autonomous negotiation space where a local sociotechnical network might be designed and brought into being *without constant interference from outside*." John Law and Michael Callon, 'Engineering and Sociology in a Military Aircraft Project: A Network Analysis of Technological Change', *Social Problems* 35(3) (June, 1988), p. 290.

[37] Steven Toulmin argues that it is precisely this systematic rejection of the cognitive validity of the local, the oral, the specific, and the timely in favor of the universal, the written, and the timeless, that characterizes what came to be the central orthodoxy of "modernity". For his argument that aversion to the specific and local stems from Europe's loss of nerve (and sense of humour) in the face of the Thirty Year's War with its carnage of competing religious orthodoxies, see *Cosmopolis*, Ch. 1 and 2. The passage about the local, oral, specific, etc. begins on p. 32.

David Harvey's interpretation of changing capitalist social definitions of space and time and their influence on contemporary society is the most helpful and sophisticated that I have read. See his *The Condition of Postmodernity* (Cambridge, MA: Basil Blackwell, 1989), especially Part III.

[38] Shoshanah Zuboff records repeated instances where managers react "irrationally" to the democratizing influences of open-access computer data bases despite the evidence that such open access, a process for which she coined the name "informating", increased efficiency and profitability, managers experienced disorientation and fear when their control over subordinates was threatened. See, *In the Age of the Smart Machine, passim*

[39] Peter Sandman has developed a persuasive model explaining the increasing social cost that comes due when non-elites are excluded from such prioritizing debates. See his 'Hazard versus Outrage in the Public Perception of Risk', in Vincent T. Covello, David B. McCallum, and Maria T. Pavlova (eds.), *Effective Risk Communication* (New York: Plenum, 1989), pp. 45–49, on the importance of inclusion of non-elites within such prioritizing debates. See also Parker Palmer, *The Company of Strangers: Christians and the Renewal of America's Public Life* (New York: Crossroads, 1985), for a complementary societal analysis.

ARIE RIP

SCIENCE AND TECHNOLOGY AS DANCING PARTNERS

1. INTRODUCTION

Derek de Solla Price, in an article in 1965, put forward the image of science and technology as relatively independent, but closely interacting activities; dancing partners as it were.[1] His analysis of the relation between science and technology is still valuable, but he tended to look at science and technology as separate, unified wholes, rather than ongoing processes and their interactions which cluster in various ways and are labeled "science"and "technology", also in a variety of ways. If this point is added to his analysis, it allows us to raise, and to some extent answer, further important questions: about the patterns of the dance, the contexts in which it occurs, and secular changes and transformations.

Historians, philosophers and sociologists will immediately agree that there is a complex relation between science and technology, which should not be reduced to simplistic formulae like "technology is applied science". What one should say instead is less clear. Price's image still suffers from a certain reification of "science"and "technology", and is thus insufficient to counteract the danger that idealtypes of "science"and "technology"are posited, based on a few examples, or just on the projection of the ideas of the particular philosopher, after which the analysis is conducted in terms of these idealtypes. The evolving character of science, of technology, and of their relations should therefore be foregrounded. Historians will immediately agree, but they may run the opposite danger: getting lost in particular historical episodes and particular individuals and social interactions.

To navigate between the Scylla of philosophical idealisation and the Charybdis of historical particularism requires more than a recognition of the two opposite dangers and the resolution to avoid them. One needs a perspective that overcomes the limitations, at least in principle. The entrance point into the complexity of the real world of science and technology taken here is to view both as search processes which gradually become embedded in local practices as well as in more cosmopolitan fields of science or technology, and which have at least some non-local outcomes.

P. Kroes and M. Bakker (eds.), Technological Development and Science in the Industrial Age, 231–270.

The non-local or cosmopolitan aspect of science and technology is not automatically given. Science is not universal by definition, it takes a lot of effort to decontextualize local findings.[2] And technology is not automatically applicable in other situations, it requires work, both on the technology and on the situations, to create a functioning technology.[3] The social side of this process of decontextualization, which is also a recontextualization into "fields" of science and of technology, consists in exchange between practices, circulation and network creation. The mobility of the Renaissance scholars and engineers,[4] of 19th century colourists,[5] and of 20th century physicists and biomedical scientists,[6] were essential conditions to the creation and maintenance of a field, and of scientific and technological identities of the practitioners.[7] The cognitive-technical side of the process is the emergence of shared disciplinary and professional repertoires. For science, this process is clearly visible since the late 18th and early 19th century. For technology it starts by the middle of the 19th century (but with important precursors, as in building of the cathedrals and fortifications in the Middle Ages).[8]

A number of implications can immediately be drawn from this perspective. One is the possibility of a certain mutual adaptation and division of labour between science and technology: thus, dancing partners. The dance, however, is not pre-given, but is created in particular historical circumstances and made possible by particular "cognitive infrastructures". For example, the emergence of "technical models", like the model of an electromotor (cf. also the late 19th-century work on a general machine), and unit operations in chemical technology, allows scientists and engineers to do their own thing and still relate to each other.[9]

A second implication is the importance of anticipations on the value of expected outcomes as on the one hand incentives for action, and on the other hand as determinants of what survives in science and technology. The dancing partners must anticipate the next steps, and find them important, to be able to continue to dance. More concretely, economic and strategic considerations come in not only after science and technology have produced their products (knowledge claims, blueprints and prototypes), but are part and parcel of the production process. Enlightened economists[10] have, by now, developed such "endogenous" analysis of technology (and may perhaps extend their analysis to science), but sociological and philosophical extension of their work is essential for a full understanding of the dance.

Thirdly, it must be possible to trace changes in the relations between science and technology, transformations even, in these terms in the past and in the present. One may, in fact, speculate about a new kind of intertwining of science and technology that occurs as part of a conscious, strategic mobilization of cognitive-technical potential. This adds up to a strategic or political transformation of the science-technology complex, including the emergence of other organizational and institutional forms than we are used to now (for example, a very different type of university). The dance halls themselves change.

In general, search processes and outcomes in science and technology are shaped by the dance halls, the music, and the dancing patterns that exist. The dancing itself creates new patterns, however, new music is written, and the dance halls may well be modified in the process.

2. THE ROLE OF SCIENCE: AGAINST THE LINEAR MODEL

It is convenient to start with a brief discussion of the so-called "linear model" of the relation between science and technology. That technological innovation derives from scientific discovery, as it were in a linear sequence, is a myth, but a prevalent myth. As a myth it is tenacious because of its links to important legitimations of science as the horn of plenty, and of technology as the magic wand. And one indeed can find examples where it makes sense to relate technical innovation to a preceding scientific discovery. The linear model has some truth in it, but it hides more than that it helps our understanding. It is important to replace it by an alternative model, even if one might argue that one should not try to capture the variety of science and technology in a single scheme, to avoid creating what is really just another myth. Models are still important, as long as their link with ongoing processes is made clear, rather than that they primarily depend on myths about the nature of science.

The myth of science as the source of all sorts of good things is brought up in public pronouncements by scientists and others, and is especially striking in the image of the goose that will produce golden eggs as long as it is fed properly. One can trace back this notion for more than a hundred years, and find quotes like this one from Helmholtz in 1862:

> The scientists – for the benefit of the entire nation and almost always at its request and expense – are seeking to multiply the knowledge which can serve the increase of industry,

wealth, and the beauty of life, the improvement of the political organization and the moral development of the individual. Yet, not immediate utility must be looked for, as is so often done by the uninformed. Everything that informs us about the natural forces or the forces of the human spirit is valuable and in time may prove useful, normally in a place where one had least expected this.[11]

To continue with the metaphor: the geese should be allowed to range freely in the meadow of science, and one might have to seek for the eggs in the dung heap. The point is not that the geese of science never lay golden eggs, only that reliance on such an umbrella legitimation hinders our understanding what actually happens.

The same point can be made about another myth prevalent in our society: technology as the means to achieve, supposedly in an unproblematic way, whatever we want. *What Mankind Can Dream ..., Technology Can Achieve* – this is the slogan of Fujitsu Company: even Japanese companies are using this image to spread the message of benevolent high-tech. The reason why technology should achieve so much is often an implicit linear model: the stereotypical image of technology as the application of all-powerful science, combined with the mistaken equation of the very real and increasing role of science in modern technology with causal sequence from science to technology.[12]

Technological innovation is not necessarily the result of a trajectory starting with scientific discovery – take as an example the zipper, a unique invention, for which neither the original 1891 patent, nor the first practicable design of 1913 owed anything to scientific research.[13] When the innovation is related to science, there is always a lot of work to do (including intervention of and response to external circumstances) before a scientific discovery results in a working process or product. Again, myths about the power of science abound, for example in the way Alexander Fleming is honored as the discoverer of penicillin, while it is through Howard Florey's and Ernest Chain's work (and that of many others) that we have penicillin as a product.[14] In fact, what Fleming discovered could best be described as a mould extract that killed certain kinds of bacteria.

As a more extended example, the case of linear polyethylene can be used to show something of the real-life pressures and contingencies.[15] After the discovery by Ziegler (and others) of catalysts that produced a regular polymer at low temperatures and pressures, there was still very much work to do. Patents were taken out, and licenses were obtained, but

these were very much "hunting licenses": "... everyone went through much the same traumatic adolescence in developing product application information." The main problem was that the new polymers fulfilled their promise too well:

As soon as enough linear polyethylene became available to try to put it to work, it was found that the "Chain Straighteners" had done their work too well, for in polyethylene as in society, absolute straightness turns out to be more virtue than is wanted for practical purposes. The advantages of hardness, stiffness, strength and heat resistance in finished products were gained at the cost of serious difficulties in the manufacture and use of those products. Since Phillips polyethylene had the "purest", 100% linear chain structure, it suffered these difficulties in the highest degree. (...)[16]

In every one of the major developed markets for which linear polyethylene seemed suited, the initial bright promise was tarnished by one or other unanticipated "different" characteristic that proved to be a significant disadvantage. For example, the established markets for packaging film were not open to linear polyethylene because its high crystallinity rendered it opaque and prone to splitting.

All these nightmarish problems came to light just as the first polyethylene plants were coming on-stream, with all the trauma associated with plant start-ups. One after another, each of the "miracle progeny of plastics" began to look more like a retarded child. The company managements that had fathered them were forced to contemplate infanticide and were, in turn, threatened by their boards of directors with financial sterilisation. Heads were greying, if not rolling, in more than one organisation, and Phillips, for one, were very close to closing their plant and aborting the whole enterprise when salvation showed up in the form of a toy.

It would be gratifying to a chemist to be able to report that the research teams had leaped to the rescue just in the nick of time with technical solutions to all the technical problems. As a matter of fact, they did eventually solve nearly all of them, but not quickly enough to save the situation had it not been for the fortuitous burgeoning of an unprecedented, unplanned, unforeseen market. The Wham-O Toy Company introduced the "Hula Hoop", made from extruded polyethylene tubing, and the ensuing craze created a non-critical demand that swept the glutted warehouses clean and put the plants back on an around the clock production schedule.

This fad, even though it faded rapidly, bought time for the polymer chemists to determine that most of their woes arose from the super-crystallinity of their super-linear polymer chains, and that by judicious adulteration with touches of a second monomer (introduced in the polymerisation step), they could put a very occasional kink in the molecular chains and thus alloy the crystallinity of the polymer just enough to suppress most of its deficiencies while preserving most of its sterling qualities. (...)[17]

In this brief example, many features of ongoing processes of science and technology are already visible. The case is also an illustration of a two-branched model that should replace the linear model as a more realistic view. Critical reading of historical case studies and a good look at ongoing interactions suggests a two-branched model, with an empirical or semi-empirical finding as the starting point, to be appropriate. Taking the idea of a "finding" as an unanalyzed category for the moment,[18] and as the source of the developments, two different kinds of activity are seen to branch out from it:

1. *Exploitation* (technological development, pilot process, feedbacks), and
2. *Exploration* to increase understanding (through scientific research).

The insights derived from the exploration branch may sometimes be called in to assist and improve exploitation (trouble shooting, rationalization, and what can be called "transformation of the exemplar", see the example of synthetic dye chemistry in the next section). The relationships are depicted schematically in Fig. 1.

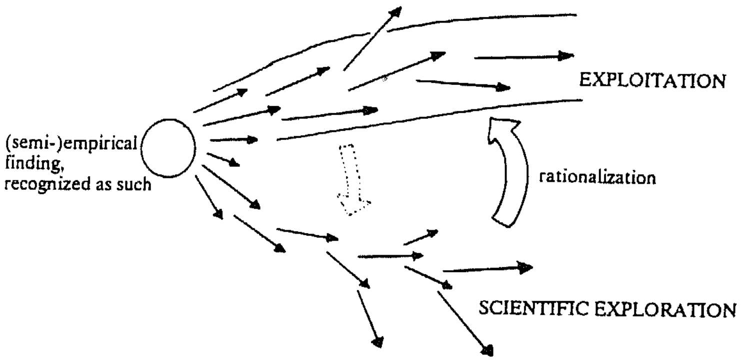

Fig. 1. The two branches of development of a finding.

The history of the linear polyethylene case further illustrates this model;[19] another example is the rapid exploitation of Röntgen radiation after its discovery in the laboratory. While the original starting point may well derive from scientific work, as these two examples indicate, it clearly is not a matter of applying a scientific finding. There appear to be three types of connections between science and technology: a laboratory effect

or method is exploited for another purpose, as happened for example with recombinant DNA techniques or with hybridomas. Or a new domain of nature is opened up in the laboratory, and then also available for technical exploitation; think of Röntgen radiation, of nuclear fission. Thirdly, as I will discuss below, science may be a source of powerful heuristics for technological search processes.

When the linear link between science and technology is severed, one becomes sensitive to the limitations of further received views. For example, knowledge transfer should never be conceived as sending and receiving. There is "local innovation" rather than adoption.[20] And even without such active adoption, there will still be mutually complementary processes of aggregation of user needs, and disaggregation of "universal", i.e. laboratory-based, science findings.[21]

The analysis of the role of science in this section is not exhausted by what was said about the two-branched model. There, the focus is on a specific finding and later developments, rather than on the ongoing activities of doing science and doing technology. What is missing specifically is the fact that scientists and technologists, but also other actors like industry and government, *expect* that there are productive relations between science and technology, and attempt to realize these, for example in innovation-oriented R & D stimulation programs. While these efforts to implement a strategic science and technology policy are a striking feature of the science and technology landscape of the 1980s and 1990s, they are possible only because of much longer-term processes of co-evolution of science and technology.

Analytically, this implies that one should look at science and technology at three different levels: one level is that of legitimations and reified notions of science and technology, which were criticized for obscuring the relation between science and technology in developing new findings. Going down to the basic level of practices of science and technology, one can see patterns like the two-branched model. This is a specific pattern, in relation to new findings and their exploitation and exploration. There are also patterns, like that of decontextualisation and recontextualisation to fields of science and technology at a third, and in a sense intermediate level. It is at the intermediate level that recent transformations, signalled by the new science and technology policy activities, occur.

3. SCIENCE AND TECHNOLOGY AS SEARCH PROCESSES

The complexity of the world of science and technology can be addressed from the perspective that the practices of science as well as technology consist of (contextualized) search processes. That is, there is perhaps no essential difference even while their historical forms are separate.[22] As institutions, science and technology *are* different. The products will often differ (working experiments and texts versus designs and blueprints, artefacts, pilots), and the contexts are still different, even though they used to be more different than they are now.[23]

One implication of the search perspective is that the concrete links between science and technology will be extremely varied, as they derive from the *bricolage* of the (re)searchers.[24] Historically, one can see both separation and intertwining, e.g. the temporary interaction in the Renaissance (and in a more imperialistic way, in the 17th century), the new interactions and demarcations in the 19th century (including the invention of the label "applied science"!), and the high tech and high science world of the late 20th century.

3.1. *Search Heuristics and Quasi-Evolutionary Development*

As the economists Nelson and Winter have emphasized, search processes are structured by heuristics, that is, rules that do not guarantee, but promise some, success in problem solving.[25] Such heuristics are not individual (as studied by psychologists interested in problem solving), but shared heuristics, embedded in organizational contexts and/or practitioners' communities. The products of the search processes are exposed to assessment and selective use by others. So there is variation and selection, the basic building blocks of evolution.

But it is quasi-evolutionary development, because intentional and strategic: heuristics anticipate on selection, and selection environments are modified by search actors. Often, expectations about success, or promise, of certain routes or options, in relation to perceptions of the environment, are the basis for decisions and directions taken.[26] Such expectations may become stabilized (in an organization, in a professional community, in a sector of society or society as a whole – think of the recent promise of biotechnology), and form a *cultural matrix of expectations*.[27] It is always possible that expectations remain informal, and barely articulated, but with the increasing need to justify and legitimate actions, e.g. of an R & D department with respect to its board of directors, or technologists asking for government support, there is ad hoc or even systematic articulation (including special assessments of trends and potential of scientific and technological domains, produced by consultants or expert committees).

So there are two processes at work: one is the articulation of expectations and incorporation of new information and assessments; the other is sedimentation, the emergence of a shared and stabilized repertoire of expectations, on which actors can draw as a resource. As soon as that happens, one can speak of a matrix of expectations, because decisions, choices and further assessments will be embedded in it. Such matrices of expectations are never static; expectations evolve, and there is negotiation about their worth (their reliability and appropriateness). So any description of a matrix of expectations is a snapshot, taken of an evolving process. Still, it is important to introduce the concept as standing for a relatively stable phenomenon, because it allows one to understand, first, how heuristics can spread and how commitments to them emerge, and, second, how others than search actors can be involved: even if not actually participating in problem solving, managers and third parties will base decisions drawing on the cultural matrix of expectations.

Because such matrices of expectations play an intermediary role, it becomes understandable in principle how the whole development is structured through rules and institutions formed around expectations, even before there is any actual external selection.

I shall use the example of synthetic dye chemistry in the 19th century to briefly illustrate the dynamics, and prepare the ground for a discussion, in the next sub-section, of social loci of search processes.[28] The success of aniline red (discovered/invented in 1859, after a variety of attempts comparable to Perkin's "extraction" of aniline purple from aniline) attracted other producers into the synthetic dye field, and the corresponding, and rather primitive heuristic was "tinker with aniline", a heuristic that indeed produced a number of interesting dyestuffs. The aniline red paradigm was rationalized by Hofmann on the basis of the chemical theory of types, and although his type formula did not represent a definitive solution as to the chemical constitution of the "mother compound" rosaniline, it was quite effective in stimulating and directing innovative activity.[29] The attempts at elucidation of the chemical constitution, and the perceived success of such attempts in guiding search for further dyestuffs, can be seen as the first step toward a new, more general heuristic: "Look for the *Muttersubstanz* (that is, the skeleton of the dyestuff's molecule), and if it is found, each derivative of the *Muttersubstanz* may be an innovation". This heuristic became the central component of a new paradigm of purposeful synthesis, which could already be discerned in rudimentary form in the elucidation of the chemical constitution of alizarin and its subsequent synthesis in 1869.[30]

Clearly the two-branched model of development of a finding is applicable, but in addition, search heuristics of more general value emerge. In these examples, the heuristics cluster around an exemplar: a product or process that is an exemplary achievement. Thomas Kuhn (1970) redefined his "paradigm" as consisting of such an exemplar in combination with a "disciplinary matrix". In the same vein, we can now speak of a technological paradigm as an exemplar (in this case, one achieving technical and commercial effects), embedded in a cultural matrix of expectations. Further work in the frame of such a paradigm leads to a trajectory, a sequence of innovations that are related through their use of a cluster of heuristics (which may well be modified along the trajectory).[31] Note that the analogy with science remains: "normal science" in the framework of a paradigm, or, alternatively, the progressive elaboration of a Lakatosian research program, are equivalent to the technological trajectory. In fact, the chemistry in that period can be analyzed with the same categories of search processes, heuristics and partial stabilization in paradigms.

In another example from synthetic dye chemistry, the emergence of azo-dyestuffs, the dynamics of emergence of a technological paradigm can be seen in detail. There was an exemplary process (a coupling reaction, named after Griess, which produced coloured compounds that were interesting to scientists); there was an exemplary product (Roussin's naphthalene dyes, with very useful properties, found by trial and error in an industrial workshop), both with their own search histories and kept more or less secret. The actual matrix of expectations emerged in a dramatic way, with Hofmann disclosing the secret of preparation of both classes of compounds in 1877, and emphasizing that a vast domain for synthetic dye chemistry was now opened, of which the limits were not yet in sight.

The conditions outlined here for the emergence of a paradigm are important for ongoing technological development in general, even when these do not fit the model of a paradigm plus trajectory.[32]

3.2. *Social Loci*

Whether sedimentation into a paradigm occurs or not, expectations create an *agenda* for research directions and investments, mobilize actors and draw them together. In a sense, an arena is created where actors have to take particular expectations as well as actions by other actors into account, because these are relevant for their own assessments and decisions. An example is the emergence of a "technical community" of practitioners, who are focussed on the same, or on mutually relevant issues, and interact directly or indirectly.[33] The 19th century colourists were well known to each other, and relatively independent of employing organizations.[34] They circulated from one firm to another, and often commanded high salaries. The colourists carried the technical heuristics of dyeing and making appropriate dyestuffs, and were thus the social locus of these heuristics.

In other cases, other actors and institutions were involved. While the paradigm plus trajectory of rosaniline dyes, for example, were carried by university researchers as well as industrial colourists (there was no specialized locus yet), the azo-dye trajectory coincided with the emergence of the industrial research laboratory and the routinization of research.[35]

These examples can also be used to make a further point: whether it is a technical community, institutionalized research laboratories, or some further variant, a social locus will introduce a dynamic of its own, because of its social and economic make-up, and its interest in survival. Further search processes will be started, and expectations will be voiced, to maintain the power of the social locus and legitimate it. This aspect of the dynamic is captured rather well by the actor-network approach of Callon and Latour, because the arrow of technological dynamics here goes from network processes and strategic interactions to technologies.[36] Particular heuristics can become so entrenched in a social locus, that they "determine" the course of developments, and the approach to new problems.

In general, one can predict that there will be a phase of rather undirected search and heterogeneous expectations, a phase of sedimentation of heuristics and expectation matrices and their institutional embedding; and a phase of elaboration of more or less accepted heuristics and expectations. This prediction can be related to the successful approach of Abernathy, Clark and others in terms of a design hierarchy and its maturation.[37] Note that a design hierarchy has a social locus: it is carried by the producer-consumer network called an oligopolistic market. In the same vein, Dosi proposes a two-phase model of technical change and industrial structure: a Schumpeterian trial and error phase, and a phase of oligopolistic maturity.[38] As with the authors quoted by Dosi, it is not clear if the model is descriptive, or has a normative point, e.g. should one stimulate oligopoly when a technology and industry is "mature"? The proposal is clearly important, but the paradigm is reified into an independently existing factor, about which questions can be asked as "In what phase of development is the paradigm of domain X?", the answers to which would then have policy implications in a mechanical way. I would argue that a process analysis of the technological dynamics is necessary in which questions about paradigms can, and should, be embedded.[39]

A final point to be made is that institutions relevant for technology (and similarly for science) emerge and function as a response to the activities of anticipation on selection (and on strategies of other actors) and as an attempt to reduce uncertainty and create stability in the environment. Van den Belt and Rip introduced the notion of institutional nexuses here, that is, particular linkages between variation processes and selection environments, *that can be adopted by others.*[40] Examples of institutional nexuses are the patent system and the test laboratory (where findings are subjected to what one could call a simulated selection environment). The spread of such a nexus is clear in the case of routinized industrial research to exploit inventions, which emerged in the late 19th century in electrotechnical and chemical industries, and was a necessary carrier for successful heuristics of system design, and rational synthesis, respectively.

Recently, and especially with the rise of strategic mobilization of science and technology, a new type of institutionalization has emerged, which allows virtual selection on the basis of expectations, as it were before the fact. Expectations then become a resource, and appraisal and negotiation routines develop. Biotechnology provides a clear example of such new institutions. Biotechnology R & D firms operate by creating expectations (up to rapid increases of their stock market value after introduction of their shares on the stock market)[41], but have to keep up these expectations by doing the right things (engineering a growth hormone, or insulin, or interferon) and surrounding them with the right publicity. These may not even be the most profitable innovations (which, especially during the first years, were drugs and feed components in

animal husbandry), but they are necessary to maintain visibility for wider publics. The whole business is precarious, as the decrease in stock market value of the same firms shows, as well as the difficulties into which a number of them have run.

More generally, one could say that in a world where innovation competition becomes more important, early promises about innovations will be valued as such, that is, as building blocks of the necessary matrix of expectations. A speculative market of early promises emerges; a process fully analogous to the emergence of stock exchanges, where shares in firms (and before that, in merchant ventures) could be traded in terms of the expected profits. The R & D firms that carry this market started out as a specific nexus, but when this type of firm became institutionalized, the market became relatively autonomous, and academic research groups and other actors could also start offering their early promises for "sale", i.e. to be taken up in exchange for resources. Strategic mobilization of science and technology can then become a regular phenomenon, rather than ad hoc and based on bilateral negotiation.

Three points can be made to conclude this outline of a quasi-evolutionary model of search processes in science and technology. Scientific and technical communities used to be the locus for appraisal of expectations and developments of heuristics as such, and while being shaped by institutional locations of the practitioners (as will be discussed in the next section), could do so relatively autonomously. Many more actors are now involved explicitly. Analytically, the point is that the activities continue to be at the level of fields of science and technology (including patterns of decontextualization and recontextualization, as I noted in Section 2).[42]

A second point is that the model connects two aspects that are often kept separate: the action and process level, where actual production of technology, adoption and diffusion occur, and the level of cognitions and legitimations, where heuristics, expectations, and rules are to be found. In other words, to "producing a working artefact" it adds two other important activities: "making a clever move" (this is getting more attention in recent technology studies)[43] and "telling a good story". The telling of stories, so that they have effects, is an important aspect of socio-cognitive-dynamics, but has been neglected in general (even if there is some interest from organization studies), but certainly in technology studies.[44]

Thirdly, for sociological and economic studies, the model outlined here is more general than the traditional focus on firms (especially by the economists) and on technical communities (especially by sociologists and historians). It also adds to innovation studies, as well as Latourian studies of Machiavellian engineers, which neglect how actions, interactions, adoptions etc. add up to something at the collective level that is sufficiently stable to be drawn upon by actors as a resource.

4. DIVISION OF LABOUR BETWEEN TECHNICAL PRACTICES AND ACADEMIC RESEARCH ON THE BASIS OF TECHNICAL MODELING REPERTOIRES[45]

Engineering design at the local level is a socio-cognitive process whereby specific goals or mandates guide the manipulating of properties and configurations of the artifacts-to-be-made, but such design processes also relate to a more or less cosmopolitan design culture, which contains technological paradigms, routines, heuristics, norms and standards. For the question of relations between technology and science the notion of "technical model" is important. It is a reasoned (though not necessarily fully articulated) conceptual representation of class of artifacts and artifact systems that is present in design practices, in technical communities, and (after a time) also in technical education.

Technical models, like the general model of a bicycle, an electromotor, a bioreactor, or a membrane, are mental or material representations of artifacts as a system of interrelated and mutually constraining sub-elements. The importance of such a model is that it allows designers to infer aggregate artifact behaviors from specific element parameters, the overall configuration of elements and the mutual constraints obtaining among them. It should be noted that any given species of artifacts may be modelled in a number of different ways, depending on the specific behaviors which are of interest. Thus, for example, ships' hulls may be modelled for their hydrodynamic, their aesthetic, or their structural properties; electrical power plants may be modelled as self-contained thermodynamic systems or as self-regulating components in a larger system of variable demand and power loads.

Thinking in terms of, and working with, technical models enables designers to engage in *virtual* manipulation of design parameters with an eye to optimization along relevant evaluative dimensions. In addition,

they provide a potential *cognitive infrastructure* for the participation of actors-at-a-distance, in an organizational and strategic as well as epistemological sense. Improvements in ship hull design can profit from work on the hydrodynamics of particular propellers in one research institute, and the development of new welding techniques in a specific firm, because they all relate to each other in a technical model.

Within well developed technical domains, technical modelling repertoires are routinely available to designers and will be incorporated into engineering curricula, complete with standardized symbol systems and application protocols. The subjective element here is reduced to decision making about how to apply or adapt available modelling strategies to local design problems, e.g. how to adapt standard practices for modelling ships' hulls to the modelling of *this particular* ship's hull.[46] Historically, such technical modelling repertoires emerge out of local modelling, and its improvement. Consensus among local designers on efficient nomenclatures and symbolizations and on the relative salience of specific relationships and parameters in technical models for particular types of artifacts, are important steps toward such a repertoire. It is then institutionalized, and further developed within professional discourses and engineering curricula.

When technical models are part of the shared repertoire, it becomes possible to improve them as such, independent of particular design tasks. This can be seen clearly in attempts towards a theory of the general machine,[47] and allows academic research to contribute to technological development (at least in principle). I shall call such work on technical models "meta-modeling".

4.1. *The Globalization of Technical Modeling*

A local technical model can be problematized because it fails to solve design problems, for example in achieving more ambitious specifications, and this may lead to local excursions into meta-modeling, sometimes involving scientific research coupled with the importation of professional researchers, as well as to efforts to learn from the experiences of others, including appropriation of published findings and other forms of information exchange with technological competitors. Within particularly large and wealthy state agencies or private companies such meta-modeling may become a differentiated organizational activity, either on an *ad hoc* basis to solve emergent design problems and appropriate new technologies (e.g. in the form of incidental experimentation within such organizations as Army artillery corps, railways, national public works agencies) or more structurally as indicated by the emergence of state and corporate research laboratories at the end of the 19th century and again in the 1920s and 1930s. Meta-modeling then can be seen as fundamental research, although not necessarily connected to basic science as traditionally viewed.

It is true that technical models are nowadays often anchored in basic science, in the sense that design-relevant representations of artifacts draw heavily on scientific theories about phenomena which are embodied in the artifacts, rather than starting out as local *ad hoc* constructions. But even then, the construction and optimization of technical models, i.e. meta-modelling, remains a distinct type of activity, and the province of research specialists within a global division of technological labour.

The configurations of actors in this division of design labour are nationally and historically contingent, but a dominant pattern emerged during the nineteenth century, at least in the nations of the European continent, which is still visible today. Professional engineering associations and polytechnical schools gradually insinuated themselves into local design processes by first establishing the necessity and legitimacy of abstract, theoretical, and therefore generalizable styles of technical modelling,[48] and subsequently assuming some of the burden of the associated labour of meta-modelling. In later phases, universities, particularly in fields like medicine and chemistry (e.g. organic chemistry in relation to synthesis) played such a role.

There was a strong social dynamic to the emergence of the dominant pattern. Unanimity, and uniformity of design protocols (thus of technical models) was a central value for the newly self-conscious engineering professions. It legitimated their claim to a unique role in design practices by their demonstrative commitment to critical, "scientific" scrutiny. In the second place, uniformity of technical models, including the standardization of symbolic representations and algorithms, facilitated communication among professional engineers (e.g. electrical circuit diagrams or stress calculations for bridges produced in one location could be routinely read by professional colleagues elsewhere). As uniformity in technical modelling thus cemented professional solidarities, it also significantly set up barriers against competition from non-professional practitioners.

Teaching staff at the new polytechnics had in common their mandate to train experts capable of producing state of the art designs and therapies in a variety of technical situations. This meant that they could not inculcate rote solutions to invariant problems but were compelled to impart more general representations which could guide optimal design strategies in a variety of settings, i.e. to teach technical models rather than techniques. This acted as a spur to faculty to produce (or at least explicate and codify) the requisite technical models, including fundamental research to establish salient relationships and parameters involved. In addition, engineering professors traditionally felt (and may still feel) forced to academize technologies as part of an ongoing collective status struggle with the classical universities. So they oriented their research not only to the specific design needs of technologically committed organizations but also to participation as scholars in emerging disciplinary fields (i.e. to profiling themselves as professors of engineering *science*). Being institutionally isolated from the specific local design imperatives of state agencies or private enterprises and seeing the education of engineers as their major task, they were both enabled and compelled to focus on the basic and universal aspects of the design process, including codification, standardization, and the formulation of general technical models.[49]

Note, however, that the engineering faculty did not fully monopolize the role of agent of cosmopolitanization in the division of design labour: engineers associated with the various technical corps within the military and the large state infrastructural agencies which were part and parcel of nineteenth century state formation in Europe also continued the originally French tradition of contributing to the cosmopolitan fund of technical models.

Cosmopolitanization occurs in different technical domains at very different points in time. Nonetheless, both the academic motive and the institutional framework for design cosmopolitanization is given with 19th century educational modernization and the rising status of "science" as a legitimate academic pursuit. From that point on, the division of design labour and the emergence of cosmopolitan design networks becomes a more or less standard milepost in the development of new technical domains. Synthetic organic chemistry passed through this phase – and in a number of steps – in the last half of the nineteenth century (and a replay occurs in the 1970s and 1980s with "synthon" theory). In chemical technology, the emergence of unit operations in the first decades of the century and the effort to introduce chemical technology/chemical engineering into the universities, is a clear example. Among civil engineering technologies, academic involvement in the mathematical modelling of reinforced concrete construction became, from the late 1890s on, a logical extension of traditional professorial involvements in the theory of applied mechanics and elasticity in general.[50] Classical biotechnology was a late bloomer, showing cosmopolitanization by the 1940s and 1950s and, from about 1960 onwards, attempts at academization, e.g. the founding of university departments, the publication of handbooks and the founding of scholarly journals.[51] There are other examples of late (and sometimes negligible) entrance in academe, e.g. polymer science and engineering, heterogeneous catalysis (pioneered by the oil companies and some big chemical firms),[52] and atomic energy engineering.

What is important for my overall argument is that there is a pattern, and that it cannot be specified in terms of a relation between "science" as such and "technology" as such. Rather, it is an autonomous development of technical domains, which makes them resemble scientific domains. In fact, the distinction becomes tenuous, and seems more related to the nature of the links with local design work, than to any inherent characteristic of science in contrast to technology.

4.2. *Additional Linkages between Academic Research and Technical Work*

In the early twentieth century, the institutional loci for the cosmopolitan design networks, were the polytechnics and technical universities and the big industrial and state-related research laboratories. With the notable exception of the United States, the classical universities did not want to be seen as centers of applied science research.[53] The polytechnics (acquiring formal academic status, i.e. the *ius promovendi*, in Germany, Austria, and Holland around the turn of the century) had effectively monopolized engineering science in most fields. Thus, the electrical and electronic revolutions, despite their historical roots in the physical sciences, did not become an occasion for university-based specialties because of the institutionalization of electrical engineering at the technical schools (and of research in state and corporate R & D labs). Until after the turn of the century, academic geology anxiously kept its distance from polytechnic-

based mining engineering, concentrating on classifications and the etiology of formations rather than on technical models directly relevant to the discovery and extraction of ore deposits. A somewhat different relationship prevailed between university chemistry and the chemical technology which had emerged as a pendant to various process industries.[54]

The definitive breakthrough of industrial research laboratories happened by the 1920s (state laboratories had a longer tradition, but were originally related to the standard setting and regulatory tasks of the state). It was increasingly often the case that ideas for new artifacts and designs emerged out of local industrial meta-modeling research itself. For example, Philips company's facile entry into radio vacuum tube research after the first world war was a direct consequence of the knowledge gained from meta-modeling work on medical X-ray tubes during the war.

Within this set-up, new possibilities for linkages between academic research and technical work (local as well as cosmopolitan) were emerging. The simplest way to show this is to start with the technical hierarchy in design:

- components (e.g. materials, nuts and bolts, resistors and condensers, radio tubes) that do not "work" by themselves, but have to be assembled;
- devices (e.g. a pump, a switching circuit, a sensor) that are assembled sufficiently to show their primary effect;
- artifacts (e.g. a machine, a bridge, a radio), that work by themselves;
- systems (a plant, an electricity network, radio broadcasting plus receivers plus organizations to produce radio programs) that fulfill a function.

The levels merge into each other (a radio tube can also be seen as a device), but the point is that a hierarchy exists, and that design work can take place at different levels.[55] At first (analytically, but often also historically), design takes place at the level of artifacts or systems, and components and devices are made for that purpose, or drawn from suppliers, as the case may be. Development work and design can occur with the suppliers, but will depend on specifications from the customer. Network relationships develop, but these are concerned primarily with production and sales, not with a division of labour in design work. In principle, cosmopolitanization can occur when suppliers supply to several customers and can optimize their products according to generalized specifications.[56] Technical and other sciences can then be mobilized. The petrochemical industry, for example, produces chemicals as building blocks for products that other firms make, and does chemical research drawing on general chemistry, rather than developing a specialty by itself (as does happen for their own process technology, cf. the example of heterogeneous catalysis mentioned above). A soon as this happens, general chemical research becomes relevant to design work. One could call this the "design supply" role of cosmopolitan research, in addition to its "meta-modeling" role.

The initiative at the supply side can take other forms as well: components or rough versions of devices can be discovered, and possibilities to use them in artifacts or systems are exploited. This can happen independently from academic scientific research. As I noted already, the zipper is an interesting example of a device, that has set into motion a whole series of innovations in clothing and coverings. The use of X-rays in medical apparatus, also mentioned in Section 2, is an intermediate case: the phenomenon was discovered in a scientific laboratory, and in the course of basic scientific research, but it might also have occurred elsewhere (say, in a photographer's studio), and the design work did not draw on scientific insights (and could not do so, because there was no systematic knowledge of X rays in the beginning). The two-branched model presented in Section 2 can be developed further by recognizing that exploitation of the finding is structured by the technical hierarchy, while exploration has a double dynamic: rationalization in terms of devices, artefacts and systems that are being exploited, and linking up with fundamental scientific disciplines and their theories and accumulated empirical insights.

By now, there are many cases where (basic or strategic) science is working toward innovative components or devices systematically, for example in polymer science and solid-state science (new materials) and in micro-mechanics (sensors and actuators) and mechatronics. It is clear from the latter examples that technical-scientific fields have emerged, that are institutionalized as fields, also in universities, and play a recognized global role in technical innovation. Before this could happen, however, it was necessary that the supply of components and devices became an innovative activity, and recognized as something worth pursuing by itself. The rise of the industrial laboratory in the interbellum, discussed above in relation to meta-modeling, was also related to the interest in research into components and devices in general, Philips Company's NatLab again being a clear example. Subsequently, World War II efforts were a further push: scientific knowledge and scientific manpower were mobilized on a large scale, and whole new areas of research were created in which fundamental work occurred, but within an overall framework defined as component or device search. Polymer science, radar and other electronics areas, and atomic energy research, are well-known examples. New specialties could emerge, and the training of such specialists was seen as a responsibility of the universities (as the pressure – but only by the 1960s – for chairs in polymer science and heterogeneous catalysis shows).

By the 1980s, three strands have to be woven into the fabric of the cosmopolitan design network. First, the meta-modeling role of the technical sciences in academic settings remains important, and links up with the mathematical modeling approaches that have become more sophisticated. Second, the design supply role has become more important, and has extended to more sciences. One element of the recent programs to promote strategic or innovation-oriented science is exactly the mobilization of academic sciences for design supply, with the programs on new materials (polymers, membranes, ceramics) as key examples. Third, innovation competition between firms and strategic positioning of states and blocks of states (e.g. in the TRIAD) has added a new element to technical innovation: not the actual outcome of the innovation is the primary aim, but the coverage of a potentially important area of innovation. One indicator is that firms are prepared to spend (a lot of) money on the support of academic research in order to have a "window on science" . The coupling with local design practices is becoming remote, also in firms themselves. Or, alternatively, local design work is shaped by the exigencies of the innovation race, as can be seen clearly in the unending quest for the "next generation" of chips, the very large scale integrated circuits now containing more than a million components. Expectations always shape technological development (Section 3); there is now a quasi-autonomous dynamic of strategic research, in which universities can take part (fully and often only very partially, given the complexity and resource requirements of strategic research in many areas).

5. QUALITATIVE TRANSFORMATIONS OF THE SCIENCE-TECHNOLOGY COMPLEX

If one thing has become clear from the analysis in the preceding sections, it is that there is a variety of relations between sciences and technologies, that these relations and the patterns in them evolve, and that the evolution is shaped by the social loci of the search processes and the global scientific and technological fields, with an important mediating role of the division of labour in global design networks. By now, science and technology are intertwined and form a complex – dancing partners that cannot be pried apart.

The science-technology complex has been built up over decades, and its shape shows the traces of its origins. I have focused on meso-level developments, talking about institutions, technical domains and scientific disciplines. These are, of course, embedded in overall societal developments, and one should, ideally, write the history also at that level.

One attempt to do so is Tom Hughes's *American Genesis*, which analyzes how the second industrial revolution is characterized by the way the Americans built a technological world. This has created a technological momentum: the systems, and their preferred characteristics, take on an autonomous character and are difficult to direct or control other than along an inertial projection. Since these mature systems experience most of their social shaping in their early stages, they bring out of their past, the solutions to past problems. Thus, the momentum of the modern may be so great in the United States that the next great technological and cultural change may occur among other peoples in another nation.[57]

The diagnosis of what happened is impressive, but there is a curious neglect of the changes in science and technology that may be happening now, in the United States as well as elsewhere. What is the new dance that the dancing partners are developing? There are good reasons to think that qualitative transformations are occurring.

One way to analyze the transformations is to conceive of an emerging new techno-economic paradigm, as Freeman, Perez and others do. They relate the transformation to the combination of micro-electronics and telecommunications becoming a so-called pervasive technology, having repercussions in all sectors of society.[58] In doing so, their interest (as economists) is primarily in the performances actually realized, not in the search processes and strategic behavior that goes into the creation of these performances.[59] Without pronouncing on the value of their conceptions, I limit myself here to what is directly relevant to my theme of relations between science and technology: the micro- and meso-levels of search processes, the practices in which these are embedded, and the structures that are emerging.

Some indicators of transformations at the cognitive side are:

- The emergence of combined technologies ("technological fusion" as it is called by the Japanese),[60] which are recognized as such and thematized in research programs, institutes, and perhaps, in time, new sciences. A prime example is mechatronics, and optelectronics is an interesting recent development. The Japanese have made a particular point about these combined fields: this is where the future is, they declare, and invest accordingly. The USA National Science Foundation program of Engineering Research Centers appears to focus on combined technologies as well.
- Technological development leaves hardware, and becomes "paper" (and in general, software) technology.[61] Within science, similar trends are visible: Monte-Carlo calculations in physics, computer synthesis in chemistry. Neural networks, the latest fashion, are often simulated on computers, rather than actually built and tested. (Note how difficult it is to decide whether neural networks are part of cognitive science or cognitive technology.) A striking further development is how the computer-aided design of chips (very large scale integrated circuits) is now linked up with computer simulations of chips production technology. Instead of real-world tests, the electronic designs are now coupled to an electronically simulated "silicon foundry". One need not leave the simulations any more.[62] These developments create flexibility in research, and a loosening of the links with actual technical-industrial practice. Models and simulations are increasingly used by industry, and because they are less specific than concrete designs and prototypes, they are less sensitive, and can be published in the scientific-technological literature. And this is in fact what happens: for the R & D stimulation programs of the European Community, there is little difference in publication activity between industrial and academic participants.[63]
- Technology becomes strategic, not in the common sense that its products are of strategic value to actors, but that its process can be characterized as strategic. *Choices* are explored rather than technical effects produced. This is a continuation of the previous point: in a simulation, one can vary parameters, and find out what happens "on paper" (paper includes electronics here). And part of the effort in creating model systems is to explore their behavior, somewhat independent of the extent to which the model systems reflect some actual or potential technical reality. Design is becoming an autonomous activity?[64]

The new institutional set-up contains industrial research labs, strategic alliances, international R & D programs, and new hybrid centers like the USA Engineering Research Centers. (Practitioners and their patterns of "circulation" through the institutions are another important aspect). Let me try to sketch the outlines of what the new institutional patterns and structures may be.[65] At the micro-level, a fruitful focus is to look at the rewards that move scientists (rewards include monetary resources, but then for the opportunities these provide, rather than as private income). Publishing is no longer the only criterion in academic careers. Institutions find the amount of grants/external money that Professor X brings in important, materially as well as symbolically, and take that into account in job interviews and career decisions. At first, this implies only that local, institutional criteria will become important. If the phenomenon is widespread however, this implies a secular change where the norms of science become more like PLACE (instead of CUDOS), with the products of science being defined as proprietary, i.e. related to employers or clients.[66] When the new external linkages become institutionalized, however, and industry and other sectors linking up with fundamental science adapt to the situation, for example by allowing some exchange among scientists, and attendant reputation building, a "new cosmopolitanism" emerges that transcends particular local criteria. Reputation and status can then be acquired at the level of the field, similarly to the way reputation and status in traditionally organized scientific fields can only be built up at the field level if there is sufficient exchange and mobility.

This is not idle sociological speculation: in the case of professional engineering, for example, engineering firms and their members often have high professional status in spite of the reduced public character of their work. In these engineering fields, there is enough personal mobility, exchange at meetings, and comparisons of performance (including promised performance, when tenders for a project are submitted and evaluated) to make reputation and status building possible. This implies that engineers can actively work to acquire such reputation, because it lends them engineering credibility that they can exploit, e.g. in further resource mobilization. In fact, one can even find further similarities with traditionally organized scientific fields, because there will be some "organized scepticism": the quality of products is evaluated virtually, by competent colleagues, before clients have to use the products and take real risks.

New organizations, institutions and relationships allow the emergence of non-traditional reward systems, while their establishment allows some of the new institutions to be integrated in the R & D system. To mention a few examples: university-industry centers in the USA may evolve from only local institutional differentiations to *loci* where reputation and status at the field level can be acquired; government R & D programs become a permanent feature of the R & D system, and while filled up with changing strategic fields and priorities, participation in such programs in general will count as important in the *curriculum vitae* of a researcher. Engineering firms have been mentioned already, and more of such "expertise brokers" appear: private medical institutions were not unknown, but occur more widely, and consultancy firms proliferate (interestingly enough, also in the environmental area).[67]

The *loci* for acquiring reputation and status in science at the field level used to be the universities, at least, after 1870. But as soon as the notion of *site* for cosmopolitanism is taken seriously, there is no principle reason for such a function to be limited to universities. Not only because scientific research is (and has always been) broader than university research, and the central role of universities in the production of scientific knowledge may thus be a historical accident. Also because the "strategic" science that has become so prominent a category nowadays, is really a label for sites and opportunities for coordinating strategic action. Therefore, a *variety* of institutions can compete for a central place. The proliferation of "centers" connected to universities, of para-university institutes of different kinds, and of institutes fully outside the university system, like the Wissenschaftszentrum in Berlin, becomes a significant phenomenon.

In other words, while it may seem that universities are only creating new linkages (e.g. with firms) for themselves, they are in fact competing with the other institutes on the new "market" of strategic science. National laboratories, for example, are also becoming more active and may well take up new positions, depending on their ties to government, but also on the opportunities that the changing R & D system offers. The specific advantage of universities may still be that they also do (advanced) training – but even there, competition may come up (even if earlier ideas in this direction, e.g. scientific training in industrial firms, or through a consortium of firms, have come to nothing).

While these considerations are, of necessity, speculative, they do show that the categories of "science" and "technology", or of the university as the "home of the scientists",[68] are of little value as entrance points to understand the transformations that occur. Instead, one has to look at the combined cognitive and socio-institutional changes. Indicative is the struggle to create new categories to capture emerging phenomena: "technologie de base" or "strategic science".[69] The metaphor of dancing partners loses its point.

6. CONCLUDING REMARKS

How far have I come in analyzing the dance of science and technology? In two ways, I tried to overcome barriers to our understanding: I did not start with science, its nature and its possibilities with regard to technology, but as it were turned the tables, by looking at search processes in general, and at technology being rationalized, becoming theoretical, "scientific" even, and thus offering opportunities for existing science to get involved.

Secondly, I emphasized the danger of following legitimatory usage of science and technology as umbrella terms,[70] and focused on ongoing processes and institutionalized activities instead. This is not to say that there is no such thing as "science", or "technology", or that it makes no sense to talk about the world of science and the world of technology, and how these interact. But the notion of science as such is constructed (and maintained) by actors in specific contexts, for example when in the 19th century calls are made for engineering to become scientific, or when science is defended as being the backbone of Western civilization. The force of science does not exist in a vacuum, but derives from these contexts and what actors do with such constructions. Similarly, there are times and places where worlds of science are created, and become sufficiently institutionalized to continue as such. Engineers (and other people not usually seen as scientists or scholars in the 19th century) can then be said to follow hybrid careers when they also play a role in the world of science.[71] For the late 20th century, however, it may be misleading to analyze in terms of a world of science, that is a direct successor to the world of science as it existed, at least for physicists in the 1920s.[72] That is why I have emphasized that transformations occur: while there is some continuity of actors, institutions and practices, the overall pattern shifts, and it now means something different to be in the world of science.

In this approach, there is the unavoidable ambiguity of using common sense notions of scientists and engineers, of science and technology and their demarcations, to mobilize empirical material and interpret it, and on the other hand not wanting to be imprisoned by such categories which will hide rather than disclose. I have taken a sociological detour through search processes, heuristics and expectations, local design work and cosmopolitan design networks with their division of labour, to be able to reconceptualize what is happening in the science-technology complex. Central to this reconceptualization is the relation between local search, design and anticipation activities, and repertoires, cognitive-technical infrastructures and interactions and institutions at the field level. In these terms, one can give an analytic definition of what science is, if one wishes to do so: when search products are "consumed", that is, assessed and used, by others like yourself, and a cognitive and evaluative repertoire has emerged together with social linkages and mobility implying community formation, it makes sense to speak of (a) science. In contrast, technology has search processes which are directly or indirectly related to local design and construction activities. (Note that this definition makes large parts of social science and policy analysis fall under technology.) The dancing partnership of science and technology now becomes a relation between activities oriented to different reference points and groups, rather than a matter of combining different cognitive-technical repertoires.

Obviously, in concrete cases people draw on different domains and specialties, some of which have dynamics like science, and others like technology. At this level, there may well be problems of interaction, but these relate to the general problem of combining insights and skills from different sources. Similarly, institutional problems, like interaction between university and industry, or new roles for academic research, need not derive from any general issue of "science" versus "technology", but from heterogeneity of institutions. As I have argued, such heterogeneous social loci will, in fact, feed into new definitions and demarcations of science and technology.

In a sense, I define the question of the relation between science and technology away. In so far as it is to be discussed, it is in a battle of legitimations. To analyze and understand interactions, one had better not use the umbrella terms.

Looking back at the alternative analyses and conceptualizations I have discussed in this paper, however, these do not completely exhaust the original question. One can do away with "science" and "technology", because these are rhetorical constructs rather than actual dancing partners, but one still has to face the question, at the micro-level, of the relation between knowledge and artifacts.

To bring out a way to approach this question, the difference between knowledge and artifact has to be reconceptualized. The received view runs as follows:

> Although science and technology both involve cognitive processes, their end results are not the same. The final product of innovative scientific activity is most likely a written statement, the scientific paper, announcing an experimental finding or a new theoretical position. By contrast, the final product of innovative technological activity is typically an addition to the made world: a stone hammer, a clock, an electric motor."[73]

The contrast, however, cannot be made so easily.

Implicit in my analysis is the idea that there is a continuum rather than a categorical difference. What is used is not knowledge as such, but knowledge claims: statements which include the claim that the insight or effect is applicable in other situations, including the situation at hand. While artifacts, at least as they are treated in technology, are not just contraptions laying around, but a promise of functionality provided one embeds and uses the artifact in the right way. Placing knowledge claims and artifacts on a continuum in this way allows me to argue that both can be inserted in search processes, without any special problem of a categorical divide (compare the beginning of Section 3). The two-branched model of exploitation and exploration of a finding, discussed in Section 2, in fact builds on this insight; insofar as different routes are followed, this is a matter of division of labour and institutionalization, rather than a difference in kind.

The interesting difference relates to the decontextualization and cosmopolitanization that happened to knowledge claims and to design and construction insights and rules. Decontextualization/cosmopolitanization creates a certain robustness, but also distance to concrete, local usage. It is this tension, and the attempts to resolve it, which creates the movement that I have described in this paper. Various cross-sections and other constructions can be labeled "science", and in other cases "technology". The dance of "science" and "technology" is then like the wooden figures on top of a musical box: they turn and bow gracefully, but are puppets rather than actors.

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NOTES

[1] In Price [1984a] he adds that what keeps them linked is that both dance to the music of instrumentalities; see also Price [1984b].

[2] Latour & Woolgar [1979]; also Rip [1982].

[3] Cf. Latour's [1987] criticism of the diffusion model. In Latour [1990] he illustrates the necessity of working on the situations with a number of examples, including one from a study by Marc Bloch: "In the late Middle Ages, the grinding stones, the gears, the wheels and the rivers are good unexpected allies that, once tied together in one mill, makes a formidable stronghold. But their efficiency stops there. A stronghold can be in the middle of a battlefield, thus bearing on the issue of the battle, or *away* from the battlefield. If each household goes on grinding corn by hand, the Prince, who holds the communal mill, will hold nothing but wood, water and stones. The mill will become a stronghold only if the Prince fetches the militia, enforces the King's ruling, the Church's teaching and compels every household to break their hand-grinders and to pass through the miller's stone." (p. 26) The moral of this story, as Latour makes clear, is that social power is necessary to ensure technical power (as well as vice versa). In his harangues against technological determinism (labeled "trajectories" in the 1990 paper) he gets carried away by his own rhetoric, however, and forgets that there may be patterns in the sociotechnical alliances that might usefully be described as trajectories.

[4] See Wolfgang Krohn's introduction to Zilsel [1976], esp. pp. 23–29.

[5] Homburg [1983].

[6] Casimir [1983], Latour and Woolgar [1979].

[7] Rip [1985].

[8] Böhme, Van den Daele and Krohn [1978].

[9] This idea draws on Disco, Rip, Van der Meulen [1992] and ongoing work of these authors. See further Section 4.

[10] Nelson and Winter [1977], Dosi [1982], Dosi *et al.*. [1988].

[11] Quoted after Van den Daele [1978], p. 32.

[12] See for example the way even Dosi [1982] essentially works with a sequence model, where findings go downstream from science to technology, passing filters (that include socio-economic criteria).

[13] Ziman [1976], pp. 180–181.

[14] Ziman [1976], pp. 188–193.

[15] As described by McMillan [1979].

[16] McMillan [1979], p. 89.

[17] McMillan [1979], pp. 90–91.

[18] For our argument here, the starting finding may be treated as a point source. When one looks more closely, it dissolves into contingent processes, and only with the benefit of retrospection can one identify a source. Compare Latour's [1987] analysis of the Diesel engine.

[19] McMillan [1979].

[20] Prakke [1988].

[21] In a study on knowledge transfer (Rip, Schoemaker and Meus [1983]), it was argued that the "matching' of university research and societal needs requires cognitive and social transformations, both the aggregation of research questions through sectoral organizations,

patients' associations, trade unions; and disaggregation of academic approaches, and that one can observe such processes in actual successful knowledge transfer. Note further that laboratory-based, in the main text, should be read as including method-based (e.g. social science methods like surveys). The point is that scientific findings derive their "universality" from being produced in restricted environments (see Rip [1982]); this point is the same as Harré's, when he argues that "manipulation of matter under the control of a skill is prior to a more conventional scientific activity' (Harré [1990], p. 35). In the complexities of the real world, applicability is not assured by "universality", and requires either de-universalization or concretisation (Böhme, Van den Daele and Krohn [1973]) or transforming the world into something resembling the laboratory (Rip [1982], Latour [1983]).

[22] This is also argued by Latour, but then from a power perspective, when he focusses on the Machiavellian engineering that goes on in all walks of life (Latour [1986], [1987]). The search perspective is not blind to power, but takes the cognitive aspect seriously. In the search perspective, there is the same attention to the need to find and enroll allies, both human and non-human: microbes and electrons have to be kept under control in order to have robust findings. But there is also attention to exploration of possibilities, following a lead, elaborating a cognitive schema. This is not sufficient to explain the "success", i.e. the robustness of findings. But it is necessary to understand the direction that is taken.

[23] This view of science and technology has been put forward by a number of authors by now, including de Solla Price and Layton, and has been dubbed the "two cultures model" by Barnes and Edge [1982, pp. 147–154, esp. p. 151]. See also Weingart [1978] for a differentiation-scientification thesis which is quite compatible with the present analysis.

[24] The term "bricolage", in the sense used here, has been introduced by Lévy-Strauss, who distinguished between "esprit d'ingénieur" (that is, the French graduates of the Grandes Ecoles with their mathematical bent), and the "esprit de bricolage" (quoted after Barnes [1974], p. 58, 146). I adapt the term further to describe those aspects of science and technology where the researcher scouts for whatever he can use, and is prepared to make do with what he finds.

[25] Nelson and Winter's [1977, 1982] argument focusses on "routines" and "routines to develop new routines" (in their terminology), where routines stand for ways of doing things, while routines to develop new routines are heuristics of search processes.

[26] Expectations about new scientific findings or technological possibilities are voiced, and acted upon, continually. The early promise of biotechnology has attracted biologists, R&D firms, and venture companies. The discovery of superconductivity in materials at liquid nitrogen temperatures had a magnetic effect on physicists and chemists, as well as funding agencies all over the world. Expectations, however, are not limited to such highly visible examples of early promises. They are at play already when a researcher chooses between options to follow and does so in terms of the expectation of success.

[27] The notion was introduced by Van den Belt and Rip (in Bijker *et al.* [1987], p. 140: "... there have to be expectations about the success of continuing work within [a particular] cluster of heuristics – expectations that must be embedded in the subculture of the technical practitioners and others involved in the development." They add in a footnote: "Sometimes the relevant expectations may be of a broad, even sociological nature. In the case of the French VEL project, the EDF engineers working on this particular project acted on an explicit vision of where French society was going – a vision that was characterized by Callon as a variant of Tourainian sociology. One can also think of the Japanese Fifth Generation project, which seems to have been predicated on, inter alia, Daniel Bell's ideas on the coming of the post-industrial society. Unlike Touraine for the French EDF engineers, however, Bell became an acknowledged ideologue for the Japanese "knowledge engineers [...]." See for more extended discussion and empirical cases of the cultural matrix of expectations, Vergragt, Mulder, Rip en Van Lente [1990].

[28] Van den Belt and Rip [1984, 1987].

[29] By contrast, the structural formula proposed by Kekulé in 1866 (using a theory of organic chemistry which was more correct according to our present day view) merely amounted to the translation of Hofmann's formula into his own theory but did not indicate new directions for innovation. Instead of immediately leading to technological applications, the contribution of Kekulé's structure theory must be found in the way it affected the cultural matrix of synthetic dye chemistry. It was only in 1880 that the constitution of rosaniline was finally elucidated by Emil and Otto Fischer in terms of Kekulé's theory. This elucidation was the source of a new sequence of dye innovations. (Quoted from Van den Belt and Rip [1987], p. 144.)

[30] Van den Belt & Rip [1984] note that the *Muttersubstanz* heuristic was after a time used in new areas of application, especially pharmaceuticals, and became a heuristic in synthetic chemistry in general. This shows how heuristics can be adopted more widely than for the technological development in/through which they occurred originally.

[31] Here, our terminology is the same as Dosi's [1982]. This concept of "trajectory" should be distinguished from Nelson and Winter's "natural trajectories", which are, in our terminology, stabilized heuristics. See their [1977], p. 56: "[...] there are certain powerful intra project heuristics that apply when a technology is advanced in a certain direction, and payoffs from advancing in that direction that exist under a wide range of demand conditions. We call these directions "natural trajectories". If natural trajectories exist, following these may be a good strategy." Nelson and Winter's equivalent of our technological paradigm is "technological regime".

[32] Note that there is no implication that technological developments always follow trajectories. A trajectory is a contingent happening. This point underscores my difference with Latour (compare notes 3 and 22). "Trajectory" does not imply any technological determinism. It indicates that search processes are sometimes oriented in specific ways.

[33] Constant [1984].

[34] Homburg [1983].

[35] This was recognized as such at the time, and led to problems with the patent law, which at that time required inventiveness for a patent to be awarded, so did not allow products from routinized research to be patented. The solution was to add an escape clause: if a commercially useful effect was found, the finding was still patentable (Van den Belt and Rip [1987]).

[36] This observation is related to the debate, started by Latour [1987], whether one should include non-human as well as human actors in the network dynamics. My position is that to understand technological dynamics, one should include expectations and the way these anticipate and mobilize. Voicing expectations, and being swayed by them, are limited to human actors. In fact, Callon (in Callon *et al.* [1986] and Callon [1987]), in his notion of actor-world as a scenario created and pushed by a human or institutional actor (in his case, Electricité de France) is using this notion already. Electrons can act, are actors (or agents) in that sense, but cannot interact on the level of anticipations and legitimations.

Actor-network theory focusses on processes, as if legitimations were irrelevant. This is a useful antidote to the exclusive attention to the cognitive level, but neglects the search and expectation part of technological dynamics. One could probably describe technological developments over time with curves for the (related) developments on the process/network level, and on the cognitive/legitimation level, and indicate where the one was dominating the other. It would then become an empirical question how important the two aspects of technological dynamics are.

[37] "When a core concept has emerged, it becomes the top of a design hierarchy. Standardization of product design changes the basis of competition. Battles in the market place are no longer fought over the kind of thing a product is or even the kinds of things it should be able to do. The locus of competition shifts to what the product costs. (...) This is not to say that product innovation disappears entirely or that it ceases to be of value altogether. The point, rather, is that what product innovation there is tends to be localized toward the bottom of established design hierarchies and, as a consequence, to enjoy little market visibility' (Abernathy *et al.* [1983], p. 24). See also Clark [1985].

[38] Dosi [1984], pp. 93–94, 194–195.

[39] A similar observation can be made about the Starnberg model of development of sciences which has been used to derive policy implications, for example the differential possibilities of orientation towards social relevance in different phases of development (Schäfer [1983], Rip [1981]).

[40] Van den Belt and Rip [1987]; see also Schot [1991].

[41] Genentech, the first of such firms to offer its shares on the open market, was introduced to the New York Stock Exchange in October 1980. Its issue price was $35, but prices jumped to $89 in just one hour, to stabilize at $75 by the end of the first day. This phenomenon has been quoted often, while much less notice has been taken of the fact most of the increase disappeared later on: by December 1980, Genentech stock stood at $45.

[42] The field level is less visible in another locus for negotiation of expectations: inside firms. It is difficult to make general pronouncements, because not many cases have been documented yet. One example is the way the strong fibre Twaron was developed within Akzo Company. At one moment, the board of directors decided to stop the development, but the network of alliances and expectations lower in the firm had become so strong that work continued (and achieved success in the end). See Mulder and Vergragt [1988].

[43] Cf. Bijker, Hughes and Pinch [1987].

[44] The example of engineering design will indicate the possibilities of this approach. A design or a blueprint is not just a set of instructions, a virtual artefact as it were, but also a story about how functions can be fulfilled and specifications can be met. The story must be convincing to move others into realizing the artefact (or an artefact more or less like the design intended); in addition, chances for its eventual survival should be increased. Thus, a good design has to take further requirements into account: the mobilization of varied resources for its realization and survival. It is a matter of heterogeneous engineering to develop working technology. Law [1987]

Engineering design is also the place where one should anticipate on future niches and future expansion. If all this is taken up in the "story telling", technological development would become reflexive, i.e. self-conscious about its dynamics. In practice, however, there are severe limitations. For one thing, there is too much division of labour, which induces modular optimization. Computer-aided design, an important tool, helps to experiment with meeting specifications, but lets designers forget about heterogeneous engineering. The danger is that so-called path-dependencies, unavoidable in quasi-evolutionary developments, will become counter-productive. Soete [1988], p. 56, quotes Arthur and David for an example of locked-in technological development due to path dependency: the QWERTY keyboard of typewriters and almost all keyboards, including those of computers, which is not the most efficient outlay, but which has resisted attempts at change. The introduction, in the late 19th century, of this kind of outlay of letters was related to the requirement of slowing down the speed of the typist, to avoid the hammers getting cluttered all the time. See David [1985]. In other words, rationalization of design should not just be optimalization of particular bits and pieces, but take process rationalization as starting point.

[45] This section draws heavily on Disco, Rip and Van der Meulen [1992].

[46] Compare Vincenti's discussion of normal design in relation to normal configuration of a device, e.g. in Vincenti [1991], and Vincenti, this volume.

[47] Pambour's theory of the steam engine is a good example, also because it was widely used to improve local design work (see Kroes, this volume). The attempts, for example by Reuleaux in the 1860s and 1870s, to formulate a general theory of machines are almost too abstract. In mechanical engineering curricula, however, one sees definite indications of such a generalized approach.

[48] Mathematization of design had already been prepared in France in the circles of the *Corps des Ponts et Chaussées* and among *Polytechniciens* in general, for example in the elaborate theory of suspension bridges published by Henri Claude Navier in 1823 (and which won him admission to the *Académie* in the following year) or the thermodynamic theory of steam engines published by Sadi Carnot in the 1820s. For Navier and the social origins of French theoretical mathematical modelling see Kranakis [1989]. Carnot's work itself is not mathematical; later Clapeyron and Clausius elaborated actual mathematization of thermodynamics.

[49] A similar point can be made (and has been made) about the importance of teaching practices, and the teaching function in general, for the emergence of theory in science, e.g. by Janich [1978], and is prefigured in Böhme *et al.* [1978] analysis of medieval cathedral building. In Rip [1982], I add to this argument by looking at technical dynamics (i.e. control of conditions and effects) within science, and show that "Increasing restrictedness leads to empirical generalizations and conceptual distinctions, i.e. "bottom-up" theory formation.' (p. 231) Generalization is important in science to spread one's knowledge claims more widely; this becomes important in technology as soon as technical sciences have created a domain for themselves, with colleague technical scientists rather than designers and practitioners as "consumers'. The point in the main text is that generalization is already stimulated by the need to spread one's trainees more widely.

[50] In radio engineering, on the other hand, the utter novelty of the technology implied a diffuse and uncertain theoretical base and the consequent dominance of practice over theory. Technical models tended to be intuitive or at best highly speculative and, such as they were, to be generated mainly within firms manufacturing wireless equipment and within state agencies with mandates in this domain. Only after radio technology began to stabilize in the mid-1920s did engineering schools become significant research sites for "meta-modelling" (generally aimed at high-level theoretical models of waveforms, propagation, etc.) and did reasonably elaborated cosmopolitan design networks emerge. Cf. Aitken [1978]; Disco [1990] discusses the cases of radio engineering and reinforced concrete.

[51] Buchholz [1979]; Rip and Van der Es [1980]. Also Vermeij [1990] for a glimpse of the local design work in the biotechnology of waste treatment.
[52] See Freeman [1974], for the history of Catalytic Research Associates, a consortium of the big companies that developed catalytic cracking of oil in the 1930s and early 1940s. While the meta-modelling of such technologies had gone a long way in industry, it was only in the 1960s that professors (at first, part-time) were appointed. This is probably related to a tradition of black boxing of the catalytic process (as a component or device, cf. discussion below) in chemical technology and process technology, and a renewed interest when research at the microscopic level became possible.
[53] Medicine might appear to be an exception but was, at the time, very little scientificized and instrumentalized, and in so far as it did work as a technology, could be ideologically papered over as really belonging to the realm of therapy.
The situation in the US is complicated because the traditional universities, involved in a long struggle to emancipate themselves from an ideology of teaching and doing practical work, did not want to fall under the newly created label of "applied science" (cf. Daniels [1967]), and attempted to imitate the German research university – while at the end of the century, land-grant colleges (including the University of California and other, by now well-known universities) could be established with tax relief etc. provided they worked in the public interest.
[54] It should be stressed that this academic distance from emergent cosmopolitan design networks was typical of the European continent and that other developmental patterns prevailed elsewhere. In Britain and especially the United States, where engineering schools were often tacked on to universities and where universities in any case had long experience in interfacing with practical fields (e.g. agriculture, industry, and mining) academic research developed an applied orientation early on in the game.
[55] The notion of a design hierarchy is used to structure the division of labour in specific design tasks, e.g. of an airplane (see Vincenti [1991], and this volume). It has also been used to describe the overall pattern of design in an industry or sector (see Clark [1985]), where the dominant design at the top functions like a paradigm. I introduce a separate concept, technical hierarchy, to emphasize the stratified character of modern technology in general, which allows for a generalized division and coordination of labour.
[56] It is for this reason that in Pavitt's [1984] taxonomy of innovation patterns supplier-dominated innovation comes out as one of the four main patterns. Suppliers can also take initiatives, based on their generalized position, e.g. when in the 1960s the petrochemical industry developed biodegradable fatty acids for use in synthetic detergents (to overcome environmental problems) rather than the detergent industry itself (Daey Ouwens *et al.* [1987], Ch. 4). Note that the research was of a problem-solving nature, and thus did not lead to a lasting pattern of division of labour.
[57] The quotes are from Hughes [1989], p. 2 and 471, while the description of technological momentum is taken from Hughes [1988], p. 92.
[58] The notion of techno-economic paradigm is pervasive in the chapters of Dosi *et al.* [1988]. While they do not use a linear model for the relation between science and technology, they seem to assume such a pattern for the new electronic and communication technology, as it spreads through society. Freeman, accordingly, has been emphasizing the need to invest in the implementation of these technologies.
[59] Rip [1992].

[60] Kodama [1990].
[61] Larédo and Callon [1990], pp. 166–167.
[62] Rip [1989b] refers to *IEEE Spectrum* [Oct. 1988], pp. 24–28, for the simulation link between design and manufacturing of VLSI. He also suggests that the innovation race to create ever larger chips could now be done completely on computers: war games instead of actual war. Orson Scott Card's science fiction novel *Ender's Game* offers an interesting (and moving) portrayal of the ambiguities of such a situation.
[63] Larédo and Callon [1990].
[64] There is (I think) complete equivalence with scenario building to explore futures and articulate decision making. This would explain the similarity with IIASA type model building (cf. energy, acid rain) and other mixed technical-social modeling, e.g. of transport systems. The logistics of transport systems (including their embedding) become object of technological development work.This is clear, for example, from the evolution of the relevant departments in Delft Technological University. See Prins [1987].
[65] See Rip [1990] and [1991] for a more extensive discussion.
[66] The acronym PLACE was coined by John Ziman to indicate the Proprietary, Local, Authoritarian, Commissioned and Expert nature of industrial research and a lot of present-day scientific research, and contrast this with the Mertonian "CUDOS" norms: Communalism, Universalism, Disinterestedness, Organized Scepticism. (Ziman [1983], [1990]).
[67] Professional engineering has evolved its own kind of "scientizing" over a period of a century or so; a similar argument can be made for more recent developments, e.g. in expert advice on health, on environment and on global issues. Then, it is understanding relevant to decision-making that counts. Some (and sometimes most) of the work is not made public, and if public, often in reports rather than "regular' scientific publications (compare the increasing prominence of grey literature). Still, there is recognition of performance, and accompanying rewards in terms of resources and careers. So scientists can (and will) move in this direction. Clearly, the traditionally organized R&D system is not sacrosanct, in any case not something for which many scientists will sacrifice career chances, and actually possibilities of research. And one can in fact see some "new cosmopolitanism", because hybrid institutions emerge like mixed scientific-policy conferences, organizations like the International Institute of Applied Systems Analysis near Vienna that create career resources and mobility. In this way, contributions of experts will still be visible and a functioning reward system can evolve again.
[68] The term has been used, see the subtitle of Wittrock and Elzinga [1985], but without the critical assessment required.
[69] For "technologie de base" see Larédo and Callon [1990], for "strategic science" Irvine and Martin [1984], and Van Lente and Rip [1991]. In fact, the word "technology" itself used to be such a new category: At the Chicago World Fair of 1933, the linear model ideology was still expressed as "Science Finds, *Industry* Applies, Man Conforms" (my emphasis). Use of the term "technology" is related to legitimation strategies of engineers and to the transformations during and after World War II.
[70] Cf. also Rip [1989].
[71] Kranakis, this volume.
[72] This world is described by Casimir [1983] and McCormmach [1983].
[73] Basalla [1988], p. 30, as quoted in Gremmen [1991], p. xiii.

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