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Science as an Agent of Change: Finalization and Experimental Implementation

Science contributes to the emerging knowledge society more than just by offering bits and pieces of research results generated in an independent realm of knowledge production[1]. Instead, science offers its mode of operation—doing research—as an integral component of innovation strategies in society. The settings of economic, ecological, social or political modernization assimilate the modes of scientific operation: experimentation, modeling, hypothetical prediction, and explanation.

By shifting the focus from the application of results of science to the application of its modes of operation we keep a distance from the model of "finalization" (Böhme, van den Daele, and Krohn, 1976; Böhme *et al.*, 1983). This model took scientific disciplines as its frame of reference and described their development with respect to the varying capacity of being responsive to problems external to their cognitive domain. More specifically, we asked how such problems are theoretically deconstructed and then reconstructed in special fields of goal-oriented scientific knowledge (finalized theories) and how this knowledge was translated into new science-based technologies. A specific bias of the model—strongly influenced by Kuhn (1962)—is its focus on physics and chemistry. Still, the road toward oriented research was taken by many disciplines and called for a better understanding of the implications for theory dynamics and concept formation. However, the model is limited in that it views only the incorporation of non-scientific goals into *theory* and considers the knowledge thus generated as a resource of social change. What has to be added is that the operational modes of research, and not just the scientific results, become incorporated into projects of innovation. Science is not only a resource but also an agent of change.

In the following we discuss some sociological and epistemological implications of the notion of science as an agent of social change. This will include a proposal to reconsider the internal-external distinction in the sociological analysis of the role of science. At variance with those authors who consider this distinction to have become futile, we believe it all the more important and necessary in order to account adequately for both the involvement of scientists in the networks of innovation, on the one hand, and for the functional differences that remain between doing research and making money, choosing between political goals, or organizing public protest, on the other.

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[1] This was the basic idea in Daniel Bell's (1974) vision of the postindustrial society with knowledge as its new axial principle and strategic resource.

We will then use two case studies from Germany—one referring to the development of the technology of waste management, the other to the introduction of genetically modified plants in agriculture—to illustrate the theoretical points we want to make in understanding the role of science in the innovation process.

1. Science as an agent of change

To view science as an integral part of processes of innovation in society is not a new approach. Social studies of science and technology have dealt with science in industry, or science and the military, or science and societal infrastructure, and have drawn attention to the networks and "webs" which take in science as soon as it operates in these fields. It goes without saying, that these studies are not confined to a narrow definition of science as a set of academic disciplines devoted to the experimental discovery and theoretical understanding of reality. Rather they conceive of science as the much broader enterprise of generating new knowledge which is not only the basis for the construction of technology and the manipulation of nature, but also a contribution to the construction of the social worlds of war and peace, money and poverty, health, education, and information. While this move to study "science in action" (Latour, 1987) has turned out to be fruitful for understanding the dynamics of modernization, the pay-offs for understanding the dynamics of science have been small—sociologically as well as epistemologically. Many studies are preoccupied with the negative finding that sociological observation reveals the absence of clear distinctions and boundaries which separate science from other activities. The observable patterns of communication in innovational fields cross the traditional boundaries of scientific, economic, legal and political discourse, and the observable patterns of organization combine heterogeneous professional competencies. As a polemic against the image of science as pure research in academic and disciplinary fields, these observations have been and still are of value. But, analytically, the keywords used in these studies to account for the observations, do not lead us very far. They speak of "sociotechnical ensembles" as an "intimately interconnected, heterogeneous ensemble of technical, social, political, and economic elements" (Bijker, 1995: 249), or of "translation networks" (Callon, 1995: 52), and of "seamless webs" (Hughes, 1986). These accounts leave us without a concept of what the relations are between the elements in these ensembles, networks or webs. The situation is further complicated by the constructivist message, which spread from laboratory studies, that "nothing epistemologically special" is happening in the pursuit of science (Knorr-Cetina, 1995: 151). It would probably be more correct to say that sociological observation of what scientists do and communicate in the laboratory has not identified anything epistemologically special—this leaves room for discussion whether this is a statement about science or about the range of sociological methods. Nevertheless, the constructivist message has been widely adopted in the social studies of science and has contributed to a "blind spot" in the analysis of what the scientific part is when science becomes embedded in the networks of innovation and how this embeddedness feeds back into science; what the implications are for theory formation, experimental strategy, methodological design, and the use of evidence within science.[2]

Some of the epistemological implications will be spelled out in the following. The most important one is probably that science which operates as an agent of change in fields of innovation will not only provide an increase of applicable knowledge, but also an increase of relevant "non-knowledge".[3] In more general terms, the application of science imports the perception of risk. We normally emphasize the capacity of scientific research to transform uncertainty of knowledge into certainty, replace conceptual ambiguity by clear theory, and turn technical impasse into manageable options. However, the more that capacity is extended to complex issues, the more we will be confronted with what we do not know and cannot control. The scientific approach to innovation is bound to reveal that applicable knowledge, generated in the laboratory, is in fact inapplicable in the world outside; that theoretical predictions are restricted to idealized conditions which have no counterpart in the real processes of innovation, and that neither the performance nor the side-effects of a technology can be anticipated or controlled with certainty in complex situations.[4] The overarching function of science operating in the "web" of innovation is not just to generate knowledge, but to relate what is known and what is unknown in complex situations of change. Of course, science as an agent of change will generate new knowledge—this is what research is all about. But on the road to that knowledge it will also accumulate new uncertainties and open questions. And the non-knowledge generated may well outstrip the relevant knowledge.

A second implication is the extension of scientific experimentation as a model for dealing with technical and social uncertainties outside the narrow confines of the laboratory. Scientists turn the implementation of new technology into experimental devices—usually under various names such as test stations, pilot installations, prototypes, or demonstration objects. That is how they try to cope with the dilemma that they are expected, on the one hand, by the public, the law and their clients, to

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[2] We omit a discussion of the conception of the "mode 2" production of knowledge (Gibbons *et al.* 1994), as differences and similarities to the finalization model are discussed by Weingart (1997) in greater detail.

[3] The German language allows the symmetrical use of the terms "*Wissen*" and "*Nichtwissen*" to denote what we know and what we do not know. Since this symmetry is not implied in the juxtaposition of knowledge and ignorance, we will use the terms "knowledge" and "non-knowledge" instead.

[4] It has often been pointed out that uncertainties about the applicability of scientific knowledge can be reduced if contexts of technology are remodeled to fit the conditions in the laboratory, for example, by shielding off the effects of environmental variables. See also Latour's analysis of the social implementation of the vaccination scheme developed by Pasteur in a scientific setting; Latour speaks of the "Pasteurization of France" (1988). However, many aspects of the innovation process will never be reducible to laboratory conditions.

apply consolidated, proven knowledge which yields reliable technology; but that, on the other hand, the very application of such knowledge usually implies new technological designs and scales, environmental conditions and organizational settings which have not been explored and therefore may lead to surprises. The scientific response to this dilemma is to turn the relationship between action and surprise into an experimental design. The cases we are going to present not only exhibit the unavoidable elements of trial and error in the process of innovation, they also show how scientists use new technology to learn more about it. The implementation of new technology may become a means to generate exactly the knowledge that is supposed to be the basis of such implementation (see Krohn and Weyer, 1989).

A third implication relates to changes in theory formation. Theoretical understanding and prediction in scientific disciplines is tailored to suit restricted and idealized model systems of the respective subject areas. It may not be a severe problem for the discipline if the theory goes wrong when it is applied to "real" subjects in uncontrolled environments. However, "external" validity is crucial for theory that is supposed to understand and be relevant for the complex fields of the innovation process. In this case, "was not covered by the model" or "boundary conditions are different" no longer count as legitimate excuses for theoretical failure. On the other hand, a trade-off must be expected in theory formation between predictive accuracy and comprehensiveness of subject areas. Complex problem definitions, which take account of the real conditions that prevail in the innovation process, may only allow theory formation which resembles less the paradigms of natural science disciplines and more the conceptual schemes developed in the social sciences. Theoretical understanding will then provide a framework for the assessment and reflection of problems and options, and only to a limited extent the predictions and techniques of manipulating outcomes. A comprehensive theory of risk ranging from the analysis of material cause-effect relations to the cultural interpretation of patterns of perception would be a model case.

As an agent of change, science becomes deeply immersed in the dynamics of the society. This will change science. However, it will not dissolve the boundaries that differentiate science from politics or economy. The question is, how the "internalism" of science can be accounted for in terms of sociology.

2. Internalism and Externalism

One of the main concerns of finalization theory was to relate internal and external factors of theory development in a dynamic and differential way. "Dynamic" because we considered translations through which external goals become *internalized* into the processes of knowledge production in science. "Differential" because degrees of susceptibility to external goals were distinguished, depending on the cognitive stage of development of the disciplines concerned. As has already been mentioned, the internal-external terminology came under attack from the social studies of science. Since case studies showed that, empirically, the boundaries between internal and external factors are permanently blurred; it was argued that the conceptual distinction was altogether useless. To a certain extent, this is exactly the point the finalization model tried to explicate. However, while we would no longer defend the assumption of the model that phases of scientific development can clearly be distinguished, which are either immune or susceptible to external goals, we still defend the basic notion that one must distinguish between what is inside and what is outside science. The very fact that the boundaries between scientific and other competencies are permanently transgressed or even blurred in innovation processes underlines the need to emphasize the analytical distinctions that still hold. This section indicates how these distinctions could be conceptualized using sociological systems theory (Luhmann, 1984).

From the perspective of systems theory, the social complexity of innovation processes would have to be "deconstructed" by distinguishing three levels of analysis: the level of functional rationalities, the level of programming decisions in organizations, and the level of personal interaction. On the level of functional rationalities, systems theory adopts the Weberian view that the course of modern society is determined by the differentiation of the codes (or media of communication) of power, money, law and knowledge, and by the development of institutional repertoires that belong to these codes and specify operations of politics, economics, law and science. On the level of rationalities, modernization means that social dynamics become increasingly submitted to processes of economization, politicization, legalization and scientification—which, according to systems theory, take place all at the same time, although they may differ in intensity and range. The differentiated rationalities become operative through *institutions* (procedures, rules, criteria, routines, devices and methods for action) that relate the codes to specific modes of communication and action. Such institutions are, for example, elections, interest rates, contracts, proofs and refutations, or publications. The institutions can be considered to form closed systems (in Luhmann's language: autopoietic systems). The legal system (and the legal discourse) is constituted by institutions of law which refer to institutions of law, which means that only the rule of law counts, not the calculus of power. The scientific system consists of the operations of institutions of knowledge—none of which can be substituted by reference to economic benefit, or legal doctrine, or the use of power. This is how systems theory reconceptualizes the internalism-externalism dichotomy. When the Weberian codes of rationality (*Sachordnungen* in his terminology) are sufficiently differentiated and institutionalized in the society they form systems of action and communication that operate with self-reference. This internalism does not mean that institutionalized systems of rationality cannot be influenced from the "outside"—there are all sorts of external demands calling for new laws, knowledge, economic valuation or power relations. But whether these demands find their way into the system and become part of the institutional operations, or change the structure of the system, depends in turn on operations of the system. Only science determines whether and how political demands can be operationalized in research (see van den Daele, Krohn, and Weingart, 1977, 1979).

When we shift the focus of analysis from functional systems to organizational structures, a different picture emerges. All organizations—markets, hierarchies, networks—operate by *combining* heterogeneous institutions of rationality in their procedures and programs of decision making, and when they account for their operations. While many organizational settings can be classified according to dominant functional goals—making profit in a business firm, producing knowledge in a laboratory—all kinds of mixtures do occur: private law firms work for profit and for law; professional lobbyism operates within politics and the economy; planning agencies operate in politics and in law. At the organizational level, operations internally related to the criteria and codes of functional systems of rationality are externally coordinated in programming strategies and decisions of the organization. But this does not at all mean that the differences between the rationalities are blurred or that they become obsolete. Big companies employ technicians, lawyers, or analysts (and small companies buy the respective expertise) precisely because it is so difficult to merge different rational perspectives into the definition of a viable organizational strategy. Or, with respect to innovation, this is why innovation networks are formed, in which the heterogeneous problems of technological feasibility, legal regulation, economic prospects and political acceptance can be addressed and integrated.

If systems of rationality are segregated because their institutions do not overlap, the question is how communication across systems boundaries is possible. The answer in systems theory is that individual actors are very good in switching between functional rationalities. They can communicate a problem in legal, economic or scientific terms. The ability to communicate different perspectives does not, of course, imply functional expertise nor organizational power. The meta-competence of communicating perspectives of communication enables actors to address heterogeneous rationalities and competencies. It may exert power in organizational contexts, where negotiations between actors about how to proceed are basic for decision programming. Even individual actors who communicate across functional systems do not blur the distinctions between the internal modes of institutional operations. They have the capacity of reflection which enables them to conceive the inside from the outside.

These somewhat abstract remarks should suffice to make our point clear: The complexity and the strength of organizational fields of action, like bureaucracies and companies (or scientific laboratories, for that matter), do not stem from blurring the boundaries between institutionalized rationalities and forming seamless webs in which rules and criteria no longer apply; but rather from integrating heterogeneous institutions into contexts of decision making, in which every thread contributes to the web because of its internal consistency.

3. The seams of science in the web of innovation

The systems theoretical reconstruction seems to contradict evidence from ethnographic and historical studies that it is quite normal for scientists to become involved in managerial, financial, political and public concern issues (Knorr, 1981; Bijker *et al*., 1987; Hughes, 1986; Wise, 1988). Scientists assess the value of scientific work not solely in terms of knowledge criteria but also in terms of economic feasibility, moral admissibility, and political acceptability. Thus reference to heterogeneous codes or patterns of rationality is an essential part of the social reality of doing science (Krohn and Küppers, 1989). If this is true, even for academic research where theory is the declared objective, it is all the more significant when science becomes involved in the processes of innovation where transforming society is the declared objective. Then, research goals can no longer be defended only in terms of science, since they reflect political preferences, economic strategy and perceptions of social needs. The "internalization" of these goals into research strategies must respect issues of regulation and public acceptance, and is often a result of negotiation with interest groups. Once science becomes embedded in the process of innovation, it becomes intimately linked to the non-scientific institutions of political choice, of economic calculus, of legal and moral assessment. However, this does not render science a mixture of all rationalities. Quite the contrary: As scientists become more tied up in the heterogeneous organizational field of innovation, the functional differences between science and politics, law or public opinion tend to become more apparent. Scientists, as individuals, may be able to bridge these functional differences and provide their own assessments of all the problems a new technology may imply. However, they will hardly be in a position to take that role in the social network of innovation. In the actual presence of professionals from policy circles, business, regulatory agencies, and public criticism, scientists cannot expect that their particular opinions on political goals, social needs, economic opportunities or acceptable risks would go unchallenged or carry much weight. Rather, the other actors in the network will insist that the sciences (and the scientists) do not have the competence or the mandate to control the process of innovation and induce social change through the implementation of new technology. The more science is submitted to economic and political criteria, the more likely it is that scientists will be confined to the functional role associated with research competence. This can be observed both in cooperative organizational networks (Kowol and Krohn, 1997) and in contexts of public controversy (van den Daele, 1996).

In general, communication among heterogeneous perspectives and interests in the organizational field of innovation will underscore that scientists lack special competence to decide on issues of public policy and social development. It will, on the other hand, acknowledge that the determination of what is empirically known and unknown rests with scientists. This is especially highlighted when experts and counter-experts meet. Thus, while all the participants involved in the networks of innovation will continue to address questions of risk, uncertainty and technological options or alternatives, it remains the prerogative of the scientist to contest and assess knowledge claims, in the final analysis. It is our observation that the more explicit the conflict over claims of knowledge, the more distinct these differences between functional competencies are kept (van den Daele, 1996).

On the other hand, since lack of knowledge is endemic in the innovation process, scientists are permanently challenged to extend the scope of knowledge, even if they must transgress the limits of what can be theoretically derived or is corroborated by experiment. Critics will often be experts in exposing the weak points and loose links in the experts' knowledge construction. Moreover, people may distrust scientists because they consider them to be too closely tied to business or state interests. They may instead trust the assessments of the speakers of social movements who they believe give fairer and more impartial accounts of what the scientific facts are. Technological failures or technology-induced disasters highlighted by the media undermine dramatically the established routines of knowledge application. But when the authority of scientific expertise comes under political fire, the battle is likely to lead to more rather than less reliance on scientific competence, because the issues are more precisely articulated and the answers are checked and double-checked. It is the virtue of procedures of public conflict resolution that they contribute significantly to this "second-order scientification"; they ensure the careful assessment of what is known about the relations between the better known and less known aspects of a project for new technology.

It is, of course, always possible to abandon an innovation because it is fraught with uncertainties. But in a society so strongly determined to play its part in technological modernization, it is more likely that innovation networks will treat uncertainties as opportunities for more research. These are the cases on which our interest focuses and to which the finalization model is to draw attention. Additional research is not just needed in the initial phases of an innovation project where the feasibility of a new technology is investigated through applied research; it is also needed when testing procedures are developed in regulatory science, when technology assessment is extended into research on the social impacts of innovation, and when *de facto* experimentation implied in the implementation of a new technology under real conditions is translated into a controlled experiment.

In the following we present two different cases from the Federal Republic of Germany in order to illustrate how science is integrated into processes of innovation. The first case, dealing with the issues of public waste management, emphasizes the stepwise "scientification" of received practices. Waste disposal practices had become increasingly intolerable in the 1970s. The subsequent calling for a better understanding of the problems and for new technical solutions comes close to a *demand-pull* case of technological change. The second case, concerned with the introduction of genetically modified crops in agriculture, is much more one of *techno-scientific push*. In the late 1970s molecular biologists rushed into the application of genetic engineering on a broad scale. Later it became obvious that they needed powerful alliances in non-scientific areas and non-scientific resources in order to provide a sound economic base and gain political support for their goal-oriented research. Both cases can be said to be *finalized* in the sense that scientific interests and orientation have been triggered by goals relevant in fields outside science. Furthermore, both cases demonstrate that the application of theoretical knowledge and the generation of experimental knowledge are equally important. Finally, both cases are characterized by the establishment of networks of corporate actors in the innovation process. But there are also differences. While the demand-pull case began with strong public trust in science-based change (and only after increasing troubles and improved understanding of the gaps in knowledge did skepticism and organized protest grow), the technology-push case generated strong opposition from the very beginning. In both cases, however, the paths of modernization led to the same state of intriguing ambiguity: science is an agent of change because it transforms ignorance into knowledge, but also because it transforms received knowledge into uncertainties.

4. Ignorance, experimentation and recursive learning—the case of waste management

We begin by summarizing the points we want to make.

(1) The technology and organization of waste management develops as a field of collective learning, in which techno-scientific competence becomes increasingly important.

(2) The experimental character of recursive learning in the field of waste site construction does not vanish over time, but becomes more and more visible and structured.

(3) Experimentation is performed by networks of collective actors. Ordinary waste disposal sites are their "laboratories"; these become more and more sophisticated. Waste management practice comprises technological design, monitoring, documentation, interpretation, and feedback of results into the planning process.

(4) The more scientific and technological knowledge becomes specific, the more complex the process of recursive learning becomes. Technological options increase, acceptance problems become more severe, and the web of legislative regulation becomes denser. The experimental design tends to shift from purely instrumental to socio-technical experimentation.

4.1 From the "self-healing" powers of nature to disasters

Early industrial society was not much concerned about the problem of waste disposal. Waste was simply dumped in open landfills. The post-war trend toward consumerism and the ever-increasing proportion of chemicals in household waste made it clear that waste is something (perhaps the only thing) that cannot be thrown away (Packard, 1961; Galbraith, 1970; Commoner 1972). As Barry Commoner (1972) insisted, you can always iterate the question: "Yes, but where does it go to?" In fact, this question became more and more difficult to answer as knowledge increased.

Until 1970 in Germany about 50,000 uncontrolled landfills were scattered all over the country. The first call for technological assistance was induced not by ecological concerns but by the intention to increase the efficiency of the waste disposal sites (Herbold and Wienken, 1993: 95 f.). The new generation of installations were called "centralized, well-organized and compressed landfills", in which waste was orderly sorted by different categories and mechanically compressed.

This technology was based upon the underlying theoretical conviction, already formulated by the renowned Max Pettenkoffer with respect to public health problems in the 19th century, that nature possesses the "power of self-purification". This theory contended, according to one of its early critics, that "rapid microbiological reduction processes at waste disposal sites and in the surrounding soil formations would eliminate organic compounds. Consequently, the negative effects of landfills on groundwater and surface waters are normally very limited, and have not led to any bad experiences worth mentioning so far" (Schrammeck, 1973: 214). The author goes on to complain that this theory was responsible for the widespread installation of "centralized, well-organized landfills" without any basis insulation.

The Popperian model that risky projects may first be launched and renounced, subsequently, if they become refuted, so to speak, by the problems they cause, describes fairly accurately the established practice of how new technology is implemented in our modern societies. The myth of the inherent rationality of that model was, in our case, deconstructed as a result of experiences gained with waste disposal sites constructed on its basis. Investigations by a Bavarian state geological institute "left no room for doubt about the heavy pollution of groundwater from the waste site, so that, as the next step, the spread of the polluted groundwater . . . has to be investigated" (Exler, 1972: 103). The report ends with a modest recommendation: ". . . it would be useful, to investigate the hydrological conditions before installing a new waste site, in order to estimate in advance what happens if, despite all precautionary measures, soluble substances get into the groundwater"(112). The term "estimate" makes it fairly clear that the operating waste disposal site would be the location for getting the real figures. It is an early indication of a conceptual shift away from landfills as the final chain link in a chain of consumption to landfills as the first link in the chain of knowledge production.

4.2 Real-life experiments and controversial theories of insulation.

Groundwater pollution was already a well-known phenomenon with numerous locally disastrous occurrences. A comprehensive article in the leading journal, *Müll und Abfall*, (waste and refuse) was aptly titled, "Contradictions in the research on waste disposal". The author stated clearly that "All authors who have worked in the area of investigating the behavior of seepage water declare, in accordance with the practitioners, that landfills spoil groundwater considerably" (Cube, 1975, p. 215). In considering what should be done, the author recommends a cautious approach: ". . . the actual state of knowledge in waste research mirrors the most contradictory views. There are many open questions, and a rational and responsible expert is forced to act as carefully as possible as long as research has not been completed." But again, the hoped for completion of research was tied to experiences to be gained from new installations built on the basis of the state of the art. One suggestion was, of course, to insulate or tighten the body of the landfill against the subsoil, but this, too, generated further open questions and contradictory views: How tight is tight enough with respect to different layers of earth? In terms of mechanical compacting techniques and synthetic materials under stress? With respect to the long-term behavior of stored waste? A leading researcher of the time commented: "The *knowledge* about the many and various conditions of stress to which these sealings are subjected is still rather vague. About the changes caused by these stresses over years, one knows, properly speaking, nothing" (Schenkel, 1975: 12 f.).

Science-based ignorance spread rapidly. Associated with the unsolved problems of insulating waste disposal sites, other questions cropped up. What would happen, for instance, to freshwater entering a well-insulated landfill? Again, two competing theories attempted to address this issue. According to one, "only after several years of practice will unexpected difficulties arise, especially because of the activity of highly concentrated seepage water which then requires expensive processing" (Hantge 1975: 2). The alternative theory predicted that contaminated water, if continuously recycled into the landfill, would become chemically inert, and that the amount of contaminated water will be reduced by evaporation. Different landfills gave different information; generalized knowledge was difficult to obtain because the installations were too different.

Equally important was the study of uncontrolled chemical reactions within the body of waste. Here again, the first modern centralized landfills were considered to be safe because of the mechanical compression and proper sorting of waste. A commentary from 1965 stated: "If these guiding rules are observed, no potential dangers whatsoever are to be expected and the various concerns about certain landfill sites can be forgotten" (Klotter, 1965: 366). Scientists later regretfully admitted that they did not know much about these uncontrolled biochemical reactors called landfills. Two of them summarized the state of empirical knowledge: "The extent and the dependencies of the biological transformation of substances in waste sites is still largely unknown. The control of these processes under practical conditions, by means of a proper steering system is possible only to a rather limited degree. . . . Suggestions to improve the milieu for biological reduction processes in the body of a waste site under practical conditions should be examined with respect to efficiency" (Stegmann and Ehrig 1990: 49 f.). In other words, practical experimentation is the way out of the newly achieved state of scientific ignorance.

This evidence should suffice to support points *1* and *2* of what we wanted to show. The call for more techno-scientific expertise increased as more negative experiences accumulated. However the experts were not able to do much more than carefully observe what was happening and give incidental advice as to what to try next.

4.3 Innovation networks: organizing the links between theory and practice

The next phase of waste management development (roughly 1980-1990) was characterized by the construction of experimental waste disposal sites and the design of appropriate organization. In 1980 a leading figure from the *Umweltbundesamt* (the German Federal Office of Environment) referred explicitly to the alliance between real-life experimentation and science: "It has become clear now that completely new requirements with respect to life span and performance control must be developed. . . . More than anything else, a consistent and systematic collection of data concerning the long-term behavior of waste is missing. It has become clear that research and development projects (i.e. small-scale experimentation) yield only preliminary information. Relevant evidence must be produced by the operators and proprietors of waste disposal sites. But, information gathered on site, as important as it is for each single case, would be wasted, if it is not analyzed and compared scientifically. In order to achieve this, we are trying to initiate long-term research at waste sites." (Schenkel, 1980: 343).

What is intended here is the creation of a closed institutional cycle for recursive learning between science-based, real-life construction and practice-based gathering of information. This means the formation of a cooperative network for innovation consisting of operators, practitioners, experts, scientists, and administrators. This was confirmed by another leading figure from the *Umweltbundesamt*: "Cooperation between researchers, and operators and personnel at waste disposal sites is the basis for successful research" (Stief, 1977).

It was clearly impossible to organize the innovation process of waste management solely in terms of the institutional rationality of science. Law, politics, and public concern all pushed the process into different directions and left room only for temporary, regionally based cooperation. Nevertheless, the *Umweltbundesamt* advanced to an important organizational platform where information came together and was redistributed, where regulatory standards were set and administrative help was available. This supports the third point we wanted to make above. The significance of science in innovation is not the contribution of approved knowledge, but rather the import of experimental strategies in the design process, the operation, and the monitoring of a new technology.

4.4 Experimenting with socio-technical systems

During the last 10 years, a further element was added to the process of recursive learning, which not only changed dramatically the social setting of the actor networks, but also the character of experimentation in Germany: the shift from technical to social experimentation. The scenery has changed for several reasons. Environmentalists' protests had become stronger and better organized. New laws, especially the *Abfallgesetz* (1986), required German districts to develop comprehensive, future-oriented waste management plans. And, many communities started to set up new systems of waste collection, paving new ways for recycling. The collection systems were experimental in Donald Campbell's sense of "reforms as social experiments" (1969). The parameters of these experiments are related to assumptions about the behavior of the people involved. And clearly, behavior varies considerably depending on housing conditions, education, local traditions, information, convenience, or prices. Some of the questions, to be addressed in designing such systems are: What kinds of substances will be collected in various containers—organic refuse, paper, glass, metals? Should there be central collection sites or should every household be equipped with a variety of special containers? Should the costs of waste be fixed—per person or household—or should they depend on the weight or the volume of the waste? With the establishment of these waste management plans, the risk shifts from hardware technology to the organization and control of human behavior. Different systems for waste disposal can be compared and assessed in terms of rates of success. This information can, in turn, be utilized to modify the design of new socio-technical systems of waste management.

Experimentation with waste has taken a new turn. It has now become clear, that the amount and composition of household waste stored at the disposal site can be controlled to a considerable degree—a fact at variance with the traditional view that household waste is a non-homogeneous and therefore biochemically highly active mixture. Control of waste gives policy makers new organizational and technical options, but, by the same token, it also implies new uncertainties, especially with respect to the willingness of people and industry to cooperate. In many cases new forms of public cooperation (mediation, round table discussions, consensus conferences) are employed—not always at the pleasure of politicians, administrators, and experts. This network of actors is searching for new techniques as alternatives to the much contested high temperature incineration. "Cold" processes such as "biomechanical waste treatment" or "dry stabilization" enjoy a high degree of public acceptance and support by environmental associations, but they are still in an experimental stage of development and partially at odds with present legal conditions (Vorwerk and Kämper, 1977).

Integrated schemes of waste management which rely on the cooperation of the people, with respect to controlling the amount and composition of waste, can only be successful when the schemes are supported not only in words but in deeds. Whether this is the case can only be determined through social experimentation. The question is whether one must implement the scheme in order to learn whether confidence in people's cooperation was justified. The sociological observation and evaluation of the development and execution of such integrated plans of waste management is therefore also becoming a new field for the social scientists, not only as hindsight observers, but also as participants in the design process. This integration is perhaps an example for a setting which Arie Rip (1995) and others have termed constructive technology assessment.

We conclude this case by briefly reiterating the points we have attempted to substantiate. Experimentation is not only a sign of an immature technology, the functions and uses of which are largely unknown; it is an essential element of all steps of technological innovation. Recursive learning tends to develop from a more or less evolutionary process to an *institutional* strategy which includes, as major components, the explicit design of procedures, instrumentation of observation, documentation, interpretation, and feedback of results. In the course of this institutionalization, the number of agents involved in legitimizing, designing and executing experimental practices increases. Recursive learning is also fed by the increasing discovery of technological options, risks and uncertainties, as well as by contingencies of the behavioral and organizational factors involved. Obviously, the special rationality of scientific discovery and explanation has become an essential feature of a more complex logic of innovation and modernization, or perhaps even reflexive modernization (Beck, Giddens and Lash, 1996)—for which waste management is a very suitable case. Recursive learning and its institutionalized practice of socio-technical experimentation is only possible under the condition that actors are involved who speak very clearly the languages of different rationalities and can together set up a field of experimental practice that is not a special field of either science, or politics, or economics, but rather an integrated field of societal innovation, impregnated with scientific, political and economic uncertainties and risks.

5. The commercialization and politicization of molecular biology—the case of transgenic herbicide-resistant crops

When biologists discovered the methods of cloning in the late 1960s, the door flew open for a new age of constructive or synthetic biology. The molecular design of novel organisms through the transfer of genetic material across species barriers became a feasible option. This option triggered not only dramatic advances in our understanding of basic mechanisms of life, since it provided unique opportunities to investigate the function and regulation of genes, it also placed the academic discipline of molecular biology increasingly into the context of technological innovation. In this context, vast hopes were vested in molecular biology for medical, agronomic or environmental advantages to be achieved through genetically modified organisms and, at the same time, unknown risks were feared which such organisms might imply for health and environment (see Watson and Tooze, 1981). Transgenic herbicide-resistant crops are among the first projects which apply genetic engineering to plant breeding. The objective is to make non-selective herbicides available for weed control in agriculture. Such herbicides would normally be inapplicable because they kill not only the weeds but also the cultivated crop plants. This obstacle is removed when a gene which codes for resistance (or tolerance) to the herbicide is engineered into the plant genome (Duke, 1996; van den Daele, Pühler, and Sukopp, (1997). We will use the case to illustrate the following points:

(1) The prospect of transgenic herbicide-resistant crops has emerged as a "push" from scientific experiments in plant genetics. It feeds back to the science by confronting geneticists with economic, regulatory and policy issues.

(2) The process of innovation transfers transgenic herbicide-resistant plants from laboratory conditions to the environments of agriculture and food production. Such transfer is bound to reveal and multiply uncertainties (non-knowledge) with respect to the performance and the safety of these plants.

(3) The scientific response to non-knowledge is research. The uncertainties inherent in the innovation process are likely to be "recycled" to science as research questions. This could be extended to the final step of placing transgenic herbicide-resistant crops on the market, if monitoring systems are installed which make this step a kind of controlled experiment ("experimental implementation").

5.1 Goal-orientation in plant genetics

Herbicide resistance was the most frequent project when molecular biologists started to construct transgenic plants—not because the agronomic problem was so urgent, but more because the gene (isolated from bacteria) happened to be available and because successful transformation was easily detectable (transformed plants survive spraying with the herbicide). In the beginning, transgenic herbicide resistance was more a research tool of plant genetics than a project in crop breeding—it was, for instance, used to study the expression and stability of transgenes in plants. Thus it was perhaps true that for a while "the science and the technology were one and the same", as Liebe Cavalieri (1978: 1153) remarked for the early days of genetic engineering. However, while this may be a fair account of the pioneering work that first established the feasibility of transgenic plants, it is no longer applicable to the series of projects that later engineered herbicide resistance to ever more species grown in agriculture, like tomato, cotton, soybean, or oilseed rape. Here the technological orientation of plant breeding took the lead: research problems focused on how useful genes can be isolated from donor organisms, and be transferred and made functional in useful target (host) organisms.

According to the model of finalization, the shift to economic and social goals does not mean that the technology (constructing transgenic plants) is divorced from the scientific discipline (molecular plant biology), but rather that the frontiers of research in the discipline undergo transformation. Orientation toward "external" (technological) goals is a normal event in disciplinary development once the knowledge of basic mechanisms has become consolidated. Or, to use Victor Weisskopf's (1967) terms, the dynamics of the discipline will shift from intensive to extensive growth.

5.2 Science to innovation—features of contextualization

Goal-orientation accommodates technical and social objectives to the problem definitions and research strategies in science. At the same time, it places science under the perspectives of commercialization and politicization. This will, for instance, mean that scientists must justify their research goals in terms of the usefulness of the technical objectives, that they become vulnerable to criticism raised in the general public against these objectives, and that they have their discoveries routinely screened for patent applications before publication. Such changes have occurred previously in other disciplines like chemistry and most of the sub-fields of physics; they are beginning to reach plant genetics now. The more the breeding perspective prevails in projects on transgenic herbicide-resistant plants, the more scientists will have to face the "real worlds" of economics, agriculture and politics. This includes confrontation with public concern, which is particularly intense in Germany. Transgenic herbicide-resistant crops imply a mass release of genetically modified organisms into the environment. Such releases had been forbidden, until a few years ago, by the guidelines regulating genetic engineering. Although these guidelines anticipated that the restrictions could be lifted if knowledge accumulates that genetic engineering does not in fact involve specific risks, they nevertheless mean that the road to innovation with transgenic plants inevitably leads through the political battlefields of deregulation.

Doing science becomes more complicated. Plant geneticists have to share roles and competencies with other experts in the innovation network and with the general public which claims the right to judge whether that science is in the general interest and should be allowed. Accordingly, we find plant geneticists acting in increasingly differentiated social contexts. In the German case we find them not only as researchers in the laboratory, but also as experts advising government or business about the feasibility of innovations, as members of regulatory committees recommending safety measures for the release of transgenic plants, as organizers of technology assessments that address broader issues of the risks and benefits of such plants, and as participants of numerous public debates where they try to defend the acceptability of their projects.

When science is pushed down the road to innovation, the limits of experimental findings and theoretical models become obvious. Unresolved questions and uncertainties accumulate regarding the effectiveness, the safety and the relevance of transgenic herbicide-resistant crop plants. Will such plants function as designed when they are grown on large fields? Is there a need for herbicide-resistant crops? Can they do harm to the environment? Does genetic modification have any effects on the nutritional value of food products? Will the consumers buy products from transgenic crops? Such questions may not concern geneticists as long as they consider constructing herbicide-resistant plants as scientific experiments to be published in professional journals. These issues cannot be ignored, however, when such plants are expected to yield a new weed control technique in agriculture. In the context of innovation these questions define relevant uncertainties, non-knowledge in our terms, which may ruin the whole project. To reduce these uncertainties the open questions are in most cases "recycled" to scientific research and expertise—not necessarily to molecular biology, but to conventional plant breeding, ecology, nutrition science, medicine, or market research.

The experimental plant that the geneticist constructs in the laboratory is a far cry from a crop variety that the farmer can grow in the field. To establish the effectiveness of transgenic plants, the criteria of and competencies in conventional plant breeding are indispensable. The herbicide resistance gene must be transferred to cultivars that have all the agronomic properties plant breeding has crossed into modern crop varieties (high yields, pest resistance, processing, storage and food quality, etc.); it must be tested whether the herbicide resistance gene interferes adversely with these properties. Furthermore the performance of transgenic plants under field conditions (when they are grown in large plots with different soils and climates) must be investigated, since this cannot be predicted from greenhouse experiments. Safety considerations provoke new questions as well. Public fears about possible health risks, legal obligations of product liability and regulations for premarket reviews of transgenic food products create a push for extensive testing of whether the genetic modification could turn a plant into a toxicant or allergen. Thus, tests in a recent case revealed that the transformation of soybeans with a gene from paranut, which enhances the nutritional value of the soybeans, can also confer the (known) allergenic potential of the nut to the soybeans (Nordlee *et al.*, 1996). The result stopped the project; it could also have renewed the search for a suitable transgene from donor organisms with are not known allergens. To address environmental concerns that transgenic plants (or their genes) might "escape" to other organisms or natural habitats, experimental programs were designed to investigate the relative fitness of transgenic plants in realistic (agricultural) environments and the rate of outcrossing (to related wild plants) or horizontal gene transfer (to soil bacteria) from the transgenic crops (Kjellson *et al.*, 1977).

These tests and investigations will inject more relevant knowledge into the process of innovation. However, they will not relieve the process from the burden of non-knowledge. In fact, the opposite may be the case. More scientific analysis of the process will better reveal what we do not know, when we manipulate complex systems such as living organisms and ecosystems to which these organisms are released. It has been made clear that the effects of transgenes on the host plant can only be predicted to a limited degree. Transgenes confront a metabolic context which is different from that in the donor organism. They may therefore have unintended consequences for the properties and environmental behavior of the host plant, which cannot be derived from the knowledge of the genetic information and its function (in the donor organism). The capacities to test for such effects are also limited. Since many mechanisms of plant metabolism are still unexplored, one may not even know where to look. It is possible to test for toxicological effects of the gene product in experimental animals, but one cannot say for sure whether these tests are a true indicator of what the effects on humans could be. Dangerous substances which might be formed through unintended interactions of the gene product with the host plant metabolism could only be detected by feeding the whole plants to test animals, which is rarely ever done. Effects of the transgenic plants on the environment are also difficult to anticipate. Real ecosystems cannot be simulated adequately by model systems in the laboratory. The field experiments carried out in the safety research programs may test whether unintended increases of fitness (competitiveness) occur due to the genetic modification, but the data refer to the specific conditions of the test fields and cannot be extrapolated with confidence to other conditions or ecosystems to which the plants may be released. Moreover, as evidence from the history of the introduction of new plants suggests, deleterious and irreversible effects of transgenic crops on the environment may only become visible several decades after they have been introduced.

The need to know more about these questions literally opens up "endless frontiers" in science. That is exactly why exposing uncertainty and non-knowledge is an attractive strategy to the critics of genetic engineering: it promises to delay the innovation endlessly.

5.3 Innovation and its critics—non-knowledge and the mandate of science

Critics of genetic engineering argue that safety research and preventive testing have not reduced the uncertainties about possible risks sufficiently to warrant deregulation; therefore large-scale releases and the placing on the market of transgenic plants should not be allowed. Whether this conclusion is justified is a matter of political judgment and shall be left aside. The point we want to make here is that arguments of non-knowledge are likely to propel the recourse to more research rather than relegate science as irrelevant from the discourse over deregulation. The reason seems to be that arguments of non-knowledge become self-refuting if they are carried to the extreme.

One extreme would be to succumb to the relativist rhetoric and the epistemological ambiguities that characterize many of the variants of social constructivism and conclude that, strictly speaking, there is no such thing as knowledge or truth, but only opinion, interest, social convention or political choice. In this case it would be pointless to criticize the uncertainties of the innovation. The subtleties of the reflection on the status of empirical knowledge, be they from philosophical epistemology or from the sociology of knowledge, apply to all that we call knowledge. Since they cannot possibly imply that it is meaningless to distinguish between what we know and what we do not know, they are of no use in discussions where this distinction is at issue. In fact, meta-theories of knowledge play no significant role in the political debates over the risks of genetic engineering (in contrast to sociological accounts of and contributions to such debates). Participants in these debates may have incompatible criteria of evaluation: What is acceptable risk? What constitutes relevant harm? What are desirable goals for social development? But they share the reference to science as a source and criterion of empirical knowledge, while they may, at the same time, challenge the competence and integrity (objectivity) of the persons who speak in the name of science (van den Daele, 1996).

The other extreme would be to require knowledge that cannot be achieved as a matter of principle. There is no way of ever knowing what the effects of transgenic plants on the natural evolution of species could be in the long run of say several hundred years from now and how that might influence the ecosystems which will then exist. It is likewise impossible to ensure that no hidden risks are involved in transgenic herbicide-resistant plants which escape our best efforts of testing and theorizing. Such uncertainties are tantamount to the fact that the future is not known. They hardly justify the argument "we do not know enough". If they were to count, we would never know enough to do anything, nor, for that matter, to decide not to do it.

Between these extremes there is much room for more research that might reduce uncertainties about the safety of the innovation. Better methods can be developed to target the integration of transgenes to specific sites of the host genome, to preclude unexpected effects from insertional mutations (positional effects). Field tests can be extended to study the behavior of transgenic crops for a longer period under a greater variety of environmental conditions. Geneticists may try to construct plants in such a way that they (or their transgenes) will not propagate in the environment, for instance, by including regulatory sequences in the gene construct which will not function in bacteria or by inducing sterility of the plants. Mechanisms of allergenicity can be further explored to assess whether new proteins introduced to the food chain through transgenes could be allergens.[5]

Arguments of non-knowledge have the strongest political appeal—to the general public, the legislators and possibly the courts—when they do not claim ultimate limits of knowledge, but rather neglect of investigation. To the extent that they focus on such neglect, they give further support to translation of the uncertainties of innovation into research programs in science.

5.4 Monitoring the risks of innovation

Transgenic crops were deregulated in most countries, although the uncertainties that could arguably be addressed by more research were not resolved. It was pointed out that regulatory routines in

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[5] While testing for allergenicity poses no problems if the donor organism is a known allergen (as in the case of paranut), the issue cannot at present be addressed for transgenes from organisms (bacteria or plants) which do not have a history of food use (FDA, 1992). Since fears of unknown allergens wage high in the public debate over transgenic food crops, the companies which develop such crops collect comprehensive additional information on the transgenes they use to exclude that risk. Monsanto, for instance, analyzed the transgene protein which confers herbicide-resistance to soybeans by showing that it has no significant amino acid homology to known allergens, that it is rapidly degraded in a digestion model, that it is present in the crop in low levels relative to common allergens, and that it is closely related—both functionally and structurally—to the corresponding soybean protein. "Based on this information, it was concluded that this protein posed no significant allergenic concern" (Fuchs, 1995: 212).

conventional plant breeding had always tolerated some uncertainties about the risks of introducing new crops,[6] and that no evidence had emerged during many years of experimentation to suggest that transgenic crops implied higher risks (and therefore needed stricter regulation) than conventional crops. In this respect it was also emphasized that the step-by-step approach, which requires that transgenic plants be successively tested in the laboratory, the greenhouse and in controlled field experiments before they can be grown in agriculture or placed on the market, provides considerable safeguards against "bad surprises", since it allows for learning by doing during the release of the plants.

The latter argument met with criticism not only from the groups which oppose all genetic engineering as a matter of principle. The critics argued that releases of transgenic plants are in fact not designed to ensure learning by doing and that the interpretation of the step-by-step approach as a strategy of implicit safety testing is misleading. Nor had the numerous small-scale field trials with transgenic plants been properly screened for harmful effects on the environment; evaluations were mostly confined to the envisaged agronomic performance of the plants (Wrubel *et al.*, 1992). Nor could the limited trials in test plots, often under conditions of reproductive containment (sterile plants and/or location of test plots at significant distances to related wild plants), yield conclusive evidence that large-scale commercial releases are harmless. The ecologist Regal summerizes this criticism "Yet this sort of nondata on nonreleases has been cited in policy circles as though 500 true releases have now informed scientists that there are no legitimate scientific concerns" (1994: 11). These arguments reopen the scientific frontier with the introduction of transgenic plants, and lead right down to the need for what we call "experimental implementation".

It is surely a significant fact that no unexpected environmental effects have been observed, although in the meantime several thousand releases of transgenic plants have occurred all over the world. But it is also undeniable that commercial releases will always be a step beyond existing knowledge—even if greenhouse experiments and test releases were more explicitly designed and evaluated as safety research. In the final analysis, innovation means the invasion of unknown territory. The step beyond is based on a political assessment that, existing uncertainties notwithstanding, transgenic plants are "safe enough" for us to go ahead and seek the benefit these plants may offer. Such assessment is by definition preliminary, and it seems a logical consequence that plants which are released "on bail", so to speak, should be monitored properly to control whether the release is justified.

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[6] Including the risk that some new unknown allergen might be induced. For instance, the allergenic potential of apples became only clinically visible after new conventionally-bred varieties had been introduced in the 1960s (Frank-Oberaspach and Keller 1996: 51).

Some postmarket monitoring is in the best interest of the companies which place transgenic products on the market, not only because they bear strict liability for any damage that might result from the products, but also because they must ensure that the products are effective. There is no way to anticipate all the possible circumstances and environments in which the products are supposed to function. To some extent, learning by doing is indeed the only way to know whether they perform as planned.[7] However, regulations rarely enforce systematic postmarket monitoring. The British deliberate release regulation, for example, only requires the applicant "to keep himself informed of any damage to the environment caused by the release or marketing [and] notify if there is any information which would indicate a change in the level of risk."

The debate over how far postmarket monitoring should go is likely to continue, because it arises within the present scheme of deregulation, not as an alternative to that scheme. The more carefully the monitoring is designed, the more the implementation of new technology will fit the scientific model of a controlled experiment. This will add to the costs of innovation, but also to the rationality of it. A truly experimental implementation may not only be necessary to provide information of whether and how a new technology performs under realistic conditions, it may also be necessary to convince a reluctant public that irresolvable uncertainties with respect to the safety of innovations can legitimately be imposed on the society.

6. Concluding remarks

To understand how science can be both distinct from and related to politics and economy was the key issue addressed in the model of "finalization". While the model may have failed, being epistemologically biased and sociologically incomplete, the issue is still with us. And it will be all the more important the more science becomes an agent of social change in processes of modernization. The sociology of scientific knowledge and the laboratory studies have drawn our attention to the fact that doing science is a social process which is entrenched in social processes and that the intellectual practices of the scientists are interspersed with negotiation, opportunism, micro-politics and rhetoric. However, these studies tend to use the term "social" as a residual category rather than as a restricted analytical concept, and invite new reductionist fallacies. The exposition of the social nature of scientific knowledge does not allow the conclusion that no differences exist with respect to the values, criteria and institutions of rationality between scientific and non-scientific activities—even if it is true that the differences are subject to historical change,

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[7] And failures do occur. Thus, a Monsanto variety of transgenic pest-resistant cotton, which had provided protection in field trials, proved ineffective in real agricultural practice; this was only detected after the variety had been registered and placed on the market. The implementation of transgenic herbicide-resistant crops should be monitored to ensure early detection of herbicide-resistant weed selection, which threatens to destroy the market for the product, since these weeds would render herbicide-resistant crops useless.

contextual interpretation, and situational opportunism. We assume that modern systems theory provides the conceptual tools for a more adequate reconstruction of the social character of science. Modern societies ”construct” the distinctions between scientific, political, legal and economic accounts through institutions of functional systems.

Networks of innovation integrate heterogeneous functional operations in organizational action. Such integration may seem to produce a seamless web in which all differences go under. But it produces a coordination of the differences in decision making. The power of the network rests in the fact that it can use different rationalities without fusing (or confusing) them. The heterogeneous rules, criteria, interests, goals or orientations that abound in the innovation process will influence the course of science, but leave its functional role intact. It is the competence of science to determine what is known and what is not known. As our cases have shown, the application of science within the context of innovation science produces both more certainty and more uncertainty. The scientific response to uncertainty is more research. The result is that the increasing integration of science in the social field of innovation means that social actions in this field become increasingly adapted to the modes of scientific operation. The design of the implementation of new technology as a controlled experiment is the endpoint of such development.

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