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Linking Theory and Practice: Introduction

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The articles assembled in this special issue of 'Research policy' are based on a set of papers originally commissioned for a conference held in November 1997 at the Max Planck Institute for the Study of Societies in Cologne. This conference, organized by the editors of this special issue, dealt with the mechanisms that link scientific research and the users of its products, with special emphasis on the consequences this has for the cognitive development of science. This is not a new topic. Since their beginning, science studies have probed into the linking of theory and practice in modern science. Different perspectives in the philosophy, history, and sociology of science as well as in economic theories of innovation have highlighted manifold facets of their relationship. In the first part of this introduction, we will review some especially influential approaches in science studies to the issue of linking theory and practice—the Mertonian/Popperian alliance of the fifties and sixties, the finalization theory of the seventies, and social constructivism which dominated the eigthies, and is still with us today. We will show that each of these approaches has different but equally serious weaknesses which do not allow them to deal adequately with our topic. In the second part of the introduction, we will turn to the contributions in this special issue and argue for a new approach to the old question of how demands of practice influence scientific development.

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1. Earlier Approaches

Disregarding mostly philosophical reflections on our topic which started with Francis Bacon at the latest, we turn immediately to the peaceful but, from a sociological point of view, restrictive division of labor between a Popperian philosophy of science and a Mertonian sociology of science. This division of labor which was established in the forties dominated the fifties and sixties. It rested on the descriptive and prescriptive postulate that scientific truths are the result of scientific work, but are not shaped by the social structures and processes of this work. In modern society, so the argument goes, the institutional structure of the 'republic of science' (Polanyi, 1962) and its differentiation from other societal spheres has led to the emergence of a 'context of justification' which is strictly separated from the 'context of discovery'. What counts as a scientific truth is determined in the 'context of justification', and here only theoretical arguments and empirical data count. Speaking metaphorically, all social noise is silenced, to hear only nature's voice. The Mertonian sociology of science then saw its task in the empirical identification of the institutional conditions of this exceptional accomplishment, and found them expressed especially in the social norms guiding scientific work (Merton, 1942). Thus the Mertonians justified sociologically the philosophical claim that there exists a 'third world' (Popper, 1968) of scientific truths which can be reached by scientific discourse if (and only if!) it proceeds according to Merton's norms. Only if this is guaranteed can scientific truths differentiate themselves from everyday knowledge and all kinds

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of ideologies, and become a superior kind of knowledge.

According to this view, social factors within and outside science do influence scientific work, but only in the peripheral ‘context of discovery’. For instance, whether any discoveries are made at all in some research field depends, among many other things, on the amount of funding for this kind of research; but whether a given finding is not just a hypothesis but true knowledge is in the end decided by nature, which uses as its mouthpiece those scientists who do their collective work according to Merton’s norms. Thus, for scientific truths Merton renounced all claims which the sociology of knowledge otherwise makes to explain ideas, world-views etc. by social factors. Mertonian sociology of science strictly respected and legitimated the ‘context of justification’ as the ‘no trespassing’ core area of scientific work, guarded from invaders by Karl Raimund Popper and other philosophers of science who declared scientific truths to be a very special, indeed unique kind of human knowledge.

In principle, this division of labor between the philosophy and the sociology of science did not forbid the Mertonians to study topics of research policy and other external influences on science, as long as such influences do not reach into the ‘context of justification’. Still, due to historical coincidences Merton concluded that the best strategy for the take-off of the sociology of science as a new sociological specialty was to avoid topics concerned with extra-scientific influences on scientific work (Ben-David, 1970; pp. 414–420). In the thirties and forties, there were hot debates about the political guidance of research. John Bernal especially was an influential spokesman for a strong research policy which should tie science to the needs of society. Later, the success of the Manhattan Project and other similar big science projects in a number of countries were used as arguments for such propositions. This provoked scientists to defend curiosity-oriented basic research. To become involved in these political, often highly ideological debates surely might have been harmful to the cognitive consolidation of the still maturing sociology of science. So Merton chose to delimit its analytical focus of attention to politically uncontroversial subject matters. Accordingly, the Mertonian sociology of science devoted its attention mainly to the internal operations of science. [1] As a consequence, empirical studies of research policy and other outside influences on science which have become more numerous and important since the seventies could not get any useful inspirations from the Mertonian sociology of science.

At the beginning of the seventies, this approach was partially revised by finalization theory which was developed by Gernot Böhme, Wolfgang van den Daele and Wolfgang Krohn (Böhme/van den Daele/Krohn, 1976; Böhme et al., 1983). Picking up ideas of Thomas Kuhn (1962), they distinguished between different cognitive phases of the development of research fields. For the first, pre-paradigmatic, and the second, paradigmatic phase, finalization theory basically agrees with Mertonians/Popperians. External influences on scientific research are considered to be detrimental to the cognitive consolidation and success of a research field. Finalization theory paid special attention to empirical cases where the normatively prescribed autonomy of science was disregarded, either by scientists or by extra-scientific actors who were too early preoccupied with potential applications. Where finalization theory and the Mertonians/Popperians differed decisively was in their view of the third, post-paradigmatic phase of research fields. According to finalization theory, science is open in this phase to external influences; in fact, scientists in this phase *need* external demand to choose among alternative cognitive paths which are equally possible and interesting from the scientific point of view. Thus, demands from practice serve as priority setters for theory development.

This view, at first based mainly on historical studies of physics and chemistry, highly provoked traditional defenders of scientific autonomy in the science establishment who were supported by the still strong Mertonian/Popperian alliance in science studies. Such political controversies almost prevented a serious, calm scientific discussion of finalization theory. Later, in empirical studies of contemporary research fields such as energy research, environmental science, or cancer research and their direc-

[1] How strong such ideological pressures on this approach were became apparent again in the sixties and seventies when the Mertonian Joseph Ben-David (1977) tried to defend the ‘republic of science’ against political guidance.

tion by political and economic influences, the basic insights of finalization theory were corroborated (van den Daele, 1979), but also very much modified and differentiated. As a consequence, the initial clear-cut design of this approach became much more diffuse. Nevertheless, it was now no longer possible to deny that, at least in certain phases of the cognitive development of research fields, extra-scientific influences on research agendas and scientific concepts do exist and are not necessarily harmful to science. Some institutional aspects of these linkages between theory and practice were also included in these empirical studies, but only in an ad hoc manner so that no systematic conclusions about the channeling of user demands by institutional structures could be reached.

Finalization theory was the first strong attack against the Mertonian/Popperian alliance. The second attack which practically eliminated this alliance as an influential approach in science studies was launched a little later by social constructivism. In hindsight, Karin Knorr-Cetina, one of the leading proponents of this new approach, declares about its aims: ''... analysts began to readress not only the surrounding institutional circumstances of scientific work, but the 'hard core' itself: its technical content and the production of knowledge'' (Knorr-Cetina, 1995a; p. 140). Social constructivists wanted to break the taboo to trace back scientific truths to social structures and processes of scientific work. Again, Kuhn's work was the most important inspiration, this time with its suggestion to subject science itself to a sociology of knowledge perspective. As Knorr-Cetina and Michael Mulkay (1983; p. 2) stated with self-assurance: ''For the first time in the history of sociological thought there was a vigorous and systematic effort to subject natural and technological scientific knowledge to the same scrutiny which has long been brought to bear on other systems of beliefs, such as religious and philosophical knowledge and political thought''. And social constructivists did a very successful job at that. They needed only a few years to destroy effectively all the myths about the special character of scientific knowledge which the Mertonian/Popperian alliance had handed down. Starting with empirical studies of laboratory work, and moving on to studies of the preparation of scientific publications and studies of scientific controversies, social constructivists demonstrated for all steps of scientific work that and how it is indeed strongly penetrated by social factors.

The social factors which shape scientific truths are not only located within the 'republic of science', but are also external to it. Scientific truths are shaped by influences from the economy, from politics, from the military, from the mass media, from sports, or from the health care system. With respect to this finding, two theoretical concepts of social constructivism are especially important: Knorr-Cetina's concept of 'transepistemic fields', and Bruno Latour's concept of 'enrollment'.

The notion of '*transepistemic fields*' refers to the fact that ''... the usual distinction of 'internal–external' draws no fruitful boundary between a relevant and an irrelevant context of scientific knowledge-production...'' (Knorr-Cetina, 1984; p. 41—our translation). From observing laboratory work it becomes clear: ''The scientists' practical reasoning routinely refers not only to specialty colleagues and other scientists, but also to grant agencies, administrators, industry representatives, publishers, and the management of the institute at which they work''. (Knorr-Cetina, 1983; p. 132) As discussed above, this fact of regular social relations of scientists with actors outside science would not necessarily contradict the Mertonian/Popperian view of scientific work. But Knorr-Cetina (1983; p. 133) claims more than that: ''The crucial question is, of course, in what sense the observed transepistemic connections of scientists are relevant to the process of inquiry. My answer is that these relationships emerge as the locus in which the decision criteria corresponding to laboratory selections—not necessarily the decisions themselves—are defined and negotiated''. And she gives the example of a grants proposal from a scientist to a funding agency: ''It has long been noted that research problems may be an 'external' input to science, for example an input defined by the research priorities a funding agency determines. Yet the funding agency usually does much more than simply promote certain goals. The proposals I looked at go through many fine-grained stages of problem translation and elaboration. It is exactly through these elaborations that scientists and financing agencies negotiate what the problem is and how it is to be translated into research selections''.

In a very general sense, Knorr-Cetina (1984; pp.

154–174) regards the 'transepistemic field' in which a particular research activity is embedded as its specific ensemble of resource dependencies. [2] The category of resources includes manifold things some of which can be acquired within science, such as empirical data or instrumental expertise from colleagues, or the opportunity to publish in an influential journal. For other resources, scientists have to turn to non-scientists—the most important one being money. By an example from biological research Knorr-Cetina demonstrates how the level of finances available to a research group at a particular point of time was one important determinant of the kind of animal chosen for experimental work. But whether these experiments were done with mice or with rats significantly changed the empirical results and the theoretical arguments based on them. Thus, finances did not simply determine whether or not some research could be done, but how the knowledge gained by that research looked like. [3] This much more profound impact of extra-scientific influences on scientific work had not been recognized by the Mertonian/Popperian view.

Thus, social constructivists point out that scientists are, in their scientific decisions about theories, methods, and finally truth-claims, influenced from outside science. But scientists are not just passive objects of such influences. Just as the other actors, scientists are able to exert social influence themselves, and this influence may reach outside science. Bruno Latour, in particular, has put the spotlight on this aspect of the science/practice relationship with his concept of '*enrollment*'. He portrays scientists as highly competent strategists who produce scientific truths—which he calls 'fact-building'—by building far-reaching alliances of scientists and non-scientists all committed to these truths according to their particular self-interests. Obviously, these strategic abilities and activities of scientists are most visible in spectacular scientific discoveries, such as Louis Pasteur's path-breaking innovation of veterinary medicine which Latour (1984) studied carefully. Scientific heroes, in Latour's view, are simply those scientists with the best abilities and opportunities to enroll other actors inside and outside of science in their own projects to propagate certain truth-claims.

In his study of Pasteur's success story, and in a major theoretical work on 'science in action' (Latour, 1987), Latour specifies 'enrollment' as the most important strategy of an active pursuit of credibility, or scientific reputation, by scientists. 'Enrollment' basically consists of a translation of interests. A scientist who wants other scientists as well as politicians, military leaders, businessmen, farmers, doctors, or other societal groups, to become his allies in the creation and diffusion of a particular piece of scientific knowledge has to convince them that it is in their well-understood self-interests to support him. As a minimum, others have to be reassured that the scientist's project does not impair their interests, so that they need not oppose it.

Latour's use of the concept of 'enrollment' has a tendency to portray a single scientist as a lonely hero who enrolls others in the pursuit of his interests. As Joan Fujimura (1992; p. 171, italics omitted) rightly remarks with respect to Latour's study of Pasteur: ''While he also demonstrates that other actors enrolled Pasteur's microbe in their efforts, Latour's focus is primarily on translations which facilitated Pasteur's network building''. But usually it is not just one group, the scientists, which takes the initiative and directs the others. Instead, often ''... all actors are simultaneously attempting to interest others in their concerns and objectives''. (Fujimura, 1992; p. 171) Thus, usually individual strategies of 'enrollment' are part of constellations of mutual 'enrollment'. [4]

With these theoretical concepts and their empirical examples, social constructivists certainly have paved the way for a better understanding of the manifold and intricate external influences on scien-

[2] Without referring to the resource dependency approach in organizational sociology (Pfeffer/Salancik, 1978), her perspective on scientists and research groups is very similar.

[3] As a comparable example of such deep impact of political pressures on scientific work, see Gläser/Meske (1996; pp. 369–370).

[4] Of course, such reciprocal translations of interests not only shape the results of scientific work, but also of political or economic decision-making, etc. But these extra-scientific effects of such a 'scientification' of society are of no concern here. Here the other causal direction is relevant: how such extra-scientific influences manifest themselves in the scientific production of knowledge.

tific work. [5] But this indisputable achievement of social constructivism went along with a fatal theoretical weakness. To put it briefly, *exaggerated situationism* replaced the exaggerated institutionalism of the Popperian/Mertonian perspective. But whereas the latter at least tried to find general explanations of action, situationalism by definition gives up this effort and, in the end, can do nothing but narrate what happens in a quasi-historical fashion, that is, as unique events. The first surprise attacks which were launched in this manner against the Mertonian sociology of science were quite successful and certainly necessary; but very soon this became nothing but beating a dead horse again and again. The methodological shortcoming which encouraged this theoretical one-sidedness consisted in the *single case approach* of social constructivist empirical studies of scientific work. It was always one laboratory, one discovery, one controversy which was analyzed in extenso. At first the respective empirical case was set in contrast to the Popperian/Mertonian theoretical model of science. But after the latter was successfully done away with, nothing new beyond this fact could be learned from any further single case study because nothing to compare it with had remained. The empirical findings could not be related to anything else. The description and interpretation of each case portrayed it in isolated uniqueness. This way of looking at the cases did not necessarily impose, but certainly encouraged an emphasis on case-specific situational features, and a neglect of more general institutional aspects. [6] Thus, social constructivism also falls short in the ability to handle analytically the linking of theory and practice. And even if social constructivism had cared more about detecting generalized theoretical patterns of linkages, it would still have neglected institutional variables which are of central interest to us.

2. New Perspectives

Today it is widely accepted that scientific development is open to the influence of a large variety of social factors. However, as steeply rising expenditures for research in times of financial shortages induce concern for a ‘just return’ to the public investments in science, attention has shifted to the question how industry (and other users, e.g., in public health) can obtain from science what they need in order to continue innovating and to survive in an increasingly competitive world economy. In this perspective, innovation is the dependent variable of interest, whereas scientific research is regarded as an input factor and considered only as an independent variable. The dominant concern with innovation has directed attention away from the question how the social embeddedness of science influences the cognitive evolution of scientific fields, the path actually taken by the contingent process of scientific development. This holds above all for studies on the meso- or macro-level. It is, therefore, quite timely to raise again, and this time in an institutional perspective, the seemingly ‘old’ topic of the social shaping of scientific development. The prevalence of the ‘innovation perspective’ is in fact so compelling that at the Cologne conference, several participants found it difficult to re-orientate their thinking so as to consider developments in science, not innovation the crucial dependent variable. In spite of the extensive revisions undergone by most of the original conference papers, this is visible even now in some of the articles in this volume.

The focal assumption on which this special issue on linking theory and practice is based is that the institutional forms in which, and through which knowledge production is linked to the social environment make a difference for the path scientific development takes. A parallel approach has, incidentally, been used by political scientists who have asked how

[5] Wolfgang Krohn and Günter Küppers (1987) took up many of these insights of the social constructivists in their general model of the ‘self-organization of science’. A more comprehensive reflection of social constructivism should include a closer look at this model, too.

[6] Recently some attempts to introduce comparative case analysis into social constructivism have been made, the most impressive one by Knorr-Cetina (1997) in her still unpublished work about ‘epistemic cultures’ in which she systematically compares particle physics and molecular biology with respect to a number of important cognitive and social aspects of scientific work. Such investigations are surely necessary to lead social constructivism out of the dead end street into which it manoeuvred itself. But curiously enough, not even the marked contrast between these two research fields directs Knorr-Cetina’s attention to institutional factors although they are as dissimilar as some of the factors she deals with.

different political institutions affect the development of public policy and the policy decisions emerging from it. In the existing 'social shaping of science' literature, the insistence on the sheer fact that external influences *are* important has detracted attention from the question *how* external incentives and restrictions come to influence the choice of questions for further research. How exactly, through what structures and mechanisms are external influences, explicit or implicit expectations about the questions science should attempt to answer, brought to bear upon the cognitive process? As we have argued in the previous section, this topic has been largely neglected so far.

To look more closely at the structures, formal institutions and mechanisms linking science and society does not mean to analyze everything that is generally covered by the term 'social shaping'. This term covers both intentional and unintentional ('cultural') ways of influencing the agenda of scientific research, and it extends beyond the 'context of discovery', i.e., the choice of research topics, to include also the 'context of justification', i.e., the validation of research results. It is mainly the epistemological relativism implied in the constructivist contention that there exist no objective criteria for the justification of truth-claims, i.e., that this, too, is a process of social construction, which has provoked the most acrimonious critique of the approach (e.g., Gross and Leavitt, 1994). The perspective here adopted is focused on the determination of the research agenda, i.e., the context of discovery, and the role played in this agenda-setting process by deliberate attempts to bring the expectations of users to bear upon it. This means to exclude 'cultural' influences, for instance by a gender-specific world-view or widely shared political or religious beliefs, factors that can influence the choice of research questions and also shape basic conceptual categories. Deliberate attempts to influence the course of scientific development focus on topics, or problems to be solved—cognitive problems which often have practical implications. Deliberate steering attempts, however, do not only influence the choice of research questions, but also the choice of methods and approaches, and this in turn affects the answers that can be obtained. Such steering is always selective, favoring certain topics, methods and approaches over others; this selectivity is built into the linking structures and institutions that channel external expectations towards science. The central question, then, is: to what extent, and towards what practical ends, is the process of scientific development in different scientific fields directed by the linkage mechanisms connecting, and mediating between, scientists on one hand, and potential users and their (political) advocates on the other hand.

In seeking an answer to this question it is, first, necessary to identify the different forms that the linking of science and practice can take. An important distinction is that between direct and mediated linkages. Direct linkage mechanisms bring research organizations or individual researchers themselves into contact with potential users of their research results. This may happen in research projects jointly conducted by firms and university institutes, through research contracts from users, or more informally at conferences or other occasions for meeting. Mediated linkages also can take many different forms; they range from science councils, research funding agencies, and technology centers attached to universities, to advisory bodies attached to ministries and the media.

In a systematic approach to the subject one would begin by studying the different types of linking mechanism, asking how and what kind of external demands they channel to those doing research work, and what incentives they provide in doing so. In this volume, both direct and mediated linkage forms are being analyzed in this way. While Henry Etzkovitz and the article by Frieder Meyer-Krahmer and Ulrich Schmoch concentrate on various forms of direct linkages between industry and university research, Dietmar Braun presents an analysis of the most important category of mediating institutions, i.e., research funding agencies, and Peter Weingart deals with the highly indirect mediating function of the modern mass media.

Particularly in the case of mediated linkages, an analysis of the interactions between the representatives of science and the advocates of external demands on science does not permit any conclusions about the effects this actually has on the development of science. Here an important intervening variable is the responsiveness of scientific institutions and individual scientists to external demands. This responsiveness is influenced by many factors: features of research organization, modes of research financing, career patterns in science, and even the professional socialization of scientists. Cognitive factors, too, play a role here. Important as the responsiveness of science is for the outcome of given linkages, an analysis of mediating institutions per se will rarely be able to assess it. For this, a research design would be needed that includes the performance level of the science system, something that is much easier where direct linkages, as between university researchers and industry, are the subject.

Detailed empirical knowledge of the way different linking mechanisms operate also permits to identify changes over time, e.g., in the kind and intensity of university–industry linkages or in the orientation of funding agencies. But we need to move to the macro-level of what one might call the 'linkage regime' of a given country in order to get an idea what the aggregate effect on scientific development of all the different linkages working together might be. These 'linkage regimes' differ in significant ways between countries, as particularly the articles by Barend van Meulen and Arie Rip, and by Pierre Papon in the present volume demonstrate. One important difference is the extent to which a layer of intermediary (and mediating) organizations that is institutionally separate both from the operative level of research performance and from the agencies of science policy exists in a given country. Mediating organizations also differ in their orientation. Thus Dietmar Braun distinguishes scientific, strategic (sectoral) and political funding agencies; the first type caters above all to scientific excellence and leaves the determination of research priorities largely to representatives from science, the second type promotes research useful for specific fields of practice (e.g., agriculture, health, nuclear energy), while the research agenda of the last type is directly determined by political considerations. Though the evidence presented in this volume does not permit a systematic international comparison, countries clearly differ in the prominence of mediating institutions and in the relative importance of the three types of funding agencies. This has consequences for the degree of autonomy the science system enjoys in defining its cognitive agenda, and for the demands brought to bear upon scientists in deciding their research agenda.

A close look at the total 'linkage regime' of a given country should also give hints with respect to user groups whose knowledge needs are not effectively communicated to science. In fact, however, a study of linkage structures and institutions can only show the demands that are at least articulated in the observed interactions, whether or not they are subsequently integrated into the research agenda of science; communication deficits cannot be directly observed unless one starts with a survey of all user expectations. It is nevertheless evident that the ability to pay for research favors direct linkages, as between science and industry (where industry thinks it can gain from the inputs of science). Other kinds of interest need advocates who can use public money as incentive; here the linkages will tend to be mediated by public agencies. Aside from research funding agencies, science ministries and other political authorities setting up special programmes for targeted research are a case in point. Here an important question is to what extent potential user groups are directly represented in the responsible committees etc., or whether politicians, public officials, or even scientists themselves serve as their advocates. In all of these cases it remains an open question whether the representatives articulate actual user demands, or their own perceptions of what these demands are or should be.

Empirical studies as well as some recent theorizing (e.g., Gibbons et al., 1994) have suggested that the relationship between science and its societal environment are becoming closer, that irrespective of possible cognitive barriers to 'finalization' science tends to respond more than ever to external expectations of usefulness. Several of the contributions in this volume attest to such a trend. Thus, university–industry relations are becoming more intense, and the involvement of science in the 'experimental implementation' of knowledge (van den Daele and Krohn) stimulates research on problems of a different sort—not knowledge about isolated causal relationships holding under idealized conditions, but knowledge about the interaction effects of a multitude of variables in real situations. The apparent increase in direct links between scientific research and users does not mean that mediating institutions are becoming less important. Intermediary institutions in France and the Netherlands are likewise changing by emphasizing their function of intermediation more and insisting less on the promotion of research according to criteria of excellence as defined by scientists; to this purpose, they increase the representation of user demands and introduce mechanisms to strengthen the horizontal linkage of research units to user organizations. By way of the impact that public opinion has on the allocation of funds by science policy agencies, the media also serve to strengthen the responsiveness of science to articulated societal demands.

But one should beware of applauding this trend too hastily. The example of the enforced user orientation of research in the former socialist academies of sciences rings here a warning note. Some of the critical arguments university scientists voice against the increasing dependence on industrial funds and the market-orientation of academic researchers are also well worth considering—as are the consequences of replacing scientific reputation by media prominence as decision criterion of both researchers and funding agents. Only if the demands actually articulated, and brought to bear upon science at a given time, are uncritically accepted as the normative *non plus ultra*, as embodiment of systems rationality (i.e., future public welfare), could we evaluate an increasing compliance of scientific research with such demands as positive without qualifications.

However, tightening links need not necessarily spell increasing compliance of science to articulated external demands.The collection of articles in this volume serves to modify in several important respects the rather simple notion of ‘channeling’ demands—even if filtered, or only selectively—from users, and user representatives or advocates, towards knowledge producers. This notion is uni-linear in a similar way as are the notions of ‘science steering’ (politics → research) and ‘science transfer’ (science-users, mainly industry). All three of these simple process models catch but one aspect of a much more complex reality in which they are densely intertwined. Thus the transfer model does not fit, as Meyer-Krahmer and Schmoch point out, the interaction between industry and university research, which is to a considerable extent an exchange process in which both sides are learning, and Etzkowitz shows that as university research is becoming more industry-like, spin-off firms come to resemble research institutes more than productive firms. Van den Daele and Krohn, finally, show how the problems arising in the experimental implementation of (incomplete, and idealized) scientific knowledge feed new, and a different kind of questions back to the scientists participating in, or monitoring the technological innovation process. The link between science and practice is today in all science-based areas of practice what the relationship between science and technology has been since the 17th century: a two-way street. What this implies is that ‘demand pull’ processes should never be seen in isolation. The expectations that users direct at science have themselves already been shaped by the knowledge, or the hypotheses offered by scientists. Surely utopian thinking can give impulses to scientific and technological innovation, but it is necessary to know first what can be had before formulating requests that are specific enough to direct action. There is, then, much more ‘science push’ at work even in a world where demand pull is supposed—both cognitively and normatively—to reign.

The foregoing remarks were intended to sketch an analytical perspective, not to flesh it out with empirical results. For this, the articles assembled in this special issue of the journal are not sufficient. They are only a few pieces of a puzzle yet to be composed. As always when we elaborate a new question, the partial answers we receive only serve to show up how much more we still need to know. The stereotypical conclusion of scientists in reporting on their work that ‘more research needs to be done’, a conclusion that also holds for the topic of this volume, is more than the facile attempt to secure future funding. It expresses the cognitive dynamic underlying scientific development.

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