Symposium: "Revisiting the theory of 'Finalization in Science' "

Symposium: "La 'Finalisation en Science': une re-lecture du concept"

Peter Weingart

From "Finalization" to "Mode 2": old wine in new bottles?

Abstract. *Discussions about "new forms of knowledge-production" refer to purportedly fundamental changes in the organization of science. A closer look reveals that these changes pertain to a particular sector of science, i.e. policy-related fields. It is suggested that a better understanding of the kind and scope of changes would be achieved by viewing them as resulting from a "scientification" of society and a correlate "politicization" of science, both of which processes signify the emergence of the knowledge society. Ironically, the "finalization thesis", which foresaw much of this two decades ago, met with opposition, while the new claims were embraced. This is explained by the context of legitimation*

Key words. *Finalization – Knowledge society – Mode 2 – Post-normal science – Scientification*

I. Observations and claims

For some time and in surprising agreement, different authors have observed the emergence of "new forms of knowledge-production" and given them flashy names: "post-normal science" (Funtowicz and Ravetz, 1993; Elzinga, 1995), "Mode 2" (Gibbons et al., 1994), "postacademic science" (Ziman, 1995). In none of these tracts,

---

This article is based on a contribution to the joint meeting of the European Association for the Study of Science and Technology and the Society for the Social Studies of Science (Bielefeld, Germany, 10–13 October 1996). The context of the symposium, "Revisiting the Theory of 'Finalization in Science' ", is described by William Leeming in his introductory article, "Revisiting Finalization", which was printed in our last issue together with papers by Gernot Böhme and Aant Elzinga.

Social Science Information ©1997 SAGE Publications (London, Thousand Oaks, CA and New Delhi), 36(4), pp. 591–613.

though, does one find a reference to a scheme which is obviously a precursor: "finalized science" (Böhme et al., 1973; Schäffer, 1983). It seems as if vague metaphors or even simply acronyms are particularly suited to propel the imagination. "Mode 2", as a catchword, has attracted the attention of higher circles in science policy and may well become a bestseller of science studies (badly needed in times of "science wars"). The mode of writing about and, even more, of perceiving the transitions of science as novel without any apparent memory of very similar observations only two decades ago is, perhaps, itself an indicator of the phenomena described by such terms as "post-normal" and "Mode 2": the most conspicuous characteristic of the new mode of science is its susceptibility to fads and fashions. With the information explosion, and the exponential growth and "medialization" of science, attention becomes an ever rarer commodity. "Hit-and-run" analyses are an ever greater temptation, especially where society and politics are involved.

Two decades ago the "finalization thesis" was bitterly opposed by scientists and policy-makers alike because of the real or imagined normative implications for science policy and the public legitimation of science (cf. Schäffer [1983] on the finalization debate). Today notions of "post-normal science" and "Mode 2" are embraced precisely because of their seemingly new implications for the legitimation of science. I will try to show that these are as much notions motivated by ideas of a politically "more correct" science as they are descriptions of actual changes, and that many of the claims to novelty in science are, in fact, revisions of previous exaggerations now being replaced by opposite excesses. The questions posed very much in the spirit of "Mode 1" and "normal science" are: What is being claimed? What is the empirical substance of the claims? What remains? My thesis is that the descriptions of the new forms of knowledge-production pertain to a specific section of the research system but cannot be generalized to science as a whole. In particular, there is no fundamental change in epistemology in sight. I will claim that the authors of "Mode 2" and similar schemes are looking at phenomena on the surface and, for lack of theoretical depth, dramatize them.

Most ironic is the similarity of these notions to some aspects of the "finalization thesis", not only because recent authors write as though reporting hot news of revolutionary changes, but also because there has been a complete turnaround in the way these ideas are received. What explains the bitter opposition then in contrast to the acceptance now?

I will point to some parallels between the new descriptions of science and the "finalization thesis", and try to explain the change of mind among scientists and policy-makers. The observations of the new forms of knowledge-production concur on a number of points:

- The university has lost its monopoly on knowledge-production. Research centers, government agencies, industrial laboratories, think-tanks, consulting firms are also producing new knowledge. Through their interrelations in networks, contexts are created which replace the traditional disciplines. The organizational forms of research teams and networks are transitory.
- Knowledge-production is no longer the search for basic laws of nature but takes place in contexts of application, i.e. with a concrete utility and clients in mind (Gibbons et al., 1994: 4; Funtowicz and Ravetz, 1993: 121).
- Disciplines are no longer the crucial frames of orientation for research or for the definition of subject matter. Instead, research is characterized by transdisciplinarity: solutions to problems emerge in contexts of application; transdisciplinary knowledge has its own theoretical structures and research methods; the results are no longer communicated through the institutional channels, but through the participants in their search process (Gibbons et al., 1994: 5; Funtowicz and Ravetz, 1993: 109).
- Criteria and quality control of research are no longer determined by the disciplines and carried out by "peer review" alone; additional social, political, and economic criteria emerge in contexts of application. It is becoming increasingly difficult to determine what is good research (Gibbons et al., 1994: 8; Funtowicz and Ravetz, 1993:90f.).
- Knowledge-production becomes socially accountable and reflexive. Research comes under new pressures of legitimation. It is increasingly oriented to social values and political objectives as well as to the media (Gibbons et al., 1994: 7f.; Funtowicz and Ravetz, 1993: 117, 121).

The claims concerning the scope of the changes described by the terms "Mode 2' and "post-normal science" are not identical, and some give rise to controversy among the authors. With respect to the persistence of "normal" or "Mode 1" science, the authors remain vague. The farthest-reaching prognosis is that "Mode 1" will disappear into "Mode 2" (Gibbons et al., 1994: 154). For "post-normal science", it is claimed more strongly than for "Mode 2" that it

is a "qualitative transformation of science", and even more that it is a revolution like that of the 17th century, which will influence the definition of subject matters, methods, and social functions (Funtowicz and Ravetz, 1993: 112, 117). Helga Nowotny, one of the authors of the "new production", is a proponent of the audacious but unsubstantiated thesis that every institutional change of science will also entail epistemological changes. Thus, "Mode 2" also stands for an epistemological revolution whose more detailed description is unfortunately not provided.

These are strong claims, and that accounts for their attraction. If they could be substantiated empirically, it would mean that Western science has changed dramatically before our eyes without anyone noticing for quite some time. But it is perhaps no accident that the authors remain vague as to the actual diffusion of the observed characteristics of "Mode 2" and "post-normal science". No one will doubt that changes take place in science. Exactly what changes, and what this means with respect to the core features of science, remains unclear.

II. "Mode 2" and "post-normal science": the dawning of a second revolution?

1. Heterogeneity and organizational diversity

The claim to heterogeneity and organizational diversity of research is the easiest to verify. As mentioned above, the observation is that extra-university institutes, research centers, government agencies, industrial laboratories, think-tanks, and consulting firms are increasingly important locations of knowledge-production. Immediately the question arises of which national science system the authors have as their (implicit?) referent. Historically, the universities were relatively late in becoming the most important institutions of research. Neither in Germany nor in France, and not even in the USA have they ever been the only ones. Government research in Germany dates back to the mid-19th century and has been expanding ever since, in parallel with the expansion of the state's welfare functions (Lundgreen et al., 1986). In France, the tradition of research on behalf of the state dates back even further, to the time of the Revolution.

Industrial research accounts, in most of the European industrial-

ized nations, for more than half, in Germany for two-thirds, of the whole S&T system. Here again the tradition goes back to the 19th century. At that time, in the chemical and electrical engineering industries, the foundations were laid for modern industrial research, which became the model. Meanwhile, a development running seemingly counter to "Mode 2" is appearing: industry and universities are forming "strategic alliances". In the USA, universities act as "venture capitalists". In Europe a similar development may come about. The crucial point, though, is that industry can achieve greater flexibility by buying into university research, instead of setting up its own research laboratories (Etzkowitz, 1990). Some of the research-intensive corporations have disbanded their once-renowned research labs and, instead, set up laboratories specialized in specific product lines. This development is taking place in the framework of globalization of research: expensive research is taken to "cheaper" countries whenever that is possible. The target of this strategy is "cutting-edge" research of "Mode 1" type. Organizational forms differ from industry to industry, as does the type of knowledge. Thus, a careful institutional comparison is necessary to distinguish fact from fad.

The interesting question arising in this connection is: what are the long-term effects of this infiltration of the universities by industry? Will the values and operating rules of academic research ("Mode 1") be undermined, as feared by the presidents of Harvard and Stanford when the first industrially financed genetics laboratories were established at their universities? The development observed since then reveals, on the one hand, a surprising resilience of traditional academic forms of knowledge-production and, on the other, a growing competence on the part of the universities in marketing that knowledge.

Think-tanks and consulting firms remain candidates for "new" locations of knowledge-production. In terms of manpower and budgets, their role is negligible compared to the universities and government research laboratories. More important, though, is that they remain largely dependent on academic research both in terms of knowledge flow and trained manpower. More realistically, they operate primarily in the transfer zone rather than in original research, the results of which they broker to the political administration.

As far as the German research system is concerned, extra-university research has been expanding for many years. This

expansion is due mainly to the growth of national research laboratories and other kinds of research institutes. By far the larger share of this research is bound by the standards of academic research via its publications and their "peer-review" process. Even the scientists in the quasi industrially organized national laboratories with strongly pragmatic missions remain oriented to their respective disciplinary communities. Apart from that, the institutional differentiation of research is limited almost exclusively to the natural sciences; the humanities are largely unaffected by this development.

Above all, the organizational diversity is not new and does not indicate a fundamental change of science. It is primarily an indication of the expanding role of knowledge in social, political and economic areas of activity. The question is how far the production of systematic, formal knowledge reaches into other institutional areas of society, and to what extent these are forms of knowledge-processing and transfer or genuine production of knowledge. The latter is at least claimed implicitly in the thesis of increasing "transdisciplinarity".

2. *Transdisciplinarity*

The claim that an increasingly important role is played by transdisciplinarity may be seen as the cognitive correlate of the organizational diversification of knowledge-production. The only definition of transdisciplinarity is that it is knowledge emerging from a particular "context of application with its own distinct theoretical structures, research methods and modes of practice but which may not be locatable on the prevailing disciplinary map" (Gibbons et al., 1994: 168). "Transdisciplinarity" is thus not identical with interdisciplinarity. Nevertheless, the thesis remains vague and ambiguous. On the one hand, the authors refer to an ongoing specialization into subfields and their recombination independently of traditional disciplines; on the other hand, they mean a type of knowledge which emerges in contexts of application, outside of disciplines and their criteria of evaluation (Gibbons et al., 1994: 5, 6).

The enormous specialization and recombination of specialties is a process which has been unfolding within the framework of "Mode 1", i.e. academic science, and within the traditional disciplines ever since the emergence of disciplines in the 19th century. The observation that disciplines are gradually losing their function

as social organization and cognitive frame of orientation has been made many times, and was analyzed systematically by Whitley 10 years before the discovery of "Mode 2". Determining the degree of specialization is a matter of definition of disciplines and their operationalization. The authors do not provide such a definition. The operationalization could be sections of scholarly societies, titles of scholarly journals or denominations of university chairs. In all these cases one can be sure to encounter a correlation between growth rates of numbers of fields or specialties and general growth of the system of science as a whole (cf. Weingart et al., 1991).

It is also an empirical question as to how far recombined research fields are removed from their original disciplinary contexts. If the patterns of recombination reveal, as claimed, that "knowledge-production moves increasingly away from traditional disciplinary activity into new social contexts", this is either given by definition or would have to be demonstrated empirically (Gibbons et al., 1994: 6). So far there is no comprehensive and systematic study of the changes of disciplinary boundaries, to my knowledge, and many impressionistic reports, notwithstanding all declarations about a fundamental structural change, are pure speculation (cf. Klein, 1996).

The few indirect examples of transdisciplinary research fields which the authors provide point to the other class of knowledge-production: technology assessment, environmental research, the so-called "integrated assessment studies". One could add climate research and research on global change (Gibbons et al., 1994: 137). Climate research, like most other research, takes place in regular research institutes, national laboratories, or universities. It may be organized in interdisciplinary research projects but their parts can be easily referred back to disciplines such as meteorology, physics, chemistry, or, on the social science side, to psychology, economics or sociology or whatever the case may be. This is also true for all research devoted, in a wider sense, to sustainable development under the conditions of global change, which has an explicit normative orientation. (The type of research focused on this problem is very close to what is called "strategic research" [Elzinga, 1995: 223].)

These examples show that, all too easily, surface phenomena are being taken for granted. One has to differentiate between the level of program funding and actual research. While the programs formulate interdisciplinary or transdisciplinary problems, much of the research funded under their headings takes place in traditional dis-

ciplinary form or as multidisciplinary research. In the best case, the different research results are referred to one another at the end. In the worst case, the transdisciplinary program titles, formulated for purposes of political legitimation with specific contexts of application in mind, camouflage normal disciplinary research (Weingart et al., 1990).

3. Contexts of application and hybrid fora

The examples of environmental and climate research indicate that the relevant aspect may not be transdisciplinarity but what the authors have in mind when focusing on "contexts of application" and "hybrid fora". With respect to these fields, the novelty may be seen in the coupling of science and politics. Governments, environmental NGOs, and supranational organizations such as Unesco, as well as supranational scientific associations, have formed international consortia and launched international research programs. An example of the former is the Intergovernmental Panel on Climate Change (IPCC), which regularly coordinates the state of knowledge of the climate research institutes and feeds it into international negotiations on the protection of the climate. Examples of the latter are the World Climate Research Program (WCRP), initiated by the International Council of Scientific Unions (ICSU), the World Meteorological Organization (WMO) and the Intergovernmental Oceanographic Commission (IOC). Others are the International Geosphere–Biosphere Program (IGBP) launched by the ICSU, and the Human Dimensions of Global Environmental Change Program (HDP) initiated by the International Social Science Council (cf. Elzinga, 1995: 224).

The most important function of these programs is the harmonization of data formats and standards of measurements and methodologies, a crucial precondition for the transnational communication and transfer of consensual knowledge into politics. The research programs are funded multinationally and are either coordinated with national research programs or have a direct impact on them. They imply political goals and interests which may be legitimating or economic. But the programs are normally initiated by scientists on the international level, e.g. by Unesco. Here, crucial negotiations take place between scientists and policy-makers in

which program objectives and research priorities are at stake which are of interest to both sides: research funds in exchange for relevant knowledge and expertise. In most cases the scientists have created the need for scientific advice in the first place, by postulating problems in need of solutions (Küppers et al., 1978). Sometimes it takes years before the research results appear on the political agenda and become issues for political debate.

The IPCC is one such transfer agency, which, on a transnational level, translates knowledge into political recommendations after having achieved an internal consensus. It is the type of organization in which international negotiations bring together scientists as well as representatives of national and international agencies, environmental NGOs and economic lobbying associations. The IPCC as well as the transnational research programs form a new type of interface between science and politics.

The novelty in this is not the type of knowledge-production, which still takes place in the respective research institutes and is subject to scientific standards carried out by "peer-review". The novelty may be seen in the integration of science into politics, the coupling of scientific knowledge-production and political decision-making. This is the core of what Funtowicz and Ravetz (1993) describe as "post-normal science". The central feature of "post-normal science", in their view, is the combination of uncertainty and ignorance on the part of knowledge-producers and conflicting values, high risks, and intense pressure to reach decisions on the part of policy-makers.

Funtowicz and Ravetz cite the design of a "repository for long-lived nuclear wastes secure for the next ten thousand years" as a "paradigmatic case" for "post-normal science". In such a case, system complexity, high risk, and pressure of decision-making come together. Another of their examples is the construction of a dam. Here they see the range of the continuum extending from "traditional" to "post-normal" science represented in problems such as the application of standard construction knowledge and the creation of local acceptance. The essential feature of post-normal science, as they see it, is that "uncertainty and ignorance, even in practice based on science, can no longer be expected to be conquered; instead they must be managed for the common good" (Funtowicz and Ravetz, 1993: 102).

It is not surprising that the frame of reference for this description of "post-normal science" is an already nostalgic aspect of "normal

science": research within small communities organized around a specialty, "alienated from their societal and natural environments" (Funtowicz and Ravetz, 1993: 117). The institutional context juxtaposed to it is one characterized by so-called "hybrid communities". Scientists move about in the diffuse border area between science and politics, between research and advising policy-makers. The interrelationships and overlaps between these areas can even be demonstrated in the respective discourses (Engels and Weingart, 1997).

All claims to the transdisciplinary character of the new production of knowledge and the importance of the context of application are based on evidence drawn from a very narrow sector of research. The complex of technology assessment, risk research, and environmental and climate research, for which uncertainty of knowledge, complexity of subject matter, policy orientation and value-ladenness are typical, represents only a fraction of the entire S&T system. The features observed can hardly be generalized, and there are no systematic reasons in sight which would make one believe that this mode should extend to all other areas of science. It is an area of science which derives its particular characteristics from being close to policy-making and in the limelight.

Wholly aside from the question of the relative weight of this area in the world of research, the decisive question is its independence from disciplinary, "Mode 1", science. The crucial criterion to determine, if, indeed, new transdisciplinary lines of research are institutionalized in this way, is their relationship to established disciplines. This is defined, on the one hand, by the stability of the contexts of application as locations of knowledge-production and, on the other hand, by the independence of their quality standards from those of the recognized disciplines. This is the litmus test of all theses about the emergence of new science, "Mode 2", "post-normal", "post-academic", "finalized" and whatever. Perhaps it is necessary to ask first if the authors claim the emergence of a new science or only the variation of the existing one.

As to the criterion of stability, proponents of "Mode 2" are vague: "Though problems may be transient and groups short lived, the organization and communication pattern persists as a matrix from which further groups and networks, dedicated to different problems, will be formed" (Gibbons et al., 1994: 6). They acknowledge, however, that the disciplinary forms of cognitive and social organization continue to be prerequisite for the creation of identi-

ties, as happens during education and training. They even admit that a transfer of research "outside universities and closer to real world problems" does not necessarily lead to a transdisciplinary mode of research (Gibbons et al., 1994: 139, 148).

While this sounds almost timid compared to the audacious claims about fundamental change, it is more realistic. The relative temporal stability of disciplines (and subdisciplines), beyond the immediate political (or other) occasions which constitute the contexts of application, is the social manifestation of the functional differentiation of scientific knowledge-production. Specialization and recombination of fields of knowledge-production do not affect this fact. Changes of disciplines, the emergence of new ones, are generated in the system of science. The emergence of economic and political contexts of application, too, which may change again due to different causes, have to be processed within and through the system of science. The new "fundamental problems" which arise from contexts of application by way of feedback processes can have a constitutive and sustained impact on knowledge-production only if they are subject to the differentiated communication on "truth". The stability of disciplines and their resistance to the supposedly different structure of "problems of the real world" is the inescapable fact of functional differentiation.

In order to prove the existence of "persisting matrices" as claimed by Gibbons et al., detailed empirical analyses are called for. To the extent that they exist, they demonstrate the contrary or at least the complexity of the matter. A comprehensive comparative study designed to explore the validity of the finalization thesis produced some interesting results which corroborated the theoretical point above (Van den Daele et al., 1979). One was that "in all phases of the transformation of social problems into scientific programs... the input of science had a direct initiating or limiting role". "Already the perception of social problems is often based on science" (p. 31). The subject matter of problem-oriented research (now "Mode 2") may be non-disciplinary. However, the generalization of the case studies led to the conclusion that "the ways in which problems are processed are more often determined by the disciplines into which they are incorporated than the other way around". And:

> Institutionally the stability of problem-oriented research depends on whether the respective "problem communities" and the "problem areas" constituted by them attain an orienting function competing with the disciplinary communities.

> Evaluation of research, communication and the attribution of reputation have to be achieved independently with reference to the problem areas. (Van den Daele et al., 1979: 55)

With respect to cognitive structures, too, the study suggests that the "contrast between disciplinary and problem-oriented research has to be seen as less distinct than has hitherto been the case" (p. 58).

4. Social accountability, reflexivity and quality standards

The second criterion of the independence of "Mode 2" research is the emergence of independent quality standards. This issue is connected with changes in the relationship of science to its social environment. "Mode 2" and "post-normal science" are supposed to be particularly sensitive to the impact of research. "Social accountability permeates the whole knowledge-production process" (Gibbons et al., 1994: 7). This is part of the context of application. The reflexivity entailed in this is similar to that of the interpretative tradition of the humanities which experience an increased demand for their knowledge. Research has to consider the options of implementation, and these touch upon the values and preferences of groups which have traditionally stood outside the science and technology enterprise. If one believes the optimistic authors, the enhanced awareness of possible problems of acceptance is reflected in the composition of research teams in which social scientists work together with natural scientists, engineers, lawyers, and businessmen (Gibbons et al., 1994: 7). Funtowicz and Ravetz shift from empirical description to normative demand (implying that things have not yet advanced quite so far) to state that "there is a need for a new, more pluralistic strategy of inquiry where the power embodied in quality assurance is more equitably shared among those with a legitimate concern for the consequences of scientific and professional work" (Funtowicz and Ravetz, 1993: 110).

Stephen Toulmin regards the postmodern, "humanized" science (and technology) as the triumph of reasonableness over reason (Toulmin, 1990: 198ff.). The superiority of local lay knowledge over that of the experts, like Wynne's sheep-breeders in Cumbria, who understand the ecology of radioactive fallout better than the atomic scientists in nearby Sellafield, is cited as evidence for the deficiency and moral decay of traditional science. "It is now widely

appreciated that the beliefs and feelings of local people, whatever their source and validity, must be recognized and respected lest they become totally alienated and mistrustful" (Funtowicz and Ravetz, 1993: 115). "Participatory science" is the key word (Gibbons et al., 1994: 148).

Under the regime of "political correctness", little can be added to so much goodness. But it is advisable to start by delineating the phenomenon. Again, the observations apply to certain areas of knowledge which are the model for "Mode 2": environment, health, communication, privacy, and reproduction (Gibbons et al., 1994: 148). They are areas of knowledge which are policy relevant; as political arenas they have emerged as a result of "scientification". Initially the issue was the implementation of certain technologies or their prevention, which has led to public debate and ultimately to legitimation crises of politics. This, in turn, has resulted in the institutionalization of reflexive fields of science or at least of reflexive mechanisms.

In arenas like environment and reproduction, there is the requisite technology assessment, in health there are ethics committees, in communication it is data protection. These, indeed, are new institutional arrangements which require new types of knowledge bearing the characteristics of "Mode 2". For areas of knowledge having no immediate connection to social values and subjective risk perceptions these conditions do not apply: high-energy physics, astronomy, and paleontology lie outside the concerns of citizens' groups and, at best, end up as issues in priority debates.

Similar arguments apply to quality control, which in transdisciplinary "Mode 2" knowledge-production is supposed to be taken away from the sole jurisdiction of disciplines and subjected to additional economic, political, and social criteria. New "adaptive and contextual forms of quality control" guarantee a more "socially accountable and reflexive mode of science". In the transdisciplinary mode, practitioners need not return to their disciplines for validation of their discoveries (Gibbons et al., 1994: 9, 5).

References to criteria of marketability and cost effectiveness are surprising in this connection (Gibbons et al., 1994: 8). Industrial research has always been subjected to these criteria. Here the trend may even go in the opposite direction. IBM affords itself the luxury of Nobel Prize-worthy basic research with ample freedom for its researchers and long-term perspectives. BASF buys into MIT and establishes a research institute with the strategic objective of cre-

ating greater opportunities for innovation by situating its research in the context of academic science and its intellectual infrastructure. It is an empirical question whether the competition between quality criteria is a dominant phenomenon, and it would have been helpful to have at least one of the many announced examples from the "biomedical and environmental sciences". Empirical evidence does not support such independent quality criteria. Industry looks for "cutting-edge" research across the world, and biotechnology remains bound by disciplinary standards even where it is directly transferred into practical application.

Another diagnosis would suggest that economic, political, and social criteria of relevance (which mean something different in each case) are being applied in *addition* to scientific criteria of quality, and that they have a closer bearing on the research process than has previously been the case. This happens *seriatim*, and in different ways depending on the area of knowledge.

The romanticizing appeal to the higher wisdom of "participating groups" so conspicuous in the concept of "post-normal science" has to be distinguished from the application of these additional criteria. It is evidently subject to a systematic error. The assumption that the participation of concerned groups or stakeholders and the resulting reflexivity lead to greater consensus overlooks the fact that the same conflicts of interest are reproduced on the next level. In the ethics commissions members of the medical profession confront social scientists or laymen. In the mediation schemes on environmentally relevant technologies, nuclear power, waste disposal, genetic engineering, etc., knowledge is applied strategically along the line of existing conflicts of interests. Agreements are reached only by way of political procedures and by juridification, or when the relevant research results can be brought from the stage of debate to that of social closure. This is corroborated by empirical evidence: if anything, participatory schemes have *increased* the level and intensity of conflict (Van den Daele, 1996).

The implied dichotomy science/concerned (lay) public is fixated on a model of science as elitist and as a source of (authoritarian?) political power. It misses the point of democratization, which is that virtually all political groups and interests have acquired access to scientific knowledge. But these groups resort to and use scientific experts as a political resource rather than claiming authority for their own (lay) knowledge. Democratization has also led to a different political treatment of the difference between privileged

knowledge and lay knowledge in that those holding privileged knowledge are no longer given undue authority. But under no circumstances does this mean the abolishment of the difference between expert and lay knowledge.

III. "Mode 2" in perspective

After the necessarily cursory treatment of the main features of "Mode 2" and "post-normal science", one can determine more precisely what the new production of knowledge is and what it is not. My thesis is that the discourse that goes with "Mode 2" and "post-normal science", inasmuch as it does touch upon new structures, can be referred to three interfering processes: (1) the scientification of politics; (2) the politicization of science; (3) the medialization of the relationship between science and politics, i.e. the scientific themes which are relevant for political legitimation are becoming topics in the media and thus objects of public attention.

1. The scientification of politics

All descriptions of the new type of knowledge-production converge in focusing on a specific institutional sector: environment, health, energy, and technology policy in a wider sense are the policy fields which have either been constituted by science in the first place and/or are particularly sensitive to new developments of systematic knowledge. They can all be characterized as belonging to that area in which science, politics, and the media interface, in which scientized politics finds its most conspicuous organizational expression. It is the area in which organizational mechanisms are devoted to scientific "reflexion" on the consequences for policy-making of the implementation of scientific knowledge. In the political system of the USA, this corresponds roughly to the "regulatory policy-making" in the context of which a corresponding type of research, "regulatory science", has emerged (Jasanoff, 1990). In the German system, one may regard the "proceduralization of law", in environmental law and other regulatory arenas, as an indication. This means that regulatory measures are connected to the actual state of knowledge and are changed in accordance with its progress. Historically this is a continuation of research in

the service of the state's welfare functions. However, its range has been expanded considerably since its inception, in the 19th century, due to both the growth of knowledge in the classic fields of state functions (e.g. health) and the knowledge-driven constitution of new functions (e.g. environmental protection).

2. The politicization of science

The fields of policy-making mentioned are not only particularly sensitive to knowledge development, but they are also very sensitive to threats to their legitimacy. Wherever high-ranking values are at stake (health, individuality, human dignity, etc.) and safety issues are involved, the state is called upon to fulfill its preventive functions. Not only does the sensitive nature of knowledge-development derive from the regulatory needs as such, but these, in turn, are connected to legitimacy. The proceduralization of law forces the regulatory agencies to take account of the newest developments in science. If they fail to do so, they are threatened with the loss of their legitimacy. This coupling of knowledge development and political legitimacy accounts for the political sensitivity of specific developments of knowledge. For example, the announcement of an anthropogenic impact on the global climate creates an immediate need to act politically (Engels and Weingart, 1997).

This is the type of mechanism that leads to the politicization of science: governments as well as NGOs and other groups participating in the political process use scientific knowledge to legitimate vested interests. In the fight for the better argument, they compete for ever newer knowledge and, thus, move closer and closer to their respective research frontier, i.e. to the realm where knowledge is still controversial and where social closure has not yet been achieved. Thus, scientific controversies, hitherto shielded from the public, are being reproduced in the political arena: nuclear power (above all radiation), gene recombination, AIDS, BSE ("mad cow" disease), to mention the great debates of the last decade. Here also lies the source of the paradox of scientification: the ever more intense recourse to knowledge on which to base decisions also reveals ever more clearly the state of ignorance. The need to decide and the risk of deciding are moving further apart.

The loss of authority of institutionalized science has often been

diagnosed as being due to public conflicts of experts. This is not supported by opinion polls, however, and is probably limited in time and to those experts debating specific issues. Even less justified are expectations that the monopoly of science on "truth" will be dissolved. The systematic point, with respect to the "additional criteria" of certification, which, in the contexts of application, supplement the traditional academic criteria of quality, must be seen in the temporal concentration of the "requirements of acceptance" for knowledge. Expectations of utility and economic efficiency vis-à-vis research are not new. But in some areas (e.g. biotechnology, information technology), the transfer time from basic research to technologies has been reduced to such an extent that the institutional distinction between the context of basic (academic) research and the (non-academic) context of application has become obsolete in organizational terms. The linearity of the model is replaced by patterns of feedback and recursivity.

The same may be said for political and social criteria of relevance ("quality" or acceptance). In the context of general democratization, an authoritative implementation of technologies is no longer feasible. The "distance" between science and the public has narrowed. The participation of the "many" constitutes the parameters to which knowledge-production has to be oriented. The originally luxurious and relatively isolated habitat of science, the ivory tower, has become smaller, and the need to adapt has increased. This is, in large measure, exacerbated by the new role of the media.

3. The medialization of the relationship between science and politics

The enormous increase in importance of the media in shaping public opinion and the perception of the world is already taken for granted. Because of this they have assumed a key role in legitimating and de-legitimating politics.

This key role obviously also affects their relationship to science. The new dependence of science on the media, or their new "media-consciousness", can be gauged by many indicators: efforts to professionalize science journalism, establishment of media and PR offices in scientific institutions, and the efforts on the part of science to instrumentalize the media through "pre-publication" or

"issue-management". Discourses on health hazards, risks of technologies or even "catastrophic threats" attract public attention, and create threats to political legitimacy and needs to act. The characteristic quality of this discursive interrelationship seems to be that the potential of threat becomes larger, the range of required actions more global, and their time horizon ever more long term.

Without the media, the problems of scientific research strategies and political decisions would be discussed in the remote expert committees of the executive. The media bring them to public attention and thus attach legitimating relevance to them. The differentiation of science policy, coined by Harvey Brooks into "policy for science" and "science for policy", in which the public does not appear, needs to be revised (Brooks, 1968: 85ff.; Guston and Kenniston, 1994: 30).

Can it be deduced from the analyses and descriptions of "Mode 2" and "post-normal science" that a "new" science is imminent? The analysis has shown that the features of "Mode 2" and "post-normal science" are limited to a fairly small sector of the entire science system. Empirical evidence for its having fundamentally changed is almost entirely lacking. The impression that the schemes are a normative program rather than an empirical analysis is increased by such statements as "our programme is ... to foster a rejuvenated science", "a new form of science" is being proposed, a "re-defining of knowledge" and a "political–ecological epistemology", even though the authors claim that their model derives "from its actual practice as revealed in historical, reflective and critical studies" (Funtowicz and Ravetz, 1993: 117, 120, 121).

The central issue is whether the observed and postulated changes in knowledge-production mean a reversal of the functional differentiation of science. If this question were to be answered positively, proof would have to be provided that the observed developments correlate with changes in epistemology. Funtowicz and Ravetz explicitly mention this crucial criterion of the establishment of a "post-normal science". According to them "truth" continues to be the objective but it cannot be proved. The objectivity of science is not created through logic, but by a social process of the application of craft skills guided by ethical principles (Funtowicz and Ravetz, 1993: 121).

The thesis that these epistemological standards have replaced the traditional ones can be tested empirically. There are, indeed, tendencies to establish new epistemologies, e.g. in the form of "re-

centering programs": physiocentrism, feminism, ethnocentrism are going examples. Developments of this kind are limited to subject matters which are potentially value-laden: the "human and social sciences, technical sciences and the technological post-paradigmatic natural sciences as well as all research fields in which different research lines merge to heterogeneous complexity" (Krohn, 1996: 21). But hardly anyone will seriously claim that any of these programs can count as generally valid and binding for the rest of science or that one will in the foreseeable future. Nowotny (in a talk at a conference in Potsdam) admitted, regardless of all other claims about the correlation between institutional and epistemological changes, that the "epistemological core" remains untouched. Thus, it seems that the "new forms of knowledge-production" and "post-normal science" do not describe revolutionary changes of traditional science after all. Rather, they are descriptions of specific institutional changes.

The dissensus vis-à-vis "Mode 2" therefore does not so much pertain to the observations as such as to their interpretation. My critique is that they are descriptions of surface phenomena, that their interpretation as fundamental changes of science is flawed because the various authors do not distinguish analytically between functional differentiation on the institutional level and changes of organizational boundaries and identities. The suggestion to revert to categories like "scientification" is based on theoretical considerations, which helps to get beneath the surface. It is ironic that in science policy departments in Brussels and Bonn administrators are working with the related concepts of the "information society" and the "knowledge society", respectively, as new legitimating concepts for science policy. The concept of "knowledge society" has the advantage of not carrying the burden of proof hampering "Mode 2", but leaves the issue of the shift of institutional boundaries at the center. In the few examples of "Mode 2", the fields concerned are primarily those which can no longer very well be isolated institutionally as a "special world of discovery". They are the expression of the expansion of knowledge-based fields of action (Krohn, 1996: 20). They are to be found in the border area which emerged through the increased importance of knowledge in modern societies and the expansion of the institutional boundaries of science. In this area there is a developing uncertainty about the identity of the system.This uncertainty is augmented by the perceived erosion, in all leading Western industrial societies, of the

social contract according to which investment in basic science was not tied to expectations of immediate utility. Now the time credit is being reduced, the control of returns is becoming more direct. The elbow room enjoyed by science is contracting. However, this is a surface phenomenon which may indicate a change in the organizational characteristics of academic science as we have known it since the mid-19th century. But this has to be kept separate analytically from another process actually running counter to it, which is captured by what I have termed "scientification" (it also appears in the concept of "knowledge society"): a process whereby the use of and claim to systematic and certified knowledge produced in the spirit of "truth-seeking" science becomes the chief legitimating source for activity in virtually all other functional subsystems. What appears as a dissolution of the identity of science on the organizational level is, in fact, a strengthening of institutional identity. The "knowledge society" is obviously not characterized by the dominance of traditional organizations of academic science over other organizations, but by the centrality of systematic knowledge in reaching decisions, political, economic, legal, etc.

Failure to see this difference leads to false conclusions. Thus, the claims about the "new production of knowledge" remain ambiguous. They reflect the ambiguity between a nostalgic clinging to the social contract for an unconditional alimentation of pure research, on the one hand, and a romantic trust in the higher rationality of a participatory science, on the other.

IV. Then and now

It is noteworthy that the theses concerning the "new production" reveal obvious parallels to those concerning the "finalization of science" postulated more than two decades ago. The latter was formulated in the context of a discourse which was still characterized by polarization: unquestioned belief in individual freedom, on one side, reliance on the rationality of the state and, thus, in the planning process, on the other. "Mode 2" and its correlates are situated in the discourse whose central element is the rationality of decentralized democratic participation.

During the 20-odd years which separate the earlier debate on "finalization" from that of "Mode 2", the context has changed considerably both in terms of the ruling ideology and of the relevant in-

stitutional arrangements.The ideological connection between basic research, freedom of science and freedom of the West in the context of the Cold War motivated the nervous and sometimes even fearful reactions against "finalization", which was identified by opponents with the "planning" of research and, therefore, with a threat to the freedom of science. Now the tide has turned. Against the backdrop of tightening research budgets and four decades of almost uninhibited growth, the orientation of science to societal values is seen as a new virtue, a promising source of legitimation.

The important similarity between the "finalization" and "Mode 2" schemes is the "external", i.e. sociopolitical orientation of science as the ultimate stage in the evolution of the relation between science and society. However, the "finalization thesis" postulated that only those fields of science could be oriented to external goals which had reached a "post-paradigmatic" stage of development. By implication, this means that the priority of theoretical development remains a prerequisite for "scientification". In "Mode 2", this important distinction is lost in favor of the romanticized appeal to the higher rationality of lay knowledge.

The shift in discursive context explains why, despite the similarity between "finalization", "Mode 2", and "post-normal science", the latter two have found their way into the rhetoric of science policy-makers. However, this recent receptivity to the schemes of new forms of knowledge-production must not be mistaken for the factual changes that have taken place. As in the case of "finalization", the descriptive content of the "new forms" should, in all likelihood, turn out to be much less straightforward than suggested by the fashionable acronyms and catch-words. This raises the disquieting question as to what extent our analyses of science and the supposed changes are a product of our wishful thinking and to what extent we are able to abstract from it.

Peter Weingart is Professor of Sociology and Sociology of Science at the University of Bielefeld, and Head of the Institute for Science and Technology Studies. From 1989 to 1994 he was Director of the Center for Interdisciplinary Research. He has published widely in sociology of science and science studies; among his recent book titles are: *Rasse, Blut und Gene* (with J. Kroll and K. Bayertz), 1988; *Doppel-Leben,* 1995; *Biology as Society, Society as Biology: Metaphors* (ed. with S. Maasen and E. Mendelsohn), 1995; and *Human by Nature. Between Biology and the Social Sciences* (ed. with P. Richerson, S. Maasen and S. Mitchell), 1997. *Author's address*: Fakultät für Soziologie, Universität Bielefeld, Postfach 100131, 33501 Bielefeld, Germany.

References

Böhme, G., Van den Daele, W. and Krohn, W. (1973) "Die Finalisierung der Wissenschaft", *Zeitschrift für Soziologie* 2(2): 128–44.

Brooks, H. (1968) *The Government of Science*. Cambridge, MA: MIT Press.

Elzinga, A. (1995) "Shaping Worldwide Consensus: The Orchestration of Global Climate Change Research", in A. Elzinga and C. Landström (eds) *Internationalism and Science*, pp. 223–55. London: Taylor Graham.

Engels, A. and Weingart, P. (1997) "Die Politisierung des Klimas. Zur Entstehung anthropogenen Klimawandels als politisches Handlungsfeld", in P. Hiller and G. Krücken (eds) *Risiko und Regulierung. Soziologische Beiträge zu Technikkontrolle und präventiver Umweltpolitik*. Frankfurt a.M.: Suhrkamp.

Etzkowitz, H. (1990) "The Second Academic Revolution: The Role of the Research University in Economic Development", in S.E. Cozzens (ed.) *The Research System in Transition*, pp. 109–24. Dordrecht: Kluwer Academic Publishers.

Frühwald, W. (1996) *Der Herr der Erde – Technikbilder der Moderne oder die neue Produktion des Wissens*. Festvortrag zur Eröffnung des Deutschen Museums Bonn. Reprinted in *MPG-Spiegel* 2: 59–64.

Funtowicz, S.O. and Ravetz, J.R. (1993) "The Emergence of Post-Normal Science", in R.von Schomberg (ed.) *Science, Politics, and Morality. Scientific Uncertainty and Decision Making*. Dordrecht: Kluwer.

Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P. and Trow, M. (1994) *The New Production of Knowledge*. London: Sage.

Guston, D.H. and Kenniston, K. (1994) *The Fragile Contract*. Cambridge, MA: MIT Press.

Jasanoff, S. (1990) *The Fifth Branch*. Cambridge, MA: Harvard University Press.

Klein, J. Thompson (1996) *Crossing Boundaries, Knowledge, Disciplinarities, Interdisciplinarities*. Charlottesville: University of Virginia Press.

Krohn, W. (1996) *Wissenschaftsfortschritt und Dezentrierung – Piagets Modell der evolutionären Epistemologie und die dekonstruktionistischen Angriffe auf die Objektivität und Universalität von Erkenntnis*. Bielefeld: IWT-Paper 9.

Küppers, G., Lundgreen, P. and Weingart, P. (1978) *Umweltforschung – Die gesteuerte Wissenschaft?* Frankfurt a.M.: Suhrkamp.

Lundgreen, P., Horn, B., Krohn, W., Küppers, G. and Paslack, R. (1986) *Staatliche Forschung in Deutschland 1870–1980*. Frankfurt a.M.: Campus.

Schäffer, W., ed. (1983) *Finalization in Science. The Social Orientation of Scientific Progress*. Dordrecht: Reidel.

Toulmin, S. (1990) *Cosmopolis – The Hidden Agenda of Modernity*. New York: Free Press.

Trepte, A. (1996) "Knowledge for the Twenty-First Century: Strategies of the Max Planck Society", in W. Krull and F. Meyer-Krahmer (eds) *Science and Technology in Germany*, pp. 125–44. London: Cartermill.

Van den Daele, W. (1996) "Objektives Wissen als politische Ressource: Experten und Gegenexperten im Diskurs", in W. van den Daele and F. Neidhardt (eds) *Kommunikation und Entscheidung. WZB Jahrbuch*, pp. 297–326. Berlin: Sigma.

Van den Daele, W., Krohn, W. and Weingart, P., eds (1979) *Geplante Forschung*. Frankfurt a.M.: Suhrkamp.

Weingart, P., Sehringer, R. and Winterhager, M. (1990) "Which Reality do we Measure?", *Scientometrics* 19(5–6): 481–93.

Weingart, P., Prinz, W., Kastner, M., Maasen, S. and Walter, W. (1991) *Die sogenannten Geisteswissenschaften: Außenansichten. Die Entwicklung der Geisteswissenschaften in der BRD 1954–1987*. Frankfurt a.M.: Suhrkamp.

Whitley, R. (1984) *The Intellectual and Social Organization of the Sciences*. Oxford: Clarendon Press.

Ziman, J. (1995) " 'Postacademic Science': Constructing Knowledge with Networks and Norms". London: Royal Society Medawar Lecture (manuscript).