of Twente during the 1980s. Although each is focused on a different aspect of the scientific/technical world, they agree in their symmetrical perspective to all knowledge claims as well as in their concern with the problem of asymmetry.

The book by Dirk Stemerding deals with the development in classification systems in natural history during the 18th and 19th centuries. After giving a fascinating account of the practice of natural history and of the ideas, ambitions and activities of its leading figures at the time – Linnaeus, Buffon, Cuvier and Geoffroy St.Hilaire –, based on study of secondary literature and a number of published sources, the author attempts to improve our understanding of this episode by applying two different perspectives.

The first one has been borrowed from Michel Foucault's *Les mots et les choses*. According to Foucault, classification systems of plants and animals can be interpreted as manifestations of larger mental frameworks, called 'epistemes', which dominate the entire way of thinking and speaking about the world during a particular period of history. Transitions from one system to another are the result of changes of epistemes. As transformations in these underlying forms of thought are by nature sudden and radical, the development in classification systems is punctured by discontinuities rather than by gradual, minor adjustments.

Stemerding agrees with Foucault in so far as the description of the rules of classifications is concerned. Whereas classification systems elaborated in the 18th century were all based on the selection of a few visible parts of plants and animals, those developed by Cuvier and other French naturalists around 1800 first and foremost rested on the comparative analysis of the internal parts of living beings. But the development of the systems themselves, Stemerding claims, can only be understood if one adopts another perspective. This is the approach proposed by Bruno Latour.[6] The author argues that both the emergence of classification systems in the 18th century and their transformation around 1800 can be adequately explained, as Latour suggested, by examining how naturalists attempted to enhance the credibility of their statements on the natural world by tying them to an increased number of other statements, things and phenomena. The 'asymmetry' which arose between different systems of classification depended on the relative strength of the 'networks' the 'actors' managed to construct. From a Latourian point of view, there is thus no need to reduce these systems to larger mental frameworks. Foucaultian epistemes arose out of Latourian networks. Cognitive phenomena, in short, can be explained in sociological terms.

Wiebe Bijker's aim is a more ambitious one. He sets out to formulate a new general theory on the development of technical artefacts. In his view, such a theory should satisfy five criteria (pp. 20-23): 1) to be able to account for change, 2) to show how and why constancy can exist, 3) to conceive the 'working' and 'non-working' of artefacts as *explananda* rather than *explanantia*, 4) to encompass both the strategies of actors and the structures by which they are constrained, and 5) to avoid any a priori choice concerning the character of actor's activities; 'social', 'technical' or 'scientific' aspects should not be distinguished in advance. None of the existing studies on technology, whether in history, economics, philosophy or sociology, is said to meet all these requirements.

To achieve his goal, Bijker proceeds by moving back and forth between theoretical reflection and descriptive accounts of three cases selected from the history of technology, based on secondary literature and published sources. The three cases presented are the development of the bicycle, Bakelite and fluorescent lighting.[7] Bijker claims the spectrum to be broad enough in terms of period of time, engineering discipline, industrial context, intended market and pro-

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[6] Stemerding has used both Latour's *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes, 1987) and a number of his articles.

[7] Parts of the first two case-studies have earlier been published in W.E. Bijker, Th.P. Hughes and T.J. Pinch ed., *The Social Construction of Technological Systems. New Directions in the Sociology and History of Technology* (Cambridge Mass., 1987), pp. 17-50, 159-187.

cess/product character to provide a sound empirical base for general statements. The history of the bicycle is used to develop the base of the general theory, a descriptive model for the analysis of technical change. In fact, never has this innocent-looking vehicle been the object of so much learned reflection since Sergeant Pluck long ago pondered on the atomic structures of men and bicycles in Flann O'Brien's *The Third Policeman*. The first element of the model consists in identifying all groups of people who constitute the social environment in which the development of particular artefacts occurs, called the 'relevant social groups', by applying the basic methodological rule proposed by Bruno Latour: To start from a limited number of actors and then chart their interrelations with other actors. Next, Bijker suggests to focus on the problems with artefacts as seen by the relevant social groups, as well as the solutions proposed, in order to discover the different meanings attributed to artefacts by each group involved. The result of the analysis is used to prove the validity of the fundamental tenet of the symmetrical approach in the study of technology: That the 'working' and 'non-working' of artefacts are socially constructed rather than determined by some 'objective' criterion independent from time and culture (pp. 82-85). But this 'interpretative flexibility' of artefacts, Bijker argues, diminishes as consensus emerges between the relevant social groups about the dominant meaning of the artefacts at issue. The reduction in the spectrum of meanings is attended with a decrease in ambiguity of the meaning that eventually prevailed. 'Closure' is allied with an increased degree of 'stabilization'. Thus, symmetry passes into asymmetry.

After repeating the same series of steps in the case of Bakelite, the author expands his theory with several additional concepts to explain technical change. The notion of 'technological frame', which comprises all concepts, criteria and techniques employed by a community in identifying and solving problems with artefacts, is introduced to clarify how existing technology structures the ways of seeing of relevant social groups and how relevant social groups in turn structure the design of artefacts. Technological frames embody constancy. They crystallize from interactions between social groups, mirroring the stabilization process of artefacts (pp. 119, 122-123, 175-176). Change, on the other hand, is accounted for by referring to the varying degrees of 'inclusion' of individual actors in technological frames, and by the possibility of actors being included in various frames at the same time. The range and nature of new technical solutions proposed by individual actors, is assumed to be dependent on the particular 'configurations' formed between different technological frames.

In the chapter on fluorescent lighting, finally, Bijker shows how these concepts can be employed to avoid any a priori distinctions between the 'social' and 'technical' world, as had been required by the last criterion proposed at the outset. He concludes that all requirements for a general theory on technical change have indeed been met.

Among the three studies at issue, the one by Boelie Elzen is the most historical in the conventional sense. The major part of his book is devoted to a detailed story on the early development of ultracentrifuges used in biochemical research, including careful descriptions of the instruments themselves. The account covers the whole period between around 1920 and 1950. It is based on extensive, pioneering research in published and unpublished sources as well as on correspondence and interviews with a number of scientists from Sweden and the U.S., who have been closely involved in the development or early use of ultracentrifuges.

But the descriptive account is closely related to the new approaches in the study of science and technology exemplified by the studies of Bijker and Stemerding.[8] To begin with, Elzen uses the network-perspective propagated by Latour and the social construction of technology-approach proposed by Bijker (in short, SCOT) as 'sensitizers' to organize his historical data. He starts in the Latourian vein with identifying key actors and then following their activities in the development of artefacts. The careers of four of them are studied in depth: those of Theodor Svedberg, the colloid chemist from Sweden who first applied ultracentrifuges in chemical research, James W. McBain, a fellow chemist from the U.S., who tried to develop an easier and cheaper alternative to Svedberg's device but discontinued his work due to lack of support, and two American physicists, Jesse W. Beams and Edward G. Pickels, who succeeded in constructing new types of ultracentrifuges which could be used for a whole variety of purposes. After describing how Pickels, joined with a businessman from California in a firm named Spinco, ultimately managed to start production of ultracentrifuges on a commercial basis, the author rounds off his descriptive account with a brief account of the further development of these instruments up to the 1980s. As the SCOT-approach suggests, Elzen focuses throughout his story on the problems perceived by 'his' actors, the variety of solutions proposed, the interpretative flexibility of the artefacts involved and the ways in which these were eventually 'stabilized'.

Moreover, in his final chapter the author employs the rich amount of empirical data to refine current general conceptions on the development of artefacts. By comparing evidence on each of the different artefacts described, Elzen infers that the primary source of technical development should be sought in the 'definition of the situation' used by *individual* actors. Technical change begins when single actors try to solve a perceived problem in their situation by creating new artefacts, or modifying certain characteristics of existing ones. It

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[8] A third approach discussed by Elzen in his opening chapter, the 'systems approach' developed by Thomas Hughes, plays only a minor part in the rest of his study.

proceeds by continued adaptation both of the definition of the situation and of the artefacts involved. As this 'definition-artefact spiral' comes in full swing, some aspects of the artefacts may get 'hardened', while others remain subject to further modification.

But the chances of survival of artefacts, the author argues, are 'co-determined' by the way in which they are embedded in interactions with other actors. The chances are highest if artefacts themselves become objects of interactions. Thus, they may get 'stabilized' at a collective level in the sense that 'characteristics ... become normal for the actors and are not easily questioned again' (p. 403, 417). The interaction patterns, in turn, can further be analyzed in terms of the SCOT- and network-approaches. And so, Elzen concludes, the whole gamut of concepts makes a perfect fit.

All three studies demonstrate once more the importance of applying sociological concepts and theories to enhance our insight into the development of science and technology. To begin with, they prove the usefulness of these tools as sensitizing or ordering devices in the research on the particular cases at hand. On top of that, they provide precious contributions to the building of a general framework for the study of science and technology that takes both 'symmetry' and 'asymmetry' between knowledge claims into account. The relevance of these studies thus goes far beyond the selected fields of inquiry from which their empirical data are drawn. Those who (for whatever reason) object to sociological theorizing on science and technology will only disserve themselves by ignoring these fine products from the Twente 'school'.

They are not without flaws, however. My reservations concern the range of the actor's perspective employed by all three authors. Stemerding almost seems to ignore any shortcomings in the Latourian approach. In fact, the network-perspective borrowed from Latour does by no means always explain why some classification systems in natural history were sooner or later accepted, while others were wholly or partly rejected. For instance: Why was the Linnaean system in the 18th century more successful than any of those proposed by other naturalists at the time, Buffon's included? The claim that 'many naturalists eagerly accepted Linnaeus's work because of his uniform and concise definitions of genera and species' (p. 105) begs the very question it purports to answer. In the next sentence the author declares that 'at the same time ... the foundations of his system were often criticised, notably in France'. Why was the uniformity and conciseness of the system for many naturalists apparently more decisive than the weakness of its basis? The attractiveness of the Linnaean network of 'statements, things and phenomena' can not be explained by a linkage with some support-network of powerful scientific institutions – not from the outset, at least. The conquest of the seats of power turns out to have been the result rather the cause of the spread of the Linnaean system throughout French society (pp. 112-113). The initial appeal was due to external factors, i.e. societal factors independent from the strength of the network itself.

Moreover, Stemerding underrates the scope of the argument advanced by his other protagonist, Michel Foucault. According to Foucault, the range of epistemes extends far beyond the field of natural history. The structure of thought that in a particular period governs the practice of natural history, also dominates other fields of inquiry, such as linguistics or economics. At a deeper level of analysis, Foucault claims, all of them show a striking similarity in ways of thinking and speaking about the world. Although the Latourian perspective adopted by Stemerding may indeed enhance our understanding of transformations inside natural history, it does not yet disprove the need for mental explanations, nor the existence of great discontinuities in thought, as long as it fails to account for this synchronism of stability or change in different fields of inquiry. Unless, of course, one also succeeds in removing the cornerstone of Foucault's model: The thesis of similarity. But this was not the aim of Stemerding's book.

Having read Bijker's study, one is likewise left with the feeling of witnessing an unfinished *tour de force*. In so far as it has been intended as a descriptive model, his theory of technical development seems to me extremely valuable. The rules and concepts proposed are very helpful to analyze empirical evidence on technological change. I think Bijker is correct in claiming that they can be applied in other historical contexts than those from which they originally have been deduced. However, does the theory offer us also a general explanation for the development of technical artefacts, as Bijker appears to be saying in his final chapter? I doubt it. What the author spells out on page 178 and 179, are the various ways and situations in which technical change may occur. His scheme has surely the merit of being sensitive both to actors' strategies and to the larger structures by which their actions are constrained. But it does not answer the question that readily springs to mind when examining Bijker's account of the 'closure' and 'stabilization' of artefacts: Why do some 'relevant social groups' prove to be more successful than others in getting their 'meanings' of artefacts accepted? Or to put it more concisely: What are the factors that determine who wins or who loses? Any reference to 'rhetorics' employed by actors begs the question why these rhetorics could have been convincing. To solve the issue, one needs a more comprehensive analysis into the wider societal context of technological change than Bijker actually provides, even though he shows himself fully prepared to undertake the task (pp. 188-189).

In the theoretical part of his book Boelie Elzen, too, proves to be more concerned with 'how'- than 'why'-questions. Again, the concepts, distinctions and definitions introduced are no doubt useful for analyzing both relationships between individual actors and artefacts and the development of artefacts at a more collective level. Elzen's key notions of 'definition of the situation' and 'definition-artefact spiral' help to focus our attention on important aspects of technological change. Still, these are tools for description rather than elements of explanation. In fact, Elzen stops generalizing precisely at the point where society creeps in. After stressing the need of artefacts to be 'embedded in stable patterns of interaction' in order to 'survive' or get 'stabilized', he next turns to making general statements on *how* rather than on *why* such stabilization actually occurs.

The clues for the answer to the last query are only mentioned in passing in his theoretical discussion. The development of ultracentrifuges was in its early stages vitally dependent on the lavish display of private or public patronage. Actors who received liberal support from the Rockefeller Foundation or even from the U.S. government (like Svedberg and Beams) continued to develop their artefacts; the one who was left in the cold (McBain), eventually dropped out. And in the later phase the development proved to be no less determined by market forces. 'If it had not been for Pickels and Spinco' – who started commercial production of ultracentrifuges in the 1940s – 'maybe everybody had forgotten about ultracentrifuges altogether', Elzen states at pp. 415-416. 'Stabilization' of artefacts used in production of knowledge can apparently only be explained if theories are extended to include the wider societal context.

In his short introduction to sociology published in 1970, Norbert Elias distinguished two perspectives in the study of social reality. In the first case, analysis starts from individual actors. Their actions are conceived as being primarily determined by their own goals and motives. In the second case, individual actors are considered as forming part of a larger 'figuration' with other actors. Their actions are seen as being dependent on a particular pattern of relationships with other people. Whereas in the former approach actions are first and foremost examined with regard to the functions they serve *for* individual actors, in the latter they are analysed as function *of* wider networks of interaction. According to Elias, the second perspective should be regarded as an essential complement to the first. It draws attention to unintentional motives of human action.[9]

The predominant approach used in all three studies has more in common with the 'actor's' than the 'figuration' perspective. Stemerding tilts heavily towards the Latourian point of view: Follow the actors. Bijker and Elzen surely prove to be aware of the significance of wider networks of interaction (witness their concepts of 'configuration' and 'patterns of interaction').[10] But they stop

[9] N. Elias, *Was ist Soziologie ?* (München, 1971^2), pp. 75-109, esp. p. 99; cf. also the elaboration on this point by J. Goudsblom, *Balans van de sociologie* (Utrecht/Antwerpen, 1974), pp. 158-161.

[10] Bijker also cites Elias' work on p. 123.

short of drawing the consequence : To explain change in science and technology by including an analysis of the wider societal context. It might be objected that such a reduction is impossible, as the 'social', 'technical' and 'scientific' world are closely intertwined. But society is of course not only constructed by science and technology – not today, and even less so as one moves back in time. Changes in science and technology may be dependent on larger patterns of human relationships that rest on completely different bases (for example religious or ethnic solidarities). Actions may be induced by factors of which actors themselves are never aware.

Thus, the actor's point of view in studies of science and technology should be balanced by the other perspective on social reality sketched by Elias – just as in the study of the history of society at large. Once the broader social context is taken into account, there is again the whole spectrum of explanations to choose from, running from the 'command'- to the 'market'-side. But a variety of tools only fits a job of such complexity: To enhance understanding of the development of science and technology.

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