Contextual Transformations in Contemporary Science

by

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Introduction

If one listens to scientists and reads editorials in *Science* and *Nature*, changes in science, and in particular changes for the worse, appear to be going on everywhere. One commonly heard concern was phrased by Don K. Price (1978) as the question whether science has turned from the endless frontier into a bureaucratic morass. The other main complaint is about intrusions of science policy on fundamental research. In both cases, the scientists and their spokesmen tend to blame governments, politics or society in general, often without detailing the specific causalities. From such a perspective, "keeping science straight" appears to be a matter of neutralizing such external influences. The problem, however, may be less simple. This is Price's (1978) argument: scientists have been pushing the cornucopian nature of science, and should not be surprised when somebody is taking them up on their promises. And the blame may not lie so much with science policy and other policy makers, as with researchers "eager to extend the 'endless frontier' of science and to trade research results for research resources" (Wittrock 1985, p. 16).

When it is accepted that the situation is more complex, further questions
related to the issue of "keeping science straight" can and should be raised. For example, have scientists, in mobilizing resources and making claims for science, changed their ways of doing science? This possibility has been noted by Ziman (1983, 1984) who claims that changes in the sociology of science will lead to changes in its epistemology. Ziman's main task is to diagnose some of the changes in the sociology of science under the label of "collectivization". Similarly, Elzinga (1985) has posited a drift of epistemic criteria in the performance of research through relevance pressure and accountability pressure. My question in this paper is not primarily diagnostic, but rather about the dynamics of these changes. Do we understand what it is that we are diagnosing?

My starting point is that complaints, and attempts to apportion blame, will not help much, and probably hinder understanding. For instance, there are complaints about the decreasing funds and/or decreasing freedom of scientists. This would undermine the vitality of the research system (Wittrock 1985, p. 16), and reduce innovativeness (Ziman 1984). Examples that are given indeed show shifts in the locus of decision making, e.g. for decisions on which research problems to address. But that may be a shift, not from science to policy, but from one group of scientists ("at the bench") to another ("in the committee room"). That is, there are structural changes going on, and 'innovativeness', as well as other features of science, may become redefined in the new situation.

Too quick a diagnosis may not recognize the issues for what they are, and lead to misguided therapies. The phenomenon of Big Science provides many examples, an ironic one being how New Scientist (1983) on the occasion of the discovery of W and Z particles by teams amounting to about 200 physicists proposes to have Nobel Prizes awarded to teams:

Science has changed a great deal since the end of the 19th
century, when Nobel died. Perhaps now that "big science" has become more mature, and scientists in more disciplines discover the benefits of pooling resources in research, the time has come to review the logic in identifying individuals, a process that is so often divisive.

The irony lies in the fact that it was a concern about Großbetrieb in science, i.e. national scientific enterprises like the German one, dominated by large research institutes, division of labour, and heavy investments in material and equipment, that led to reactions around 1900, including emphasis on prizes like Nobel's to highlight individual achievements in science. (Crawford 1984, p. 204).

Even without historical comparisons, commonly heard diagnoses can be checked. Johnston (1985) has shown, for example, that concerns about decreasing resources (in his case, of Australian scientists) are not related to total resources (which were stable or perhaps increasing), but to discretionary resources, which can be used to follow up new research questions. Thus, the concern of scientists about resources remains a real one, but should not lead to global claims for higher budgets, but focus on different allocations and institutional mechanisms.

Thus, that something is changing in contemporary science is not at issue. But the simplistic idea of policy inroads into the Republic of Science cannot be the final answer. Not only have scientists actively collaborated in bringing about such a situation, the Republic of Science idea is also misleading in that it suggests that the basic ways of doing science remain (or should remain) the same. In fact, I will try to show that structural changes are occurring in science, which affect its sociology as well as its epistemology.

To do so, I shall develop a perspective on changes in science which builds on
recent insights of "constructivist" sociology of science, but takes a further step by applying this approach on the meso- and macro-level, not only on the micro-level of construction of facts in a laboratory. As Peter Healey noted (1982, p. 15): "After all, if science is constructed in laboratories, laboratories are in part constructed in research council committees." Funding agencies for science, which have become an accepted feature of the scientific scene since 1945, are a good example to analyze from this perspective, and such analyses can be used to elaborate it further. But the role of funding agencies is not the only example. The increasing importance of mission orientation in science and conscious, strategic mobilisation of science for political goals, have resulted in a new layer of institutions (in the sociological sense of the word), like programming bodies, implementation agencies, and ways to evaluate "relevance". And thirdly, issues of scientific expertise and public understanding of science, both related to public legitimation of science, can also be understood better with the help of this perspective.

Resource mobilisation in a changing environment

As was noted in the introductory section, the dynamics of contemporary science are sometimes conceptualized as those of a reasonably well functioning system under pressure from a changing environment. As has been discussed extensively in organisation studies, especially from a resource-dependency perspective (Pfeffer and Salancik 1978), organizations respond by developing new "routines" — in the case of the science system, one could think of the addition of 'accountability' and 'relevance' to the CUDOS norms — and by negotiations about boundaries and responsibilities. This is indeed visible, and in the course of my analysis I will come up with some further examples. But there are dangers in this conceptualization, especially if one wants to prepare
the ground for evaluation as well.

The dangers can be recognized by noting that the picture is too limited on two counts. First, and obviously, the Republic of Science does not exist, i.e. science is not one system, but an assemblage of interrelated elements (institutions, texts, communication networks, knowledge products) belonging to different organizations. Insofar as it has some unity, it is as a research system, often recognizable as a national research system (cf. OECD 1972, 1973, 1974). Thus, it could be called, after Scott (1983), an organisational field at the level of institutions.

Second, scientists are not functionaries following the scientific method, or CUDOS norms, or whatever rules and routines appear to be prescribed, but entrepreneurs, mobilizing resources and acting strategically to further their own ends. Although one speaks of the production of scientific knowledge, and economic metaphors have been used to analyze the working of science (and criticized, e.g. by Knorr-Cetina 1981), the scientific entrepreneur is more similar to political, than to economic entrepreneurs. When scientists mobilize resources, as well as when they present their products to audiences, they have to justify their actions and their products, often as part of their action (for example, in writing an article or a research proposal). Their success does not depend on a market taking up their products, but on their justifications being accepted. (Rip 1987b) In a comparable way, the production of authoritative decisions in politics cannot proceed in a purely technical way, but depends critically on justifications and their acceptance.

From this perspective, the dynamics of change in contemporary science can be conceptualized as taking place on (at least) three levels. One, there are the scientific entrepreneurs mobilizing resources (intellectual, technical, justificatory resources, but also funds, institutional support and social legitimation).
Two, the organizational field of scientific institutions (which include research councils and universities, but also institutionalized peer review). And three, the general social context (think for example of the rise of the welfare state and its use of science, both instrumentally and symbolically). On each level, one can find specific dynamics, e.g. of research problems and knowledge products (traditionally analyzed by history and philosophy of science), and of the institutions of science (as studied in sociology of science). There is also coupling between the levels. The institutions of science constrain actions and interactions of scientists, while at the same time enabling them, e.g. by providing legitimations and opportunities. On the other hand, the outcomes of the scientists' actions add to and modify the organizational field. This "dual contingency" (Anthony Giddens’ term) is the first crucial point to be made about the contextual dynamics of science.

The notion of "dual contingency" can be demonstrated for the particular cases of changes in research and disciplines. Scientists base their work on the state-of-the-art and the rules of the reigning paradigm. But their products, i.e. their knowledge claims, often try to redefine the orthodoxy, if only slightly. In order to get a hearing, they have to claim something new, and interesting. If this claim is taken up by others and made part of their work, the research front and, in time, the knowledge core of the scientific field is changed. Recent ethnographic studies of scientists in their laboratories and institutes, as well as debating among themselves, have detailed these processes. A scientific article, for instance, is now recognized as effecting a double transformation: from the "tinkering" (Knorr 1981) and "heterogeneous engineering" (Law 1987) in the laboratory to a text that can be sent out into the world and act upon intended audiences; and contained in this text, a transformation from the research front as-it-was to the research front as-the-author-claims-it-is (or will be). Thus, the local production of knowledge, conditioned by concrete local circumstances and conditions, leads to changes in de-localized scientific fields.
In this example, only one aspect of the dynamics of science is taken up, and it is assumed that the institutions of science are more or less constant. Even when keeping to this latter assumption, the picture can be made more complicated, since resources (whether cognitive or reputational, i.e. deriving from the scientific field, or financial, personnel, or legitimatory) always come with this attached to them. (The phrase was coined by Zeldenuist 1985) In other words, the credibility cycles within which the scientists operate with respect to scientific fields are coupled to scientific institutions in which scientists work, from which they get funds, and through which they are provided with career possibilities and legitimation. By studying such linkages, one can introduce a much-needed structural aspect into the constructivist sociology of science. Or, in other words, by specifying what the linkages are, at least part of the "transepistemic arenas" (Knorr 1982) of science can be detailed.

Credibility cycles for corporate actors

The notion of 'credibility cycle' is adapted from Latour and Woolgar (1979). The two essential aspects of a credibility cycle are that resources are transformed into one another (scientific articles into scientific credit into funds and access to materials and information into productive research into scientific articles) and that the transformations are driven by (more or less justified) claims that are honoured.

This definition is slightly more general than the one used by Latour and Woolgar (1979); the advantage is that it now allows application to scientific situations (and other corporate actors), for example a research council. A research council must get high-quality proposals in order to dispense enough
money to at least maintain its budget for the next year. The important point is that one can see how this credibility cycle is coupled to the credibility cycles of scientific entrepreneurs (individuals, but more often leaders of research groups), both by their needs for funds, and by their role in reviewing research proposals and thus legitimating the decisions of the research council (by referring to scientific merit and relevance to scientific fields).

Figure 1
The credibility cycle for a scientist
(adapted from Latour and Woolgar 1979)
The research councils themselves have to earn their budget by showing to governments and the public that they are doing worthwhile things with their money. The "Golden Fleece" Award for the stupidest project funded by the US National Science Foundation indicates how politicians tend to look at research councils ("Why are they giving the taxpayer's money away to study how monkeys grin?"). Research council officials and board members are very sensitive to these issues. In the same vein, they like positive publicity, and urge scientists to acquire it. Diffuse publicity as well as formal and informal links to (other) government agencies and patrons couple these intermediary institutions to wider social context.

The mutual dependence of research councils ("buying science") and scientists ("dividing the spoils") is a feature of the present situation that has emerged and stabilized in recent decades (Rip 1985). The "struggle for fundability" has become institutionalized by now: To obtain funds from funding agencies, traditions have been established to submit project proposals, and sometimes proposals for institutional grants, and subject them to decision making supported by peer review. Criteria of scientific quality can be applied, presumably in a systematic way. The criteria debate of the 1960s (see Shils 1968) can be seen as an indicator of the transition having occurred. Since then, the system is often seen as the mainstay of science (compare most articles in the special issue of *Science, Technology & Human Values* (Summer 1985 on peer review). Again, this is not a criticism of peer review of proposals – which has its possibilities and limitations – but of the short memory when it comes to institutions of the science "system". What is considered to be hallowed, cannot easily be evaluated.

The system, never fully stable, is now slowly, and consciously, changing, because of the ideas about strategic science, relevance pressure etc. (Van
Rossum 1986). One can imagine that such pressures change the behaviour of research councils in their credibility cycles (which in fact happens) which will have impacts in the credibility cycles of the scientists (which is the concern of many). Thus, a mechanism is provided how the social relations of science can influence its epistemology.

Figure 2
The credibility cycle for a research council
Further differentiations in the social system of science

The mechanism of interaction between social context, institutions and scientific practices, as outlined here, captures a general feature of the dynamics of contemporary science. Each level has its own dynamics, but the linkages lead to coupling; this is the second crucial point to be made. To elaborate this point, I start with what is the essential linkage: claims about scientific knowledge products or knowledge production and status of scientists become coupled to contextual elements. This linkage has emerged in two phases. Coupling between scientific knowledge production and professional careers started in the 19th century especially after 1870, when state patronage for basic research was sought and increasingly obtained. The justification of disbursing state funds had to come from the scientific products (or the promise of them). This is now general practice, and recently includes additional features like the use of citation scores in academia and dual ladders for careers in industrial research and some governmental institutes.

More recently, intermediary institutions (like research councils, and universities as receivers of state funds for research) have emerged where the same coupling applies but at the level of agencies, or of corporate actors in general: their use of state funds has to be legitimated in terms of the science that is or could be produced. The mutual dependence between the scientific entrepreneurs and the institutions of the contemporary science system has been discussed in the preceding section. In the last 10 or 20 years, a new layer of institutions, explicitly oriented to "missions", to programming, to strategic mobilization of science, is emerging. One can see this as a response to wider social changes, e.g. the pressure for social relevance in the 1970s and the attempt to renovate the economy through scientific-technological innovations in the 1980s. But the additional dynamic is, first, that scientists themselves are also offering "early promises" (whether they promise new research or relabel
ongoing research does not matter for our argument), and, second, that programming bodies try to select what is, or could become, relevant. The latter bodies are forced to make hard choices, not only because it is difficult to distinguish between "good" and "bad" promises from scientists, but also because it is important for them to maintain a portfolio of "relevant" projects in their programme, to maintain credibility with respect to their contexts. (Rip and Nederhof 1986, p. 265) This whole change also includes the emergence of criteria, and in general, a repertoire for judging relevance that is shared between actors. On top of the struggle for fundability, there is now a struggle for relevance.

Figure 3
Struggles for relevance and legitimacy on top of the traditional struggles for facticity and fundability
When state patronage of science – for which research councils were one of the mediating institutions – shifts to include the strategic use of and priority setting for science, this constitutes pressure on the network of institutions. Even if new mediating institutions may be thought necessary and are created, these have to be formed in an existing organisational field, and adapt themselves to it. An illustrative example is the way how the Dutch Programming Committee Biotechnology had to adapt to existing structures, including ZWO, in order to realize some of its goals. (Rip and Nederhof 1986) So at the level of corporate actors, some of the mechanisms of resource mobilization in opportunity structures can be found, only the wider context (government, social groups, international relations) now becomes more important, and cannot be changed easily by the actions and interactions of science institutions.3

To individual scientific entrepreneurs, the intermediary institutions and other contexts coupled to science appear as an opportunity structure to be exploited. However, by perceiving opportunities and making use of them, they shift the balance of opportunities more generally. In this way, for instance, scientific fashions emerge: when enough scientists make a certain decision, others cannot afford to say no any more, and the bandwagon starts to ride. In the same way, policy priorities can get implemented, especially in more glamorous areas like biotechnology, micro-electronics and parts of new materials. (See Rip and Hagendijk 1987, and for the argument about opportunity structures, Rip and Nederhof, 1986).

Thus, the activities at micro-levels and meso-levels may introduce changes at a higher level of aggregation, which cannot be reversed easily. In the same way, Ziman’s descriptions of collectivization show how the coupling appears to individual scientists (others are getting a say in the management of their credibility cycles), but should also be seen as indicators of a change at the level
of institutions — what Ziman calls the sociology of science. It then supports the general perspective outlined before: do not just blame science policy, but do not blame scientists exclusively either. Scientists, funding bodies and other agencies collude in creating the "directed autonomy" of contemporary science.

Struggle for legitimacy

"Directed autonomy" may well appear in further aspects of scientific practices, although the dynamics will be less clear when there are not obvious mediating institutions. There are two domains where one can see couplings similar to the ones traced for funding and programming. Both domains are related to the issue of legitimation with respect to societal actors.

Scientific experts/expertise in advisory contexts and in public controversies, the first domain, is itself a complex and controversial area, but it is possible to single out some important trends. Originally, the advisory context was structured by an understanding between advisor (scientific or other) and advisee, when their relation was relatively private. Professionalization of such relationships occurred, especially after the second world war. Expertise was needed in ever more areas to ground and/or legitimate decision-making, and institutionalization led to the emergence of a scientific advisory "game" with recognized rules. Now that experts appear in public arenas, however, and advice has to be made public in order to confer legitimacy on decisions, the "rules of the game" do not apply anymore in a simple way. (Rip 1985a) Advisors have to achieve credibility with wider publics, not just the decision-maker who commissioned their advice. The rules for construction and de-construction of scientific facts will shift accordingly, for instance when battles
between experts and counter-experts become a public spectacle, or when lawyers, with their standards of evidence, undermine testimony of scientific experts. (Oteri, Weinberg and Pinales 1973) Controversies exacerbate such credibility problems; they may well be an additional burden, but are not the cause of the credibility problem, as some authors tend to think.

Scientists and administrators, in their attempt to recreate some stability, try to regain the boundaries lost. One example is the well-known, but never fully implemented proposal to have Science Courts. (Kantrowitz 1975) Whatever the institutional possibilities, calls by scientists to close the ranks abound. In the controversy about the ozone layer in the 1970s, such statements can be found, sometimes harsh ones (people should be forbidden to speak out), sometimes without explicitly invoking a rule:

At the moment, half-baked ideas are being produced at a ferocious rate. That's all-right when you're only talking to your friends. But it's most regrettable that scientists are telling politicians that they must regulate (as if the evidence was hard).
(M. McElroy, quoted in New Scientist 70 (24 June 1976) 685)

The interest of the quote is in how it tries to define and defend a boundary, while recognizing that there is a legitimate role for scientists to speak out and raise an early warning based on their expert, even if insecure knowledge.

The problems of scientific advice continue to be discussed: participants are clearly struggling to come to terms with the changes. This indicates that the transition, if it is one, has not yet stabilized. In the terms of this paper: the struggle for legitimacy cannot yet be very productive because the rules are not clear, let alone accepted by various parties. (Rip 1986a) Credibility cycles, with their repertoires and linkages, are still absent or fragmented; although there are domains where patterns begin to emerge. Regulatory science is a domain where controversies have been fierce, but rules that bring order are
being accepted. For example, there is recognition of the necessity of "pragmatic rationalism" (Ezrahi 1980, Rip 1985a), while at the same time some institutional separation is attempted between "scientific assessment and deliberation" and "advisory reports", which take economic and political contexts into account. The tension between those two trends is itself resolved pragmatically, it appears.\footnote{4}

The linkages between "science" and "the public" are not exhausted by scientific expertise in public arenas. The whole domain of communication of science and public understanding of science is cause for concern, and subject of an increasing number of studies, reports and official and unofficial position statements. Again, diagnosis and therapy is not always based on understanding of what is happening. A curious example is the report *Science in the Streets*, the result of the discussions of a distinguished working party gathered by the 20th Century Fund (1984). The report contains, as an appendix, an extended background study by Dorothy Nelkin on the role of the media, where she concludes that media, in general, do a good reporting job, with little exaggeration and with attention to the necessity of letting different sides of an issue be discussed. In contrast to this conclusion, the working-party report contains an attack on irresponsible media, as if the problem of interaction between science and the public can be solved by scapegoating one of the actors!

What seems clear, is that a new "intermediary institution" is in the making. Of course, there have always been activities of individual scientists to reach publics. Victorian Britain saw many eminent scientists lecturing all over the country, a tradition that continued into the 1930s. In the US, and somewhat later, some professional societies have been active, e.g. the American Chemical Society. (Thackray et al., 1985) But there are now professionalized activities, staff groups, science journalists etc. etc. Clearly, "the" public is a
third party that cannot be neglected: it is itself becoming a constituency for science that can confer or withhold legitimation, credibility, and indirectly also other resources.

A concurrent change in the practices of science is that media/the public become a resource in the struggle for fundability: press conferences and media coverage are actively sought (compare also note 2), while new rules emerge to specify the relation with regular scientific publication. An older example is the Ingelfinger rule (in biomedical science): press releases can appear only when an article actually appears in a referred journal. More recently, additional interests have come into play, when early promises, e.g. about new biotechnologically relevant findings, are made during conferences or in specially arranged meetings, and have effects on the value of the shares of R&D firms. Another trend is that there is renewed interest from scientists in “performing” before a public; it is not seen as an activity for failed scientists anymore. The main effect of these changes in the short term is a flywheel effect: early promises become exaggerated through playing them out before the public. This can already happen with scientific findings that have a curiosity value -only, as in the example of the polywater affair. (Franks 1981) For biomedical sciences, the relationship with illness and health makes flywheel effects unavoidable, and playing upon it in a publicity “crusade” has almost become a new tradition. (cf. Panem 1984) Again, the concern about such activities should not immediately lead to attempts to contain them through external rules and provisions, but first elucidate the dynamics in order to understand what is driving the system.
Conclusion

In setting out a perspective on changes in contemporary science, I have attempted to include a variety of phenomena, and data from a variety of sources, into one analysis. There are dangers in such an attempt, including the danger of specious unity where there are only fragments. But the general notion of contextual transformations of science defended here can be made plausible in two ways. First, by the grounding in general sociological theory, references to which were made in passing, and which amounts to analysis of multi-level quasi-evolutionary development. Second, by showing that this notion can be fruitfully applied to other periods of science.

In particular, the late 17th century can be analyzed in terms of a "positive" contextual transformation of science: Van den Daele (1977), among others, has discussed the outcome in terms of institutionalization of a "positive" science, while others, e.g. Shapin and Schaffer (1985), provide particular details that can even be rephrased in terms of emerging credibility cycles. It is possible to analyze the role of the Royal Society and the Académie des Sciences (and later also other scientific societies) in terms of their functions with respect to scientific practices (arbiters, communication junction etc.) and with respect to social context (channel to the powers that be, legitimation broker). Without neglecting the differences, there seem to be structural analogies with the role of national research councils in our century.

I would contend that the 19th century is the scene of another contextual transformation of science, a "professional" transformation. (cf. also Mendelsohn 1964) The new social division of labour in bourgeois-industrial society created opportunities for scientists to shape out professional jobs in education, in expert advice and experimentation, and after these kinds of jobs had become institutionalized, also positions in research per se. There are
parallel epistemological changes: from the earlier tradition of building (scientific) pictures of the world, to disciplines and discipline-oriented re-
search, including "regional" theory development; also separate attention to
scientific method as something that can be taught and discussed; and in relation
to this, the rise of the ideology of rational, as well as instrumentally useful
science, as two sides of the coin. Specific linkages and sequences in this
long-drawnout process (which continues, in fact, until today) can be further
specified, although the patterns are quite complex.

What one sees happening in contemporary science might be termed a third
contextual transformation, this time a "political" transformation. Together,
the three contextual transformations are a way of conceptualizing secular
change in science in which internal and external, social and cognitive changes
are intimately related, and in which patterns, stabilization and transition
replace ideological notions of "progress".

The other question that must be asked is whether the conceptualization and
analysis leads to new insights, and to insights that could not be obtained in
another way? In any case, I have demonstrated that there is no easy reference
to an earlier and better state of science. This implies also that one cannot fall
back upon normative criteria derived from a philosophy of science: there is no
Archimedean point outside science from which the worth of certain
approaches can be evaluated. Often, this view is described, i.e. condemned as
social relativism. In fact, it is the recognition of an evaluative circularity: the
production of valid (and of high-quality science) appears to require certain
institutions (including concrete institutional linkages as institutional norms)
but what counts as valid, or of high quality, can only be defined as what is
produced in such institutions. (cf. also Ziman 1984) A quasi-evolutionary
approach, as advocated here, does not offer an easy way out: survival cannot
be a normative criterion, because it is determined by a "selection environment", and why should a selection environment be "right"? But such an approach does show that one cannot look at validity and quality of science without taking institutions and other linkages with context into account. In fact, what one could, and should, work toward, is epistemologically relevant externalistic sociology of science or ERESS. The term and its abbreviation are less important than the thrust of the idea. Within this overall thrust, there are several ways to do ERESS. It is possible to further analyze the phenomena (if indeed they are phenomena) of "epistemic drift" (Elzinga 1985, see also note 1) and "collectivization" (Ziman 1983, 1984). There have been attempts to apply the theoretical concept of "hybrid research communities" (Van den Daele, Krohn, Weingart 1979). At the moment, I am intrigued by the possibilities offered by quality control (the notion and the practices) in science and of science as entrance point. (Rip 1987b, see also Sechrest 1987)

The thrust can be summarized in two sentences: There is not, and never was, a "straight" science that should be "kept" straight. But some contextual transformations are better than others.

My point in this paper has been that evaluations of what is better should not be made too quickly. A sociological detour is necessary to provide understanding of the dynamics of the transformation and the role of the various actors in it. And sociology of science has by now made enough progress to help make this detour, provided it does not isolate itself from sociology in general, history and economics and political science.
Notes

1. Elzinga combines his global diagnosis with a claim about causalities: "In the light of the foregoing theoretical framework, epistemic drift may thus be interpreted as a shift from a traditional reputational control system associated with disciplinary science to one that is disengaged from disciplinary science and, thus, more open to external regulation by governmental and managerial policy impositions. The norms of the new system have a strong relevance component, transmitted from the bureaucracies to which the hybrid research community is linked. The bureaucracy thereby influences not only problem selection but also the standards of performance of research, standards of significance and territorial definition of the field or speciality in question." (Elzinga 1985, p. 209) Elzinga's analysis is compatible with the one I will give; compare also a later quote: "We see then a shift of values, norms and ideals of science at several different levels. The phenomenon here referred to as epistemic drift at the level of research performance seems to be attended by a general shift in the social paradigm of research." (Ibidem, p. 210) Examples of the latter shift are new contractual and communicational relations that have emerged between researchers and financiers. Compared with Elzinga's analysis, I want to know more about mechanisms, and I would not contrast "disciplinary science" so easily with "hybrid research communities": the reason to privilege so-called disciplinary science is only that it has been institutionalized in a specific way.

2. Susan Cozzens (1985) reports about Solomon Snyder's work on opiate receptors: "This team's results were announced with great fanfare. At the urging of the funding agency which supported the work, a press conference was held, and Walter Cronkite reported the story on the evening news. It also was covered in a number of papers around the country. Snyder's department chairman actively sought opportunities for the work to be presented."

3. The (partly independent) dynamic at the level of institutions is, apart from it being the outcome, on the collective level, of individual actions and strategies, also "mandated" by wider contexts: through design of institutions, science policy and its implementation on the level of institutions (e.g. reorganization of research councils, conditional financing of Dutch universities). A third aspect is the interest of the institutions themselves in survival: the "secretariat interest" in science policy programmes (Thomas Sjöderqvist's term), for instance, and in general structural inertia: what has been created at one time to meet the circumstances, continues on with only partial adaptation (cf. Stinchcombe quoted by Scott 1983).
4. Examples of the separation between scientific assessment and advice (or management) occur also in the area of risk, e.g., in the proposal of the US National Academy of Sciences to distinguish between Risk Assessment and Risk Management, and the enthusiastic reception of this proposal (Rip 1986c). A useful critique of the ideological character of this and other comparable distinctions is given by Jasanoff (1987); all the same, the distinction may become a rule of the game. Examples of pragmatic resolution of the tension are described in Rip and Gereonewegen (forthcoming).

5. The ideology of rational and useful science also contains the stipulation that scientists should be left free, like geese laying golden eggs, to roam in the meadows of science. In 1862, Helmholtz phrases this argument as follows:

"The scientists -- for the benefit of the entire nation and almost always at its request and expense -- are seeking to multiply the knowledge which can serve the increase of industry, wealth, and the beauty of life, the improvement of the political organization and the moral development of the individual. Yet, no immediate utility must be looked for, as is so often done by the uninformed. Everything that informs us about the natural forces of the human spirit is valuable and in time may prove useful, normally in a place where one had least expected it." (quoted after Van den Daele 1978, pp. 31-32)

The argument continues to be put forward (it is enshrined in Vannevar Bush's report Science, The Endless Frontier) and is now related to the nature of science in general. During the 19th century, its emergence and specific details depend on national circumstances; compare for instance Daniels 1967 (and his description of "professional schizophrenia" of scientists: "Utility is not to be a test of scientific work, but all knowledge will ultimately prove useful.")

The notion of a professional transformation of science during the 19th century is already contained in Mendelsohn's seminal paper of 1964. He focuses on institutional innovations primarily, and the joint socio-cognitive development is only recently being made explicit. An example (with the interesting twist that a hermeneutics of natural sciences may be helpful) is the following quote from Markus (1987, pp. 42-43):

"The concept of an endless scientific progress [...] was still [early 19th century] bound together with an equally firm belief in a definite (achieved or soon achievable) "scientific world view", whose principles were beyond any reasonable doubt and provided the guarantee for an extensive growth of knowledge. [...] It is only from the late nineteenth early twentieth century onward that the conception of an endless growth of knowledge in science has become interwoven with that of a principled fallibilism. [...] At about the same time the literary objectivations of the
natural sciences, and the generic conventions and rules concerning their appropriate constitution and literary use also acquired their contemporary, modern form. In particular, the presently known rules of referencing, together with the specific "short-term historical memory" of the natural sciences described above, have been slowly established from the second half of the nineteenth century. Historians of science recently began to speak with growing frequency about a "second scientific revolution" which occurred during the nineteenth century – meaning either some radical changes in the theoretical orientation and methodological standards of science (first of all physics), or a fundamental transformation in the forms of social organization of scientific activities in general. A hermeneutical analysis of the natural sciences suggests that these two types of transformation were interconnected and integrated with each other through a series of simultaneously occurring changes not merely in their literary practice, but also more broadly in the set of cultural (ATR) relations which sustain this practice. [ATR = patterns of Author-Text-Reader relationships]

6. Campbell has called for an Epistemologically Relevant Internalistic Sociology of Science, ERSS, and organized workshops under that title in the early 1980s. See for an example of his argument Campbell (1979), and comments in Rip (1987b). I am not resurrecting an artificial contrast between 'internal' and 'external', but using this distinction to point out that so-called external considerations will influence decisions of scientists, and that this does not make such decisions irrational or less valid. In the hermeneutic approach advocated by Markus (1987), the same point can be made: "[...] the very distinction between "external" and "internal" factors [should not be considered invalid], because it belongs to the cultural organization of the contemporary natural scientific practice that such a distinction – again in a tentative and negotiated, but not-arbitrary way – ought to be made. In general, a "prudential" fulfillment of the "internal" requirements of scientific activity in the on-going process of cultural interaction between the members of scientific community is a condition of the meaningfulness of these activities, given the present constitution of natural scientific practice." (Markus 1987, p. 44)

The further step, and the one which I am preparing in this paper, is to recognize that the "present constitution of natural scientific practice" is changing, and that attempts to maintain or recreate "meaningfulness" will always be partly determined by structures of the practices, the institutions of science and their contexts.
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