SOCIETAL CONSTRUCTION OF RESEARCH AND TECHNOLOGY

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Inside the black box of science and technology

In traditional science and technology policy making, in discussion on the impacts of science and technology, and in social debates and controversies, science and technology are almost treated as a black box. True, science and technology need resources, they produce outputs, and they may perhaps be directed through judicious allocation of resources, or even programme management - but what happens inside the black box is treated as unproblematic, or only the scientist's business, or both. In contrast, I shall argue that science and technology are socially constructed, and such a perspective is necessary to understand what is happening with science, technology and society nowadays. This is a precondition for sensible and realistic priority setting.

Recent sociology of science and technology has shown, not only that a lot happens inside the black box of science and technology (that's nothing new), but that what is happening there, is pertinent to priority setting and other policy making and implementation. Thus, understanding what is happening inside the black box is necessary to choose the right priorities and avoid pitfalls. For example, the question when to believe scientists if they come up with promises of solutions for society's ills "just around the corner" - if only they are given more money, prefferably with no strings attached. Or how to resist the temptation of following the myth of the linear-sequential model of innovation and societal effects of science and technology.

There is a second reason to look inside the black box. A new kind of science & technology policy has emerged, which positions itself between the traditional patronage of fundamental research, and the emphatically mission-oriented projects like "Man on the Moon" and "War on Cancer" (both from the USA). This new kind of policy emphasizes strategic mobilization of science, and its main instrument (in Europe) is the initiation of time-limited R&D programmes. Such programmes do focus on specifiable outputs (in contrast to general patronage of science), but their outputs need not be of a problem-solving kind (e.g. reduction of cancer mortality) and will often take the form of commitments of scientists and scientific organizations to strategic goals. The establishment of new networks, e.g. among scientists and technologists and with actors like industry, is as important as the production of specific research results. The European Community R&D programmes like ESPRIT, and national programmes like Alvey in the UK are clear examples; FR Germany started such strategic R&D programmes already in the 1970s.

The objectives of the new programmes relate to processes in the R&D system, not just to research outputs, and without understanding of what goes on inside science and technology, it is a matter of luck if such programmes succeed. Policy-makers and programme managers tend to build on their experience with R&D and on the advice of scientists and technologists. By now, it is possible to offer a general analysis of processes, and perhaps even design them.

An intermediary layer in the R&D system

So what is inside the black box of science and technology? The primary process is that research practices, developement and design work, in laboratories and other institutions, produce outcomes, and recieve resources (money, people, other forms of support). The outcomes consist not only of research results, blueprints and prototypes, but also of (knowledge) claims and arguments about the promise of certain developments. For the mobilisation of resources, promises are often more important than actual results. This holds for research proposals submitted to funding agencies, for plans to establish or expand laboratories and institutes, and is visible in reports on whole areas of science and technology. Resources, and laboratories and institutes, necessary for R&D do not fall like manna from heaven. "Laboratories are constructed in committee meetings", and influencing committee meetings is an important part of the effort to do science. In addition to the research and development practices going on in laboratories and institutes, one should therefore also include committees, panels, funding bodies, R&D programmes, as part of the R&D system, and as an essential element of the processes in the black box. In fact, a whole layer of actors and institutions has evolved:

- Research councils and their peer review panels (since 1945, sometimes earlier);
- research adminitrators and research (advisory) committees in national laboratories, in universities (in the 1960s and 1970s, when R&D management became an activity distinct, and increasingly separate from the actual doing of R&D);
- special R&D programmes and their secretariats, programme bureaus etc. (since the middle 1970s);
- executive government bodies with science policy responsibilities (starting around 1960, and fully institutionalized by the early 1970s);
- quite recently, political bodies like parliamentary committees, and also TA offices, are set up.

These actors and institutions have, of course, their own interests to look after, but they have to interact, and thus form, in a sense, an **intermediary layer** between R&D and government / politics / societal demand. The interplay of actors within the intermediary layer, with R&D performers, and with political and societal actors, determines what kind of science and technology we actually get, and what we do not get.[1]

Actors in the intermediary level have mutual (and asymmetric) dependencies with the research actors and with the political system and other sectors of society. For example, research councils depend on the scientific community for the submission of interesting research proposals, which they need in order to show that there is a portfolio of high-qualitiy research on offer; such a protfolio then justifies continuation of their budget, in the face of government pressures to justify budget claims. The scientific community, on the other hand, depends on research councils to channel state money to them. In the allocation of funds, legitimated through peer review of proposals, this marriage of necessity is continually celebrated. Rules have evolved to play this 'research funding' game, and the players recognize each other's roles, as well as the stakes of the game, and try to increase their returns.

The point I want to emphasize is that such a game stabilizes, and because of that, exerts influence on the kind of sience and technology we get. For example, funding decisions of research councils do not favour innovative research: Partly because peer reviewers and council administrators do not like to take risks, partly because scientists do not submit innovative proposals. Another example is the difficulty to mobilize funds for big programmes in such a dispersed system: apart from the obvious barriers (effort to create sufficient coordination; opportunity costs, where many small grants are threatened by one big block grant), the stability of the system depends on the continuity of the rules and the willingness of many actors to continue to play.

Another example is how R&D programmes have to be implemented by creating some commitment of scientists and technologists to its goals. Once established successfully, and being embedded in a network of actors that constitute an 'Implementation structure', programmes, and in any case the networks of committed actors in the implementation structure, want to survive, and for this reason alone become a barrier to the introduction of new programmes. Nuclear energy R&D was so entrenched in groups and institutions, that it was almost impossible to introduce alternative energy R&D programmes successfully during the 1970s. The skills of practitioners, the interactive networks and vested interests of institutions, all were oriented toward the earlier, nuclear "game".

The notion of 'game' that I use here to describe and explain how R&D activities are being shaped through the intermediary layer has the added advantage of reflecting the two sides of the situation: In a game there are the oppurtunities to create commitments and productive arrangements, e.g. to use state funds to support science, or to mobilize R&D capability for new priorities; but there also constraints and limitations that derive from the particularities of the game and the need for its continuation.

Thus, we recognize one way in which science and technology are socially constructed: through the interactions between resource strategies, institution building, and the network relationships of actors, which have effects on the kind of science and technology that is actually produced.[2] In addition to the social-institutional construction, there is also social construction at the cognitive side, and as I will show, this an essential part of whatever game evolves.

Search processes, heuristics and expectations

In general, research and development practices should be seen **as search processes**. The search can be theoretical, empirical, experimental, oriented toward a knowledge claim, the realization of an effect, the shaping of a design or of a construction. These categories are often not easy to separate out, and one may create artifical distinctions this way. Think of high-temperature superconductors: the search is for new materials, as well as for ways to produce them, as well as make them utilisable (e.g. in thin films), but also for ways to understand why such materials are superconductors. All this can occur within one research practice, or there can be some division of labour. To capture the essential forward-looking, uncertain but highly oriented nature of research, the comprehensive term 'search process' is particularly suitable.

To understand the dynamics of search processes, it is useful to introduce the notion of **heuristics**: guideliness that structure search, and promise success without guaranteeing it. In superconductor research, the guideline may be to use a paticular type of crystal structure, in which one compenent is varied; and to vary particular parameters in the sintering process. In synthetic organic chemistry, we have traced the emergence of heuristics was "Look for the **Muttersubstanz**": the rule to search for the carbon skeleton, often with some essential functional groups of the interesting compound that counts as the exemplary achievement (say, a dyestuff, or a medical drug, or a compound with curious properties). If this is found, variations on the theme of the **Muttersubstanz** will then probably produce further interesting compounds. This heuristic has become the guideline for much further work in synthetic organic chemistry, and can count a large number of successes.[3]

The notion of heuristics is also used at the individual level, e.g. by cognitive psychologists and by artifical intelligence scholars, to describe strategies that reduce complexitiy of problems in a productive way. Heuristics in scientific and technological search processes will indeed originate (or be taken up) at the individual level, but the point is that they become shared, form part of the subculture of a scientific specialty, or of a technical community, or sometimes of just one organization ("This is how we do things here"). In fact, there is a social side to reduction of complexitiy: new approaches are offered to others for approval, are taken over of promising, and become accepted, thus creating a situation where practitioners know what to expect from others, what a "well-formed" search process will generally look like. (Note that this provides a reinterpretation of the notion of "normal science" introduced by Thomas Kuhn). The search processes in science and technology are conducted against the backdrop of a repertoire of heuristics: a collection of rules how to act, that not only may be followed in the expectation of success, but also **should** be followed if one belongs to that particular subculture.

The important implication of this perspective for my overall argument is that heuristics are coupled to expectations of success: in making a new superconducting material, in synthesizing an interesting organic compound. And the success may be defined explicity in terms of social/societal relevance: a better therapy for cancer, speciality polymers that overcome limitations of new materials for certain applications, etc. In these examples, the promises are global, diffuse. But looking at research more closely, one sees also concrete expectations. In industrials research and technical sciences, for example, in terms of the ability to achieve certain specifications, e.g. a polymeric material with better cable-insulting properities than the usual, celulose-(i.e. paper) based materials. Such expectations are translated into specific search processes, for example in a research project and the way researchers anticipate on judgements of research directors and other important actors.

The coupling between heuristics and expected success works also in the other direction: Because the heuristics are related to a social goal, the outcomes of the search processes will, in principle, be relevant to those goals. I am not saying that there is a pre-established harmony between research and social goals, but that there are "mechanisms", i.e. socio-cognitive pressures and rules, that make it possible for research and social/societal goals to be coupled strategically. Whether goals are actually achieved, depends on other things - including luck!

Let me take a further step, and look at how expectations of success are also important in the acqusition and mobilization of resources: such expectations can be rephrased for external consumption as relevance claims and early promises. Take a research proposal submitted to a research council or a programme body. It has to claim scientific (often disciplinary) and/or social (programme) relevance, and can only do so by raising expectations. Some proposals claim a lot, others are more modest; this is a matter of style, of tactics, and of what appears acceptable to reviewers of proposals and to decision makers. So the repertoire of shared heuristics (with the promises of success that go with them) has a second role to play; it is the backdrop against which research proposals are being judged as to feasibility and the extent of their relevance. The reviewe can say: "I don't think it can be done this way", or : "It can be done, but it will not lead to anything interesting for the central problems in this area." When challenged to justify such assessments, he need not fall back on his personal opinions, but can refer to accepted notions of what leads to which kind of success in the particular area.

It is on the basis of such a shared repertoire that aquality control is exercised. At the same time, the diffusion of a heuristic is also how fashions emerge and bandwagons start to ride. As soon as apromising heuristic to make high-temperature superconductors emerged, based on the exemplary achievement of two researchers of IBM Switzerland, others could follow it in their search practices, and get support for proposals in which such an approach was specified. Thus, particular approaches will be exploited intensively, while others will be relatively neglected. In one and the same movement, an agenda is created for research, and is it implemented, because of the coupling between heuristics and expectations on the one hand, and the decisions of actors and the allocation of resources in the R&D system on the other hand. The policy implication of this phenomenon, or aspect of the workings of the R&D system, is clear: if policy makers, or other societal actors, can get their issues (say, energy, or environment) on such internal agendas, implementation is no problem. But it is not easy to influence these dispersed, dynamic processes, even from within science.

Superheuristics and examples of games

Expectations and promises play a further, though more diffuse, role when scientists offer general claims to society about whole areas of research. For example (as noted above), "a cure for cancer", or, less ambitiously and thus risky: "molecular biology will give insight in basic mechanism of cellular development, and will therefore be the royal road to attack the cancer problem." There is a lot of strategizing in such claims do have consequences for the direction of molecular-biological research. When funded with reference to such claims, something has to be done to maintain credibility.

A good example of this mechanism can be found in recombinant-DNA research in biotechnology. The concern about risks of such research in the middle of the 1970s led proponents to make exaggerated claims about the "goodies" that were to be expected: new wonder-drugs, "green" production processes, and solutions to world food problems. Credibility pressure forced recombinant-DNA researchers to orient themselves to such goals to some extent, independently of direct commercial reasons that also played a role (but less importantly, and as it turned out, only in agricultural biotechnology, and for a few pharmaceuticals). So a **super-heuristic** emerged: a guideline that orients research choices, rather than search processes directly. In this case: orient your work to proteins (or proteins plus sugars) that have a recognized role in human (and animal) physiology, and you will be successful, at least in acquiring resources.

There are other super-heuristics in this domain, for example the pathogenicity principle (when genetic make-up is altered, the risk of creating a pathogenous organism is determined primarily by the nature of the host organism, not by the inserted DNA). The general acceptance of this principle (for which some exemplary are indeed available) allowed the trajectory of recombinant-DNA research to continue. Not only because researchers used it to design (hopefully) safe experiments, but also because the principle became embedded in guidelines and decisions of regulatory bodies, and in the arguments to defend the research against concerned governments and publics.

One can, in fact, describe the situation as one of mutually dependent actors and institutions, whose interactions are channeled by these super-heuristics. Thus, again a strategic game has emerged, not primarily institutional, as in the case of the funding game played in and through research councils, but focused on a specific research domain and substantive questions of research goals and risks. Research choices are then moves in such a game, and the research outcomes we get and do not get are socially constructed, in the sense that they are partly determined by the nature of the game.

Other such games can be identified. One very interesting example for my argument is the case of very large scale integrated circuits (VLSI) and the innovation race between firms and the block of the "Triad" Europe, Japan and the USA.

Since the first integrated circuits, from 1959 onward, the denseness of the components ("gates") and the speed of operation have increased continually, while production technology and yields have improved so that the price per gate has gone down, and at a fairly regular rate of 30% per annum. Moore´s law, the prediction that the number of components of an IC doubles every year, is often in articles and presentations, and curves for actual performance are compared with the predictions - and it turns out to work surprisingly well. In 1977, Moore´s law was used to predict that the Megabit memory chip should appear by 1991. Thanks to heavy investments of the Japanese semi-conductor firms, and their follow-up competitors Siemens and Philips, limited production of Megabit memory chips was occurring already in 1989.

A curious aspect is that Moore´s law is used as if it were a natural law. But the fact that it represents actual developments pretty well, must be a social construction. IC development cannot occur without intellectual and technical commitments, mobilization and allocation of resources, pressures to improve production skills. If these lack (or just lag), Moore´s law will fall. But the law does not fall, because if functions as a superheuristic in the game of IC development: actors refer to the law, not only to impress an audiance, but also to check if they are "up to make", and if not, they will put more effort into achieving that mark.

The game of IC development contains other rules than Moore´s law has a broader agenda. For example, the discourse of "next generations" (of IC), the time necessary to create this next generation, and the possibility of jumping a generation (with the attendant risk of not being able to master the technology) in comparison with the progress that other actors are making. Critical problems are defined against projections into the future: because one expects the size of the components to decrease further, one can predict that physical limitations will have to be faced, e.g. because the dielectric constant of silicon dioxide sets limits to the size of the storage charge, and thus to the reliability of still smaller components. In anticipation, other materials are explored, and other techniques developed (some of them, like trenching, were tried out before). Thus, the search processes are directed to overcome barriers to the continuation of the trajectory. Success of the search then allows continuation of the game as played according to the existing rules.

In addition to the cognitive-technical reinforcement of the IC game, there must be institutional reinforcement. One factor is the emergence of high-tech conglomerates, in which firms, R&D actors (institutes, professional groups) and governments participate. In Japan the VLSI project, in the USA Sematech, and in Europe the Megabit project (Siemens, Philips, the West-German and Dutch governments) and now JESSI (to mention just the visible tips of the icebergs), are built upon expectations about IC developments, and drive this development further. External reinforcement derives from the definition of the situation as an Innovation race, and the attendant rhetorics. In the early month of 1989, the German Minister for Research and Technology, Riesenhuber, argued the case of JESSI by warning that we should otherwise become economic slaves to Japan. The effort within JESSI will then , at least partly, be oriented to stay with the Japanese, and this will require assessments of the effort needed to achieve the "next generation", and thus maintain the shape of the game.

This is not to say, though, that the IC game will continue forever. Apart from the physical and technical limitations, there are also resource limitations (opportunity costs will become very high with the increasing size of investments in further development) and credibility limitations (in general, and because participation of public actors requires different legitimations for the IC game). Actors are prepared to come up with very general statements - like "integrated circuits assist in solving the world's most important problems: mitigate unequal distribution of food, energy, information; avoidance of global conflicts." And there is a need to show relevance concretely every now and then; for the IC game, High Definition TV is one such possibilty (which is creating its own strategic game now), and the renewed turn to the military may be another. Whatever one's political or ethical assessment of such possibilities may be, the point is that strategic games, even the seemingly autonomous IC game, are open to societal processes that imply some priority setting.

A dynamic and interactive three-level system

What I have tried to show with these examples and their analysis in terms of games, is how the social construction of research and technology can be described and understood, by looking at actors and institutions, and at heuristics and expectations, as they function in a complex system of interactions. The complexity may be reduced by distinguishing three levels. One level is that of search processes in institutions, linked to scientific and technical fields. A second level is that of intermediary actors and interactions, including reviewers of proposals, expert advisers, funding bodies, R&D programmes and bureaus, conglomerates. And thirdly, wider society with its industrial companies, government agencies, medical and agricultural sectors, politics and publics. There are no sharp boundaries in this system, and new actors emerge, for example R&D companies in biotechnology, high-tech conglomerates especially in the micro-electronics sector, evironmental consultancy firms, all of which have a broker role between the search processes and the wider society. There are obviously also direct linkages, e.g. between search processes in an Industrial R&D lab, and the industrial company owing the lab. But heuristics and promises cannot be controlled by the company directly, and in terms of its own interests.

In such a three-level system, two kinds of construction of research and technology can be distinguished.

(1) Social construction (soziale Konstruktion), among scientists, researchers, scientific and technical fields and their institutions, and the actors and rules of the intermediary level. The

example of the research councils discussed above shows the social construction at work, while the emergence of strategic science and the "relevance game" is an indicator that further construction is going on. (This kind of social construction is the enlarged version of what sociologists of science have been calling 'social construction of science'.)

(2) Societal construction (gesellschaftliche Konstruktion), between society and the intermediary layer. Strategic R&D programmes, for example, are a new feature of the intermediary layer, and link up with government agencies and industrial (or medical, or agricultural) actors on the one side, and with scientists and technologists on the other side. In one and the same movement, scientists and technologists capture these programmes, make it part of their resource mobilisation and judegement practices, and are pressed into developing new strategies, come up with new claims, follow new rules, and interact with new actors.[4]

Implications for priority setting

There are implications to be drawn out from this perspective. Some will not be too different from what experienced practitioners in the R&D scene have learned by trial-and-error - and this is as it should be: we are talking about the same world -, but have the advantage of being systematic, and thus a basis for design of action, at least in principle. The setting of priorities and their implementation, for example, can now be understood as a process of aggregation and then disaggregation of interests. Articulation of a priority requires experts, who often have been involved in putting the priority on the agenda by voicing expectations, and will always anticipate on later implementation and the scientific and institutional set-up that results. At the same time, different interested parties, including various government agencies, must be aligned so that a programme statement can be made up (with the right mixture of vagueness, political appeal, and promises to satisfy special interests) and will be authorized. As soon as this has been achieved, implementation and execution of the programme requires disaggregation (who is going to do what? and why?) and the earlier tensions and divergences reassert themselves. Each particular priority programme has of course its specific location in the overall system, from which it derives its dynamics. The general point about aggregation and disaggregation of interests serves to highlight the importance of analyzing, and to some extent, designing the programme so as to take expected dynamics into account, instead of falling into the trap of rational, top-down policy making and being faced with implementation and commitment gaps.

This example leads on to my first main implication: how to design R&D policy in the complex, interactive system. The science and technology policy maker is one actor among many. The policy actor may have a special position, because he is not, or not necessarily, bound to particular science and technology interests;[5] but the policy actor cannot simply impose goals. So the linear-sequential model of policy formulation and implementation (That was called rational policy making in the case of priority setting) is at best irrelevant.

Recognizing the social and societal construction of research and technology does not, however, by itself enable the policy actor to introduce changes. The term 'construction' indicates that it is man-made, but not necessarily that there is conscious design, and even if there is some design, that the policy actor can influence effectivly. The problem for the policy maker is not that he has insufficient power: even absolute power cannot force dynamic interactive systems. Nor is his problem the principle and the practice of the autonomy of science that should be maintained; research is heteronomous through and through. His problem is to control without command.

The advice to the policy maker that follows from the perspective I have developed, is: try to influence the games that are being played. When you know their players and rules and dynamics, you can modulate them, by introducing new dependencies, by favouring some heuristics and superheuristics, by creating incentives to invest. Policy analysts have dubbed such approaches 'backward mapping' (define where you want to be in the end, and design a sequence of steps that will shift dynamics in that direction eventually) 'orchestration' (put actors and rules on the map so that a new game becomes possible that - hopefully - works in the right direction). It is important to add that joint learning should occur: there is a cognitive aspect to science and technology policy that should not be forgotten. Policy makers should not just define a goal autonommously, but relate to expectations about research and technology, and their (continual) assessment.

The second main implication of the perspective is that priority setting and implementation need not be limited to policy actors at the top. The intermediary layer in the system has a broker function between the research and technology supply (or actually: promise) and the societal demand (often only articulated in relation to supply). There is a variety of actors in the intermediary layer. Some see themselves as brokers, e.g. Dutch sector councils, and advisory bodies in general, but also spokespeople for science and technology, who point out possibilities and opportunities of science and technology in relation to social goals (and the possibilities for resource mobilisation that go with them).Others are forced to become brokers, because of their position in the system. Research councils, for example, have taken up societal priorities and their implementation in the science system (the Deutsche Forschungsgemeinschaft less so than most others).

New actors are becoming involved as well, because of the strategic mobilisation of science and the way researchers themselves play the new opportunities. Environmental issues provide many examples; one could argue that in the past the same dynamics can be seen in the scientification of the medical sector.

Thirdly, events and measures in other countries, findings and claims of science and technology (e.g. about potential damage to the evironment, or the application of a new effect or material) create decision pressures on the system, which force actors to respond. They will get questions, both publicy and in private, about what they will do or will not do, and have to come up with answers. A striking example is how those Joint Research Centres of the European Community working on nuclear energy, especially hot fusion reactors, had to stop their work and investigate the possibilities of cold fusion after the announcement, at a press conference, of its possibility. In this case, it was a wild goose chase, but things might have turned out differently.

So one sees articulation of possibilities, both at the "supply" and the "demand" side, at the same time, and as part of the same process in which interactions and interrelations among actors occur.[6] This creates dependencies among actors, and forceful repertoires, of heuristics and superheuristics, and definitions of players and games, which shape search processes, outcomes and their utilisation. The separate broker role of the intermediary layer of the system also implies that repertoires, superheuristics, and games are created more consciously. There are still no easy recipes for science and technology policy, but one can hope that reflexivity will make developments more rational and responsive. With a little bit of help of policy actors and other actors pursuing a general interest, of course.

References:

1 Compare the conclusion of Mayntz and Scharpf's chapter in this book. My general point is, in a sense, the sequel to their analyses of the situation in the FRG.

2 This is also Weyer's point. In his chapter in this book, he emphasizes the possibility (which is exemplified in his case of space technology), that technology has no value of its own, but is regarded by the co-players as a means of restructuring the social arena in a way that serves one's own interest.

3 The data and points made here, and in other places in this chapter, can be supported with detailed references to relevant literature, which are available from the author.

4 The chapters in this book by Weyer und Lorenzen discuss examples.

5 For ease of reading, I write "he" rather than "he or she". As Mayntz and Scharpf also note, there are very few woman policy makers in science and technology policy.

6 See also Kodama's chapter in this book for examples.