WORKING PAPER
ALFRED P. SLOAN SCHOOL OF MANAGEMENT

SCIENCE AND INDUSTRY: A THEORY OF NETWORKS AND PARADIGMS

Koenraad Debackere, Bart Clarysse and
Michael A. Rappa

15 February 1994

Sloan WP # 3655-94

MASSACHUSETTS
INSTITUTE OF TECHNOLOGY
50 MEMORIAL DRIVE
CAMBRIDGE, MASSACHUSETTS 02139

Massachusetts Institute of Technology

SCIENCE AND INDUSTRY: A THEORY OF NETWORKS AND PARADIGMS

Koenraad Debackere, Bart Clarysse and
Michael A. Rappa

15 February 1994

Sloan WP # 3655-94

Alfred P. Sloan School of Management
Massachusetts Institute of Technology
50 Memorial Drive, E52-538
Cambridge, MA 02139

Forthcoming in: *Technology Analysis & Strategic Management, Vol. 6, No. 1, 1994*

Science and Industry: A Theory of Networks and Paradigms

KOENRAAD DEBACKERE, BART CLARYSSE,
NACHOEM M. WIJNBERG & MICHAEL A. RAPPA

ABSTRACT *The recent interest in 'network' forms of organization serves as a starting point to better understand the dynamic characteristics of technology development. Network theory allows one to describe the relations between 'actors' involved in the development of new technologies both cross-sectionally and longitudinally and, as a consequence, to model the structural and behavioral development of a community of actors (regardless whether this 'community' is defined as a set of individuals, e.g. the 'scientific' community, or whether it is defined as a collection of organizations, e.g. an industry). In this paper, this network approach is used to develop a theoretical framework to understand the knowledge transition from 'scientific' paradigm status to a 'technological' paradigm status. It is believed that the propositions made in this paper will enable truly empirical studies on the nature of the development of 'scientific' and 'technological' paradigms.*

Introduction

This paper attempts to link science and economics at two different levels. First of all, the relations between the development of a scientific field and the body of knowledge applied in an industrial environment will be studied. Second, we demonstrate how certain concepts and models used to describe industrial change can be successfully applied to scientific change and vice versa. On the one hand, we will focus on the differences between industrial and non-industrial research. On the other hand, we will stress the similarities which appear in the development of a scientific field and an industry. To prevent unnecessary causes for ambiguity and debate we will not speak of science and technology (e.g. Kroes, 1989; Richards, 1987; Weingart, 1978) but of industrial and non-industrial science or research.

Two concepts are crucial to the line of inquiry we will pursue: networks and paradigms. Both scientists and enterprises form 'networks.' Paradigms can be used to describe the developments of these networks. Dosi (1982) introduced the concept of the technological paradigm. In this paper we argue that it may be appropriate to define these technological paradigms as industry-specific. They represent an implicit agreement between producers and consumers/users about the nature of the product or the service to be delivered. Hence, they form the

Koenraad Debackere and Bart Clarysse are with the Vlerick School of Management, University of Gent (Bellevue 6, B-9050 Gent, Belgium. Nachoem Wijnberg is with Erasmus University (Rotterdam, P.O. Box 1738, 3000 DR Rotterdam, The Netherlands). Michael Rappa is with the MIT Sloan School of Management (50 Memorial Drive, Cambridge, Mass. 02139 USA).

basis of technological competition to the producers. Industrial researchers have to take into account the technological paradigm as a 'standard' against which they can benchmark their own efforts.

We further argue that scientific activity is a highly competitive process. Scientists attempt to successfully market their 'products' to consumers who are their peers. Hence, it is attractive to re-apply the concept of a 'scientific' paradigm much in the same way we will apply the 'technological' paradigm to an industry. A scientific paradigm can then be considered the 'standard' scientific product against which all products are measured in a specific scientific field. This happens in the context of a 'scientific community.' A scientific community is nothing else but a specific 'network' of scientists that is comparable to an industry. Within this network, certain norms prevail as to what is a valuable 'scientific' product or not.

Networks and Paradigms: A Review of the Literature

Network theory is useful to describe relations between 'actors' and to quantify positions of actors in a network. In this way, the structure and the development of a group of such actors can be studied both from a qualitative and a quantitative perspective. Network concepts were developed in sociology but their application to economic problems has become increasingly popular (e.g. Burt, 1992; Freeman and Barley, 1990; Hagedoorn and Schakenraad, 1992; Jarillo, 1988). Burt's notion of social capital is highly relevant in this respect:

> "Financial and human capital are distinct in two ways from social capital. First, they are the property of individuals. (...) Second, they concern the investment term in the market production equation. (...) Social capital is different on both counts. First, it is a thing owned jointly by the parties to a relationship. No one player has exclusive ownership rights to social capital. If you or your partner in a relationship withdraws, the connection, with whatever social capital it contained, dissolves. If a firm treats a cluster of customers poorly and they leave, the social capital represented by the firm-cluster relationship is lost. Second, social capital concerns the rate of return in the market production equation. Through relations with colleagues, friends, and clients come the opportunities to transform financial and human capital into profit. Hence, social capital is the final arbiter of competitive success." (Burt, 1992: 9)

Turning to the development of scientific disciplines, sociologists have found that scientists working in the same field take notice of each other's work and thereby start forming a professional network which often has been referred to as the 'scientific community' (Cole, 1992; Crane, 1972; Hagström, 1965; Price, 1963). Given the fading boundaries between what traditionally has been called 'scientific activity' and 'technological activity,' Rappa and Debackere (1992) have proposed that the behavior of academic researchers and industrial researchers should be studied at a more holistic level, i.e. the level of the 'technological community' also referred to as the 'R&D community.' This community consists of the scientists and engineers who are working towards the solution of an interrelated problem-set, who are dispersed across both private and public sector organisations, but who nevertheless communicate the results of their work to each other, and hence, participate in community 'networks.' Within these networks, information and knowledge flow rather freely.

Sociologists consider these information and knowledge flows as 'gifts' that get exchanged within a community of practitioners sharing a similar set of scientific and technological problems. This gift and reward exchange process has been identified as one of the primary mechanisms enabling the way in which science functions (e.g. Hagström, 1965; Merton, 1973; Ziman, 1984). Latour and Woolgar (1982) extend this notion of gift-giving. According to their model, scientists are investors of credibility and they invest their credibility where it is likely to be most rewarding. This quest for credibility further implies that there exist, within a particular community, norms and standards as to what is considered relevant or legitimate 'research' and what is not. Law (1976) therefore distinguishes three generic types of scientific specialties. *Technique- or methods-based specialties* occur when the scientists' solidarity rests on the basis of shared gadgetry and its development. *Theory-based specialties* are defined in terms of a mutually shared formalism. *Subject-matter specialties* have as members those researchers who work on a particular subject matter or problem.

As far as theory-based and methods-based specialties are concerned, clear opinions exist within the respective scientific communities as to what is legitimate work or not. This is, however, less evident for subject-matter specialties. For instance, if one takes the case of neural network research, which clearly is a subject-matter specialty, the solidarity among neural network researchers is described as follows:

> "Neural network scientists are talking about modeling the human brain and its parts, about gaining an understanding of how the brain works. Others in the field are talking about building new and unusual forms of computers having brain-like capability and being constructed of brain-like parts." (DARPA, 1988)

Within the context of subject-matter specialties, the solidarity among the researchers in the specialty is 'organic,' with social factors influencing community cohesion gaining in importance. More in general, the continuous quest for credibility leads to a constant battle of ideas among practitioners of science. As a consequence, scientific activity is not only marked by gift exchange and cooperation, though also by (fierce) competition. Illustrations of this dual nature of scientific activity abound: Watson's account on the discovery of the double helix structure of DNA (Watson, 1980), Alfred Wegener's theory of continental drift (Giere, 1988), the 'battle' between pheneticists and cladists in systematic zoology (Hull, 1988), or the competition for 'Nobel Prize Results' among high-energy physicists as described by Traweek (1988). Mitroff (1974), in his study of the Apollo project, pointed to the:

> "often fierce, sometimes bitter, competitive races for discovery and the intense emotions which permeate the doing of science."

Contrary to scientific activity, industries are classified for purposes of national statistics. Scientific specialty classifications, as discussed previously, are input-oriented. Industrial classifications, on the contrary, are mostly output-oriented. According to the majority of theorists in industrial economics, these classifications are highly imperfect (Wijnberg, 1989). In theory, the industry consists of those enterprises who are in competition (Boyer, 1984) and who form a network of structurally equivalent actors (Burt, 1987). As will be discussed further below, many similarities exist between networks of scientists and networks of enterprises. Moreover, it will be argued that different types of networks, as they evolve as the technology matures from a body of knowledge rooted in scientific theory and technological practice ('input-orientation') to commercial 'hardware' ('output-orientation') can be used to illustrate the paradigm-shift which occurs during this process.

The modern popularity of the term paradigm started with Kuhn's alternative (1970) to strict Popperian falsification in which theories develop gradually and steadily into better theories. Kuhn considered the growth of science to consist of relatively long periods of 'normal science,' operating within the confines of a specific 'scientific paradigm,' alternating with 'scientific revolutions,' propelling new paradigms to the forefront. However, Kuhn's use of the paradigm concept was, at least, ambiguous. Its contents ranged from actual experiments, theories or artifacts serving as guidelines to all scientists in a certain field to a complete set of search heuristics to identify problems, to guide problem-solving, and to express and to evaluate results.

Economists explicitly invoked Kuhnian ideas to describe technological progress. Rosenberg (1976) has introduced the concept of *focusing devices*, Sahal (1985) has talked about *technological guideposts*, while Nelson and Winter (1980 & 1982) have proposed the notion of *natural trajectories* and *search heuristics*.

Dosi (1982) was the first to introduce the concept of the *technological paradigm*, defining it as:

> "(...) a 'pattern' of solution of selected techno-economic problems based on highly selected principles derived from the natural sciences, jointly with specific rules aimed at acquiring new knowledge and safeguard it, whenever possible, against rapid diffusion to the competitors. (...) A technological paradigm is both an *exemplar* — an artifact that is to be developed and improved — and a set of *heuristics*...' (Dosi, 1988: 1127).

Dosi went on defining a *technological trajectory* as "*...the activity of technological progress along the economic and technological trade-offs defined by a paradigm*" (Dosi, 1988: 1128). However, using this perspective, two interrelated problems appear when speaking of technological paradigms and trajectories: scope and content.

Dosi's paradigms are technology-specific and often, but certainly not always, industry-specific. Furthermore, more than one paradigm may be relevant to a specific industry. Freeman and Perez (1988) use the term techno-economic paradigm (TEP) to describe patterns common to all industry in a long-wave period. Andersen (1991) defined micro-scopic (to distinguish them from Freeman and Perez's macro-scopic TEPs) techno-economic paradigms as "*...a mutually agreed definition of the producer-user interface which partly takes the form of specifications of the commodities to be delivered*" (1991: 119). This point of view comes very close to an industry-specific definition. Saviotti and Metcalve (1984) considered products to be a combination of three sets of characteristics: the technical features of the product, the services performed by the product, and the methods of production. These sets also describe the outer boundaries of a competitive process localized in a specific industry. If a paradigm is attributed to a specific network, the structurally equivalent actors of this network are the enterprises in a specific industry.

Therefore, it seems to be an appropriate choice to define a technological paradigm as industry-specific, at the same time representing an implicit agreement between producers and consumers/users about the nature of the good or service to be delivered and forming the basis of (technological) competition to the producers. A scientist working for an enterprise has to take into account the technological paradigm as a standard against which to compare the results of his own efforts. In this way, the concept can be used much more effectively to describe the nature of competition and the course of technological development in specific industries. A new paradigm means a new industry and vice versa, even though the exact identification of both may take time.

As in the case of Kuhn's paradigma, the content of the concept 'technological paradigm' can vary in scope. The first part of the definition by Dosi primarily focuses on heuristics, giving priority to certain problems and looking in certain directions for solutions. The second part of the definition explicitly combines heuristics and exemplar. However, his definition of the technological trajectory seems to be much easier to understand if only the paradigm is understood as an exemplar. Indeed, Dosi's exemplars resemble ideal forms of products, *the* internal combustion engine, *the* semiconductor chip. Also, Dosi defines his paradigms-as-exemplars only in terms of narrow technical characteristics.

If one takes users/consumers into account, it seems reasonable not to consider the paradigm-as-exemplar as an ideal form but as a 'standard' that is appreciated by consumers, the 'average' specimen of a specific class of products at a certain moment in time. This 'average' specimen does not have to exist in reality. For example, the 'average' car in the minds of car-buyers would be like the common denominator of several middle-class cars, like a Ford, an Opel, a Toyota, etc. An expensive or fast car would be more expensive or faster than *that* car, a cheap or slow car would be cheaper or slower.

Consumers are not interested in the way producers, and the industrial researchers in particular, go about their business as long as they deliver a product with the right characteristics at the right moment and, in some cases, perform suitable after-sales services. Therefore, in the definition of the technological paradigm proposed here, there is no room for the way in which enterprises innovate and produce.

On the other hand, the meaning of the concept should not be restricted to narrow-technical characteristics. The characteristics that are relevant to the consumer should be the relevant dimensions of product-space; including price, after-sales services, and even purely psychological effects, as may for instance be provided by a persuasive advertising campaign.

This fits well with an industry-specific interpretation of the concept of the technological paradigm. The paradigm then becomes the cluster of characteristics which represents the 'average' offering of the industry at a certain point in time. The position of the product an individual enterprise offers on the market can then be described in terms of the distance to the paradigm along all relevant dimensions of product-space. The paradigm is not a constant during the lifecycle of the industry but changes continually. The pattern of those changes constitutes the technological trajectory. The technological trajectory represents technological change over time as observed by the only competent judges in the process of competition: the consumers.

The concept of the paradigm, as it has been reshaped here, may be usefully transferred back to the study of science. As discussed previously, a number of recent attempts have aimed at describing science in economic terms. Of course, not everything about science can be explained in a purely economic way. Human curiosity for example, is not to be explained by economic theory. However, although economics cannot exhaustively explain why people have inventive ideas, it can tell a lot about why and how innovations are generated. Radnitzky (1989) attempted to apply cost-benefit analysis to scientific methodology. Callon, Law and Rip (1986: 9) stated: *"The behavior of the scientists studied [in recent studies of laboratory science] conforms in every way to the classical picture of the entrepreneur."* Latour and Woolgar (1982) (and also: Latour, 1987) explained the behaviour of scientists, their 'methods' as well as their interactions with other scientists, as an effect of their desire to successfully invest in credibility. Hull (1988) also depicts scientists as competing for 'credit.' Scientists and their publications are the interactors, in Hull's terminology, in the case of conceptual replication.

A successful scientist is one whose work is used and acknowledged by others, he writes papers that are accepted by journals and that are quoted in papers by others. But not, of course, 'any' others. The consumers of a scientific product are other scientists and especially other scientists in the same (sub-)discipline. They are the editors and referees of scientific journals, they are hypothesized to judge the quality of their peers' efforts by quoting them or not, while specialist commissions advise about the award of grants and subsidies among members of the scientific community. Fellow scientists thus have to assess the 'objective' value of a specific scientific product. But, as the last century of methodological research has shown, this is a far from simple matter. It may even be impossible in principle.

Amongst other problems, there is Quine's argument that no theoretical statement can be truly tested as in a vacuum. Meaning can only be ascribed to the statement as an inseparable part of a larger theory or complex of theories (see also Law, 1976).

However, it has taken economics a long time to realize that there is no unambiguous method to assess the value of any products. After centuries of debate, the marginalist school at last discovered what is still taught to every undergraduate: that the value of a product is a function of consumer preferences and cannot be discovered in a vacuum. The economist cannot explain consumer preferences, he can only observe their effects, e.g. in the form of price-elasticities.

If science is understood as a competitive process in which scientists attempt to successfully 'market' scientific products to consumers that are their fellow-scientists, it seems attractive to re-apply the concept of paradigm to science in the same way we applied it to industries and technology. The paradigm should be considered the 'standard' scientific product against which all products are measured in a specific scientific field, i.e. *in a specific network of scientists that is comparable to an industry.* An acceptable paper has to conform to certain standards with regard to mathematical rigor, statistical analysis, the nature of acceptable qualitative proof, thoroughness of the review of prior literature, etc. Also, there are multiple requirements that are determined by the specific subject-matter.

If you test a new anti-cancer drug on rhinoceroses your paper about the experiment will have a smaller chance of acceptance than if you had stuck with mice. All of these requirements are nothing else than the expression of the 'consumer preferences' by the 'consumers' of science. They change over time, they differ from field to field and from journal to journal. It is still unusual to apply marketing techniques to scientists' behavior to detect their preferences but, most scientists can give you examples of what they consider perfectly 'average' papers in their field. These papers 'define' the current paradigm and its most salient characteristics can be considered the relevant dimensions of product space. A paper that scores less with regard to the most important characteristics, *in the eyes of its consumers*, is not or only barely acceptable in journals and will be seldom quoted. A paper that scores better will have a much higher probability to be published and quoted. The scientific paradigm will gradually evolve over time and its normal progress could be called the scientific trajectory, in analogy with the technological trajectory.

A radically innovative scientific discovery may have the effect of so dramatically altering consumer preferences that the dimensions of the product space change. This is what happens during a Kuhnian revolution. However, as Lakatos and others have argued, competing paradigms or research programmes may continue to exist along each other for a long time.

The concept of the paradigm has now been transplanted from the theory of science to the theory of industrial economics and technology. We have redefined the concept to make it more operational and consistent with economic theory. Science too can be understood as a process of competition for scarce resources, in other words, it can be defined as an economic process. The redefined 'economic' concept of the paradigm may again be usefully applied to the theory of science.

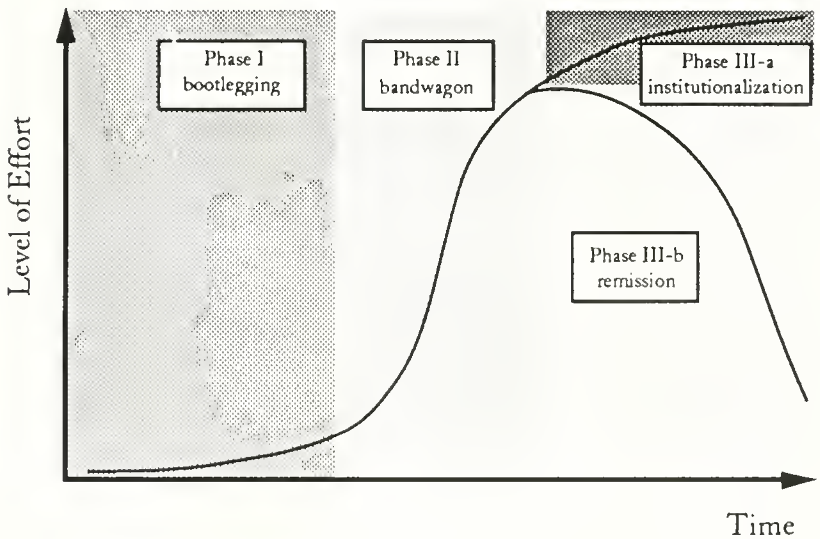
Networks and Paradigms: How to Apply them to the Study of Science and Industry

The previous section has emphasized the similarities between science and industrial technology. Their evolution can be expressed in terms of networks and paradigms. Both are involved in a selection process in which the ultimate 'judges' are the consumers. Here appears, however, an important difference: the producers of science are also its principal consumers. While different technological solutions in industry finally compete in a market, science is to a certain extent a process of auto-selection operating through the peer-review process. One might compare this to resemble, though not completely, the difference between natural selection (in a Wallacean sense: selection of those who are best adapted to their environment) and sexual selection where animals of the same species but of different sex decide who may transfer his genetic material to the next generation. These differences between the selection processes may lead to important consequences.

To show this, a brief sketch of a general life-cycle model of the development of a R&D community may suffice. This model in fact consists of two life-cycles: that of the R&D community *sensu stricto* and that of the industry employing industrial researchers to make use of the specific field of scientific knowledge (on the industrial life-cycle, see for instance Abernathy, 1978 or de Jong, 1988). The development of industrial research is mainly a function of the development of the industry while the development of non-industrial science is much more autonomous.

If we abstract from the unlikely case of a pure demand-pull innovation in a newly-formed industry, we can begin at the start of the life-cycle of the R&D community. The phases are diagrammed in Figure 1 in terms of changes in the level of effort within the field. The first phase is characterised by a relatively low level of effort. During this period, which may last for a long time, a handful of researchers dedicate themselves to furthering the field, even though their enthusiasm may not be shared by their peers, and indeed, may be severely criticized. Typically, they have difficulty securing adequate funding (hence the name 'bootlegging,' which implies that researchers struggle to maintain their research without formal recognition or funding to underwrite the cost of their work.) The recruits to this new community, the so-called pioneers, share a large number of psychological and sociological characteristics, whether they are employed in industry or at universities (Debackere and Rappa, 1993). Typically, a few isolated individuals start working on similar problems with roughly similar ideas.

Figure 1. A Life-Cycle Model of R&D Communities



Researchers dedicated to the new and unorthodox field of inquiry are confronted with a difficult dilemma. On the one hand, they need more proof that their work will yield results before receiving resources. On the other hand, without resources, they are unable to do precisely that. 'Bootlegging' enables fledgling research to go forward without the full knowledge and scrutiny of managers and other researchers, up to a point at which the promise of the idea is clear. During this phase then, the community will be highly concentrated among a small number of organizations and the yearly increase in number of researchers is rather moderate.

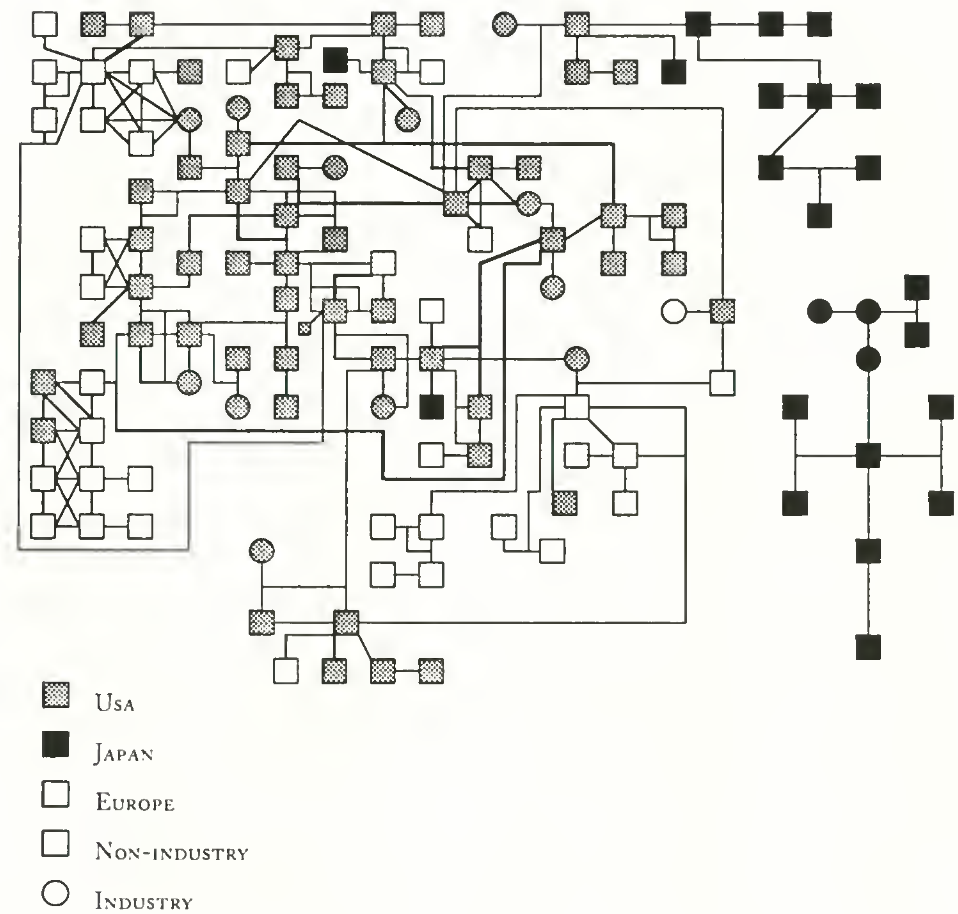
As the number of individuals working on the same problem area increases, though, a communication network emerges with ties which are much stronger than the ties binding the individuals to the organizations they formally belong to. During the second phase of the community lifecycle, a very rapid increase occurs in the number of researchers working in the community in a relatively short period of time. This phenomenon is sometimes referred to as a 'bandwagon,' hence the name for this phase (Barber, 1990; Crane, 1969). As the community grows, a new paradigm comes into being which is seen as competing with an older paradigm by the higher-level network of the (sub-)discipline. The community tries to organize congresses and found journals to be able to steer the selection process (see for instance Hull, 1988, for well-documented examples). The R&D community typically becomes more widely distributed across organisations, sectors, and countries. If the work of the new community seems interesting from a commercial point of view, some scientists may be recruited by enterprises, some who already work within industry are allowed to openly devote their efforts to the new field, finally some scientists may decide to become entrepreneurs themselves (here we refer to the phenomenon of New Technology-Based Firms).

Thus, the rapid growth and diffusion of the community during the 'bandwagon phase' likely has a beneficial effect on the commercialization of the technology. More and more researchers, employed in an expanding array of organizations, should almost certainly contribute further to advances already made. As the community expands and spreads, it develops a powerful momentum that derives from the force of its numbers and the ingenuity of researchers working independently in laboratories around the globe. However, the advantages of this expansion come at a cost: the diminishing ability of researchers to easily communicate with one another. This is a heavy cost, indeed, since it is the communication of information and knowledge among researchers that holds the community together. As a community grows in size, it quickly becomes a virtual impossibility for any given researcher to frequently communicate information to all others without some degree of coordination or structure.

Some organization has to settle within the community in order to hold it together. One formal means of communication is through the published literature. However, this is a very rudimentary communication mechanism with severe limitation given the constant flow of new, complex information abounding from laboratories. Instead another mechanism is needed: the informal communication network or the grapevine. In Figure 2, we show a visualization of the 'grapevine' as it has developed over the last two decades in the field of neural networks. It is obvious that the cohesion of a similar network requires the presence of a shared formalism, method, or subject-matter among its adherents. Thus, there has to be an 'average' scientific product against which to judge the contributions of the numerous community members.

The network shown in Figure 2 is based on the analysis of co-authorship data for 2,740 articles published in the field of neural networks over the period 1969 till 1989. A careful analysis of the published literature is indeed believed to offer valuable insights into the growth and emergence of new scientific and technological disciplines (Rappa and Debackere, 1992). Even if co-authorship data may appear an overly rigid criterion to study networks among researchers within a particular community, the results in Figure 2 show that, at least, they enable a rather detailed first-order insight into the development of the collaboration structure within a particular field.

Figure 2. The Informal Network ('Grapevine') in the Field of Neural Networks, 1969-1988)



During the 'bandwagon phase,' scientific and technological development has important economic consequences. A new industry comes into being or an old industry may restart its life-cycle. In any case, a new technological paradigm has been created. Usually, consumer preferences in the market are not yet sufficiently clear to allow enterprises to give very strict 'guidelines' to the researchers in their employ. Using Perrow's terminology (1974), the degree of analyzability of the technology is still low, and, the variability of possible problem-solving approaches remains high. As a consequence, the network of researchers remains intact and transcends organizational boundaries. The scientific paradigm remains the standard of excellence for industrial and non-industrial researchers. Open and speedy communication remains the norm, even though researchers may at the same time strive to obtain property rights to their ideas (patents etc.).
However, as the (successful) industry grows further, consumer preferences 'crystallize,' many innovative small companies have failed or have banded together to achieve scale advantages in production, marketing, *and research*. This is the point where the selection processes for industrial and non-industrial research start to divide. The enterprises which have survived the first phase of industrial growth have a much clearer view of their position in product space and of the R&D needs to strengthen this position. Also, there can appear so-called 'strategic groups:' clusters of enterprises with similar positions in product space which are shielded from competitors in the same industry by 'mobility barriers' (Caves and Porter, 1977; McGee and Thomas, 1986; Mascarenhas and Aaker, 1989).

This is also implied in the life-cycle model. As the bandwagon progresses, the community enters the third phase, where one of two paths will emerge: (a) researchers continue to make progress in solving the problems confronting them, allowing the community to institutionalize itself, or (b) progress begins to slow down such that researchers become discouraged, forcing the community to contract and perhaps eventually return to the conditions prevailing in the first phase.

One effect of this life-cycle model is that the original R&D community is being fragmented at the very moment of its expansion phase. The researchers in industry are obliged to let their work be dominated by the technological paradigm, not the scientific paradigm. They collectively stop forming an integral part of the scientific network, although, of course, individual researchers, often the original pioneers, in industry may still remain inside the scientific network too. For the researchers in industry, the norms and requirements of their own organizations predominate over the norms and requirements of the network of researchers. This in itself can be a factor contributing to the height of mobility barriers because if a group of enterprises forms a cluster in product space and their researchers become much less communicative with fellow-researchers, it becomes much more difficult for another enterprise to join the cluster.

If the new scientific paradigm is successful, 'consumer preferences' in the new field will become clearer. Editors and referees will have stricter ideas about what they want a paper to look like. The process of auto-selection fully comes into being. Also, the new paradigm, if legitimized and sustained, will attract many new researchers and, analogous to industrial development, groups of researchers will increasingly tend to cluster in specific parts of product space and strategic groups can form. The most easily observed aspect of this development is the proliferation of scientific journals in the new field, each with their own specific interests and 'quality' requirements. This process leads to further specialization and, ultimately, sterility. However, this may be prevented or at least postponed by new stimuli coming from the industry which is in a phase of maturity and which is increasingly dominated by demand-pull innovation. Although the R&D community of *all* researchers in a specific field has long split up in a mature scientific community, possibly consisting of many scientific strategic groups, and groups of researchers walled in by their respective organizations, and possibly strategic groups in a mature industry, the long-term vitality of both successor-parts is still very much linked.

Conclusion

In this paper, we have presented a theoretical life-cycle model to study the development of new technologies. This life-cycle model was then used to operationalize the concept of a 'scientific' paradigm and its shift towards a 'technological' paradigm. Conceptualizing a paradigm as an 'average' scientific or technological product judged in the market-place for 'ideas' (i.e. scientific community) or 'products' (i.e. industrial community) is believed to offer an interesting avenue to start studying the paradigm-shift from a more empirical perspective. This research agenda is now being vigorously pursued.

At the heart of the whole model then is the assumption that technology is, in essence, a body of knowledge. This idea has gained acceptance among scholars in several disciplines (Layton (1974), Constant (1980), and Latour (1987)). Even though the ultimate goal may be to produce something (the amazing amount of new ventures in newly emerging fields of technology stands witness to this), the currency of R&D communities is not so much actual things as it is the ideas, or theories, about how and why things work the way they do. Therefore, technological development can be understood as an intellectual process that evolves over time, whereby new knowledge is created and applied in order to construct a new product or process. Of course, new technologies are not developed from scratch but are a combination of newly created knowledge and existing knowledge drawn from other epistemic realms. However, in the early stage of a technology's emergence, this body of knowledge is necessary incomplete. One simply does not know everything one needs to know in order to make the technology work. The areas in which knowledge is lacking can be characteristically viewed as problems. If the technology is to be successfully reduced to practice, then new knowledge, in the form of solutions to the problems, will have to be found.

The central actors in this process are the individual researchers who become dedicated to solving the problems, and it is they who set the process in motion with their efforts to create and apply knowledge. In the course of their work, they perform three basic activities: (a) they produce information, (b) they transform information into knowledge, or in other words, they solve problems, and (c) they communicate information and knowledge to each other. This last activity then is a basic characteristic of the communal behavior that goes on within a R&D community. It is exemplified by the existence of communication grapevines. These grapevines hold together the members of the R&D community all over the world, and are analogous (though not the same as) to the invisible college in science (for instance, see Mullins' description of the Phage Group communication network and its link with the origins of Molecular Biology, 1972).

The grapevines then are informal, but remarkably efficient networks of researchers who facilitate the flow of information among different laboratories. Chances are the researchers are well acquainted with each other, perhaps having previously worked or studied together, or having become friends at a conference while commiserating over the years spent toiling over similar problems. Moreover, the dedicated core of researchers (the vast majority of them being 'bootleggers') are likely to be central nodes in the grapevine.

It is logical to assume that the rate of progress in a technology's emergence is a function of how quickly problems are solved, which, in turn, depends on the amount of information produced, the number of solutions attempted, and the extent to which information and knowledge are communicated among researchers. The more information available to a researcher, the more likely he is to arrive at a useful solution. Moreover, the more diversity in the types of solutions attempted, the more likely that critical solutions will be found. Lastly, communication between researchers enhances the probability of finding useful solutions. The relationship found by Schmookler (1966) between investments and patenting activity certainly pointed in this direction: the more efforts spent on a particular technology, the higher the probability to encounter reverse salients, and the higher the probability that these will ultimately be solved. In a similar manner, Nelson concludes *"there are industry-wide efficiency gains to be had by sharing technology. Everyone would be better off if everyone shared."* (Nelson, 1990).

Borrowing from the economists' profit-maximizing axiom and in the wake of Latour and Woolgar's credibility cycle (1982), it seems reasonable to assume that researchers are rational, in the economic sense that they are motivated by self-interest: that is, they are eager to solve problems because there are rewards for those who do. The researcher's objective is to maximize the amount of knowledge he produces and can lay claim to before other researchers because these claims have potential value.

This behavior is well-understood within scientific communities where publication and peer review serve this end. The localness concept of technology, however, has long prevented a similar view from gaining acceptance with respect to technology development. Recently, however, this view has become increasingly considered as being too constrained. Hughes (1987 & 1989), for instance, clearly demonstrates that technologists do disseminate their knowledge via papers and patents. Dasgupta and David (1987) also imply that patenting can be seen as fulfilling needs similar to publishing, namely staking priority claims before other researchers. According to these scholars, the main distinction lays in the social ethos of both worlds. It is the confrontation of the gift-exchange character of science, in its Mertonian tradition (which fosters publication), and the public good character of technology, in its economic tradition (which fosters patenting).

However, for the purpose of the R&D community model, this distinction does not matter very much. The community, as defined in the previous section, indeed implies a blending of norms of science and norms of business. What really needs revision is the belief that "scientists are highly motivated to publish but not to read, whereas technologists read assiduously but are not motivated to publish" (Price, 1967). This view on information and knowledge dissemination may well be too narrow. In publishing (whether it is a paper or a patent) there is both room for openness and secrecy, just as there is room for the co-existence of cooperation and competition within a R&D community. As Collins describes in his account of the development of the TEA laser, scientists are also prone to secrecy. Seldom does a paper reveal all that is needed to replicate an experiment (Collins, 1982).

The Mertonian norms are ideal-types, and do not necessarily reflect real behavior. In the same sense, publishing a paper or patent from the point of view of an industrialist need not reveal everything. Claims can be laid with only a limited amount of information and knowledge disclosure. Communal behavior does certainly not imply complete openness. Moreover, even an industrial organization can gain multiple benefits from publishing and staking claims. It not only serves its image as a technological leader, thus becoming an acceptable player in the technoscientific arena (for instance Hounshell and Smith (1988) on R&D at Du Pont, and Dickson (1987) on IBM's enhanced image after Bednorz and Müller's Nobel Prize), though it serves commercial purposes as well. For example, in the field of polypropylene catalysis, the leaders in technology development pursue an active publication record in order to convince potential licensees to adopt their catalyst systems (Debackere and Rappa, 1990). Thus, the community allows for openness and secrecy at the same time. It allows for both the individual researcher's objectives and the organization's objectives to be realized, divergent as they may sometimes be.

Moreover, from the researcher's standpoint, it is obvious that the actual value of a claim is contingent upon whether or not all problems necessary for reaching commercialization are resolved and the length of time taken. Expressed in another way, for a given piece of knowledge, it is more valuable in use, and the sooner it is used the more value it will have. A researcher need not produce all of the knowledge required to commercialize a technology, as long as his own knowledge claims are secured. Since his choice is guided by the objective to maximize knowledge claims, the researcher is motivated to participate in the R&D community, within the realm of his disciplinary expertise, in which he believes there is a good probability of finding solutions that yield him valuable claims. If the actual probability is equal to or greater than expected (the task of solving problems is easier than anticipated), then the researcher will likely remain in the field (and the community increases its momentum towards institutionalization); however, if it is lower than expected (solving problems is harder than anticipated), then the researcher might switch to another community with a higher perceived probability of success (and, similar to the stock market, if many researchers decide to invest their time and effort in other areas, a decline of the community may occur).

The switching behavior of a researcher is moderated by the exit barriers which have built up during his activities. The longer he has contributed to a technology's development and the larger the amount of knowledge claims he has accumulated, the more difficult it may become to him to leave the community. His knowledge becomes a sunk cost: the more knowledge specific to a technology the researcher accumulates over time, the less likely he is to exit a community prior to reaping the rewards that come with reaching commercialization. It is clear that in this process, there is room for 'irrational behavior' as well. The behavioral model depicted here should certainly not be seen as overly mechanistic.

Rather, it is an organic process, the researcher's decision to enter or exit a community being not a one-time choice, though being subject to frequent reconsideration based on new information which he gathers through the community grapevine. This will be especially true for those researchers who jumped on the bandwagon. The attractiveness of developing a particular technology changes over time, as more information is produced and researchers re-evaluate the probabilities of successfully solving the problems they face. Its attractiveness also may be influenced by changes in other technologies, and indeed a variety of other events, all of which will be reflected in the researcher's decision.

As mentioned previously, a primary activity of researchers is to produce information and transform it into knowledge. By 'information production' we mean the collection of new data through observation and experimentation. Knowledge is a distinct entity from information in that it allows the researcher to do something (know-how) or explain something (know-why). Having information does not imply either. For instance, a researcher runs an experiment and obtains negative results. He cannot use the information in the practical sense that it enables him to do or explain something — that is, it is not knowledge. Nevertheless, the information may be instructive and eventually prove key in creating new knowledge. Moreover, even if the information is not valuable in that it should be published or patented, it does have value in the sense that one who is aware of it will not waste time discovering it again.

The researcher must first make sense of the information available to him — the cognitive process of transforming information into knowledge — in order to solve problems. The processes of information production and transformation are time-consuming; therefore, any individual researcher can only accomplish a certain amount of effort in a given period of time. The information he produces and the solutions he pursues are an expression of the researcher's own judgment and creativity, although there is room for influence from colleagues in the broader community. Unfortunately, not all attempted solutions will work satisfactorily. The probability that a particular solution will work successfully can only be subjectively determined a priori, and this is likely to change over time as researchers generate new information in attempting to implement solutions. Moreover, the relevance of any bit of information (in that it might contribute to the successful solution of a problem) cannot be determined a priori. For this reason, the value of information is less certain than that of knowledge.

The amount of information and knowledge available to a researcher depends upon how much he can produce himself, or receive in the process of communicating with others. The communication of information and knowledge implies that a researcher can also gather information and knowledge produced by another researcher, and disseminate to others that which he produces or learns in the process. For the most part, information is communicated informally by means of interpersonal communications, whereas knowledge is communicated in the form of documented claims, such as with the submission of papers to refereed journals or patent applications.

Without any doubt, the communication of information among researchers is influenced by the existence of organizational boundaries between researchers and their (and their organization's) economic interests (for instance, see Hounshell and Smith (1988) for a real-life example at Du Pont where both researchers and managers were engaged against each other in a constant battle for openness and secrecy). As described previously, this communication impedance effect becomes acute during the shift from 'scientific' paradigm towards a 'technological' paradigm. Organizational boundaries are important for two reasons: first they give rise to information asymmetries among researchers because they impede the flow of information and increase the cost of information gathering. As a result, organizational boundaries can slow the rate of knowledge production within a community by reducing the amount of information available to each researcher. However, organizational boundaries can also enhance knowledge production to the extent they increase the diversity of problem solutions pursued by the community as a whole, since researchers in different organizations are likely to have different information sets.

To illustrate this point, assume the extreme conditions. In the first case, as the research community grows, all researchers are employed by the same organization. Thus, all researchers are exposed to the same information set, but the diversity of solutions performed is limited by the researchers' mutual influence. In the second case, as the community grows, each researcher is employed in a separate organization. Thus, each researcher is working from a different, limited set of information and performing independent solutions, such that the diversity of attempted solutions is maximized. Simply stated, in one situation each researcher in the community has a wealth of information but a limited number of approaches in solving the problems being confronted; in the other situation, although the community can generate the same amount of information, the amount available to any particular researcher is small and the variety of approaches taken is great.

The communication impedance effect of organizational boundaries is overcome via researchers who act as technological gatekeepers — that is, researchers who tend to communicate with others in different organizations (Allen, 1984). In the wake of the economically rational behavior axiom, the communication of information across boundaries likely occurs as a form of 'know-how trading' (von Hippel, 1988). Furthermore, information is more likely to be the trading object of grapevines than is knowledge, because the value of information is indeterminate and because knowledge (which does have potential value) needs to be formally documented when disclosed in order to secure its value for the inventor.

It is clear then that the model only partially captures all the subtleties of technological development as it might actually unfold. It is, however, not the objective of the R&D community model to enter into the many organizational variables that come into play for the successful management of innovation projects within organizations. Neither does the model attempt to unravel all the economic, societal and cultural variables that come into play within the selection environment of a new technology. Rather, the model tries to complement both approaches through capturing the essential 'internal' dynamics—the functioning of R&D communities—that might become one component of a more comprehensive theory on the emergence of new technologies.

References

Abernathy W.J. & Utterback, J. (1978) "Patterns of Technological Innovation," *Technology Review*, (June-July), p.40-47.

Allen, T.J. (1984) *Managing the Flow of Technology*. Cambridge, Mass.: The MIT Press.

Andersen, E.S. (1991) "Techno-economic Paradigms as Typical Interfaces between Producers and Users," *Journal of Evolutionary Economics*, Vol.1, pp.119-144.

Barber, B. (1990) *Social Studies of Science* . London: Transaction Publishers.

Boyer, K.D. (1984) "Is There a Principle for Defining Industries?" *Southern Economic Journal*, Vol.50, No.3.

Burt, R.S. (1987) "Social Contagion and Innovation: Cohesion versus Structural Equivalence," *American Journal of Sociology*, Vol.92, (May), pp.1287-1335.

Burt, R.S. (1992) *Structural Holes: The Social Structure of Competition*. Cambridge, Mass.: Harvard University Press.

Callon, M., Law, J. & Rip, A. (1986) "How to Study the Force of Science," in M. Callon, J. Law & A. Rip (eds.) *Mapping the Dynamics of Science and Technology*, London, MacMillan, pp.3-19.

Caves, R. & Porter, M. (1977) "From Entry Barriers to Mobility Barriers," *Quarterly Journal of Economics*, (May), pp.241-261.

Clark, K.B. (1985) "The Interaction of Design Hierarchies and Market Concepts in Technological Evolution," *Research Policy*, Vol.14, pp.235-251.

Collins, H.M. (1982) "Tacit Knowledge and Scientific Networks," in Barnes and Edge (eds.) *Science in Context*. Cambridge, Mass.: The MIT Press.

Cole, S. (1992) *Making Science*. Cambridge, Mass.: Harvard University Press.

Constant, E.W. (1980) *The Origins of the Turbojet*. Baltimore: Johns Hopkins University Press.

Crane, D. (1969) "Fashion in Science: Does it Exist?" *Social Problems*, Vol. 16, pp. 433-441.

Crane, D. (1972) *Invisible Colleges: Diffusion of Knowledge in Scientific Communities*. Chicago: The University of Chicago Press.

DARPA. (1988) *Neural Network Study*. Fairfax: AFCEA International Press.

Dasgupta, P. and P. David. (1987) "Information Disclosure and the Economics of Science and Technology," in G.R. Feiwel (ed.) *Arrow and the Ascent of Modern Economic Theory*. New York: New York University Press.

Debackere, K. and M.A. Rappa. (1990) *The Dynamics of R&D Communities: A Comparative Study of Polypropylene and EPDM Rubber Technology*. Paper presented at the TIMS/ORSA Meeting, Las Vegas, May 7-9.

Debackere, K. and M.A. Rappa. (1993) "Institutional Variations in Problem Choice among Pioneering Scientists," *Research Policy (in press)* .

Dickson, D. (1987) "IBM's Zürich Lab is 'Flower' in Europe," *Science*, July 10, pp. 125-126.

Dosi, G. (1982) "Technological Paradigms and Technological Trajectories," *Research Policy*, Vol.11, pp.147-162.

Dosi, G. (1988) "Sources, Procedures, and Microeconomic effects of Innovation," *Journal of Economic Literature*, Vol.26 (September), pp.1120-1171.

Freeman, C. & Perez, C. (1988) "Structural Crises of adjustment: Business Cycles and Investment Behaviour," in G. Dosi et al. (eds.) *Technical Change and Economic Theory*, London: Frances Pinter, pp.38-67.

Freeman, J. and S.R. Barley. (1990) "The Strategic Analysis of Inter-Organizational Relations in Biotechnology," in R. Loveridge and M. Pitt, *The Strategic Management of Technological Innovation*, London: John Wiley and Sons Ltd.

Giere, R.N. (1988) *Explaining Science: A Cognitive Approach*. Chicago: The University of Chicago Press.

Hagedoorn, J. and J. Schakenraad. (1992) "Leading Companies and Networks of Strategic Alliances in Information Technologies," *Research Policy*, Vol. 21, pp. 163-190.

Hagström, W.O. (1965) *The Scientific Community*. New York: Basic Books.

Hounshell, D .A. and J.K. Smith. (1988) *Science and Corporate Strategy: Du Pont R&D, 1902-1980*. Cambridge, UK: Cambridge University Press.

Hughes, T.P. (1987) "The Evolution of Large Technological Systems," in Bijker, Hughes and Pinch (eds.) *The Social Construction of Technological Systems*. Cambridge, Mass. The MIT Press.

Hughes, T.P. (1989) *American Genesis: A Century of Invention and Technological Enthusiasm*. New York: Viking.

Hull, D.L. (1988) *Science as a Process*. Chicago: The University of Chicago Press.

Jarillo, J.C. (1988) "On Strategic Networks," *Strategic Management Journal*, Vol. 9, pp. 31-41.

Jong, H.W. de (1988) "Market Structures in the European Economic Community" in H.W. de Jong (ed.) *The Structure of European Industry*, Dordrecht: Kluwer Academic Publishers, pp.1-39.

Kroes, P. (1989) "Philosophy of Science and the Technological Dimension of Science," in K. Gavroglu, Y. Goudaroulis & P. Nicolacopoulos (eds.), *Imre Lakatos and Theories of Scientific Change*, Dordrecht: Kluwer Academic Publishers, pp.375-382.

Kuhn, T.S. (1970) *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.

Latour, B. and S. Woolgar. (1982) "The Cycle of Credibility" in Barnes and Edge (eds.) *Science in Context*, Cambridge (Mass.): The MIT Press.

Latour, B. (1987) *Science in Action*. Cambridge (Mass.): Harvard University Press.

Law, J. (1976) "The Development of Specialties in Science: The Case of X-ray Protein Crystallography," in G. Lemaine et al. (eds.) *Perspectives on the Emergence of Scientific Disciplines*, The Hague: Mouton.

Layton, E. (1974) "Technology as Knowledge," *Technology and Culture*, Vol. 15, pp. 31-41.

Mascarenhas, B. and D.A. Aaker. (1989) "Mobility Barriers and Strategic Groups," *Strategic Management Journal*, Vol.10, pp.475-485.

McGee, J. and H. Thomas. (1986) "Strategic Groups: Theory, Research and Taxonomy," *Strategic Managment Journal*, Vol. 7, No.2, pp.141-160.

Merton, R.K. (1973) *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: The University of Chicago Press.

Mitroff, I. (1974) "Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists," *American Sociological Review*, Vol. 39, pp. 579-595.

Mullins, N.C. (1972) "The Development of a Scientific Specialty: The Phage Group and the Origins of Molecular Biology," *Minerva*, Vol. 10, pp. 51-82.

Nelson, R.R. (1990) "Capitalism as an Engine of Progress," *Research Policy*, Vol. 19, pp. 193-214.

Perrow, C. (1974) *Bureaucracy, Structure and Technology*. London: Tavistock Publications, pp. 50-91.

Price, D.J. deSolla. (1963) *Little Science, Big Science, ... and Beyond*. New York: Columbia University Press.

Price, deSolla D.J. (1967) "Research on Research," in D.L. Arn (ed.) *Journeys in Science — Small Steps, Great Steps*. Albuquerque: University of New Mexico Press.

Radnitzky, G. (1989) "Falsificationism Looked at from an conomic Point of View," in K. Gavroglu, Y. Goudaroulis and P. Nicolacopoulos, *Imre Lakatos and Theories of Scientific Change*, Dordrecht: Kluwer Academic Publishers, pp. 383-398.

Rappa, M.A. and K. Debackere. (1992) "Technological Communities and the Diffusion of Knowledge," *R & D Management*, Vol.22, No.3, pp.209-220.

Rappa, M.A. and K. Debackere. (1992) "Monitoring Progress in R&D Communities," in P. Weingart, R. Sheringer and M. Winterhager (eds.) *Representations of Science and Technology*, Leiden: DSWO University Press, pp. 253-265.

Rappa, M.A. and K. Debackere. (1993) "Social and Cognitive Influences in Problem Choice among Researchers in an Emerging Field," *Best Paper Proceedings*, American Academy of Management, pp. 347-351.

Richards, S. (1987) *Philosophy and Sociology of Science: An Introduction*. New York: Oxford University Press.

Rosenberg, N. (1976) *Perspectives on Technology*, Cambridge, UK: Cambridge University Press.

Sahal, D. (1985) "Technological Guideposts and Innovation Avenues," *Research Policy*, Vol. 14, pp.61-82.

Saviotti, P.P. and J.S. Metcalve. (1984) "A Theoretical Approach to the Construction of Technological Output Indicators," *Research Policy*, Vol.13, pp.141-151.

Schmookler, J. (1966) *Invention and Economic Growth*. Cambridge, Mass.: Harvard University Press.

Traweek, S. (1988) *Beamtimes and Lifetimes: The World of High-Energy Physicists* . Cambridge, Mass.: Harvard University Press.

von Hippel, E. (1988) *The Sources of Innovation*. New York: Oxford University Press.

Watson, J.D. (1980) *The Double Helix: A Personal Account of the Discovery of the Structure of DNA*. New York: W.W. Norton and Company.

Weingart, P. (1978) "The Relation between Science and Technology - A Sociological Explanation," *Sociology of the Sciences*, II, pp. 251-286.

Wijnberg, N.M. (1989) "'Industries' and 'Innovation'," *De Economist*, Vol.138, No.4, pp.499-505.

Ziman, J. (1984) *An Introduction to Science Studies*. Cambridge, UK: Cambridge University Press.