ARTICLES

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Reflections on Research Evaluation

Over the years I have been involved in a number of the evaluations. This will form the basis of some more general observations on the role of research evaluation and its possible effects on research practices, as well as shifts in dominant science policy doctrines. Such doctrines constitute articulations of broad perspectives on the development of science in the eyes of significant actors in society. In the realm of research evaluation they are relevant as contextual contingencies to be taken into consideration. In the latter respect, therefore, an attempt will be made also to attach to recent epistemological and sociological discussion in the multifaceted field of science studies.

Altogether five different episodes will be recounted here, and reference made to a sixth. All of them have to do with evaluation in one or another form.

1) an evaluation of evaluation methodologies for the Swedish Agency for Research Cooperation with Developing Countries (SAREC);
2) an evaluation of a Unesco project – International Comparative Study on the Organization and Performance of Research Units (ICSOPRU);
3) evaluation in the context of various foresight exercises at the Science Council of Canada;
4) participation in an international panel organized by the Swiss Science Council (SSC) to review opportunities and bottlenecks in four disciplines in the social sciences in that country;
5) participation in an international panel to evaluate the Scientific Committee on Antarctic Research (SCAR);
6) an evaluation of an undergraduate course "personal development" at Örebro College after controversy regarding the course's quality and legitimacy in an academic curriculum.

The sixth case is briefly touched upon here because it alerts us to the boundary management role evaluations sometimes play, both internally to articulate boundaries between scientific fields, and externally to draw up certified boundaries between science and non-scientific as well as pseudo-scientific knowledge claims. It also reveals how evaluation of academic performance may sometimes be called upon to define and identify pseudo-science, provide empirical and analytical grounds to relegate it outside the

Science Studies, Vol. 8 (1995), No. 1, 5—23.

walls of academy and possibly excommunicate its most unrelenting advocates.

The Importance of Context

My first two cases really concern evaluations of the evaluation game. They are included because they throw some light on the importance of the broader context in which evaluation exercises are generated and from which they indirectly derive their authority together with some general conceptual framework or perspective that lurks behind the detail of nitty gritty techniques. The evaluation for SAREC resulted in a critical assessment of techniques recommended in the handbook literature on development aid projects whereby Western countries transfer expertise and technologies to Third world countries.

The evaluation for Unesco constituted a critical review of a project that had been going on for seventeen years.

The third case relates to some work done as a Science Adviser at the Science Council of Canada, which was an arms length from government agency with a staff of 65 persons, whereof 23 researchers, whose task it was to provide the federal government with advice on science and technology policy. Here the particular effort consisted in the setting up of a unit for research foresight in 1984 to "pick the winners" as popular terminology rather jokingly called it at that time. Research foresight involves assessing strengths and weaknesses of scientific communities and institutions, diagnosing structural bottle necks and articulating threats and opportunities when it comes to mobilizing newly emerging scientific and technological fields for commercial gain in a global market place, or for the benefit of a wider population when it comes to health, welfare, transportation, and the creation of new jobs. "Threat" in this context means a lost opportunity. Here again research evaluation is ultimately colored by changing political agendas.

The fourth and fifth cases are more easily recognizable as research evaluations in the restricted sense. Both of them were diagnostic evaluations. This means that they were launched to diagnose social, managerial and cognitive conditions of science. In Switzerland it was a matter of four social science disciplines at both French and German speaking universities. With SCAR it was a matter of a regular cycle of audits of scientific unions and specialized committees under the umbrella of the International Council of Scientific Unions (ICSU) and initiated by this illustrious scientific non-governmental organization. Whereas the Swiss evaluation was initiated by scientific communities via the the SSC, the SCAR-evaluation flowed from decision-making at a high level within the realm of international scientific administration. This difference in the degree of participation of the scientific communities concerned proved to be highly significant.

Evaluating the evaluation game for SAREC

The point of departure in the SAREC-report was the expressed need in 1979 of "a novel, theoretically ambitious approach to tackle not only the problems of project evaluation but also the question of design of evaluation procedures and conceptual frameworks. For a start, such an approach will require analysis of various 'evaluation models' propagated, and their interrelations with underlying theories, ideologies and policies of development". (K-E. Knutsson, cited in Elzinga, 1981; also SAREC, 1981: 9)

The usual distinctions were made between *ex ante* and *ex post* evaluations, as well as different units of analysis – project, program, institution. It was found that cost-benefit analysis weighs possible losses against potential 'benefits'. The entry of pollution and other environmental aspects opens the scope of the analysis of a wider range of qualitative parameters having to do with the impact of projects and programs. This is the case today also with research in laboratories, where animal ethical commit-

tees set limits to what methodologies are permissible. In research on the Antarctic continent the last couple of years, environmental impact statements have become a normal procedure in *ex ante* project evaluations prior to funding.

The report further notes how evaluations are made from a variety of different standpoints and viewpoints. In the case of officially authorized evaluation, it is usually someone who is paid to obtain information that will serve as a basis for decision-making. If government funds are involved the evaluation may be by a civil servant or someone commissioned as such. There are many variations. The important point is that the evaluation will most often reflect the standpoint and perspective of those commissioning it. In a situation of severe opposition, criticism or legitimacy crisis concerning previously determined priorities and policies, such an evaluation may even serve the purpose of redefining policies and priorities in order to legitimate a new course of action. Today we see this motive tacitly present in the attempts to introduce new institutional arrangements into academy, to foster stronger interaction between the worlds of university and industry, to promote a clearer internationalization of research agendas, and to profile specific fields as strategically important.

John Irvine and Ben Martin in their book *Foresight in Science: Picking the Winners* (1984) define strategic research as follows: *basic research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognized current or future practical problems.*

It is clear that evaluation is not a question of a socially neutral exercise for assessing the value and efficiency of a given project vis a vis predetermined goals or objectives. It is much more than a technique or method, even if that is what one most frequently associates with the term when used in a scientific context. Evaluation is a social activity that is often linked to a social process of legitimation and rationalization on the part of the entrepreneurs and agencies who have to account – either to shareholders, governments or the public – for funds invested.

Pared down to the bare bones, evaluation in development research projects is found to display basic stakeholder commitments. Thus the evaluation handbooks and manuals of the World Bank were found to be built up around certain ideological premises, and thereby their application in practice was found to support and legitimate aid to certain kinds of economic systems while disavowing others. When applied in a social setting whose value system coincides with the ideological premises there is no clash and hence the evaluation technique appears to be objective and neutral. That this is not the case becomes evident first when an attempt is made to apply the evaluation technique in a social setting with a value system contradicting its premises, or geared to entirely different institutional motives for research and development.

Applying the concept of paradigm to development theory, three different perspectives become visible, each with. its own type of associated evaluation format: first a neoclassical paradigm (GNP-growth; and the modified version 'redistribution with growth', to which we today might add the notion 'sustainable development' that grafts on an ecological dimension); secondly dependentia theory, developed in Latin America, contributing a reorientation from a more or less descriptive approach towards a structural-historical one linking development/underdevelopment as two interdependent processes; and finally the "small is beautiful" alternative that derives inspiration from populist and neo-populist traditions and broad social movements. The latter has been articulated in the self-reliance paradigm, with names like Gandhi and Nyerere. Each of these three paradigms in development theory reflects commitments and partisan values of a social sort, and each has its own preferences as to evaluation formats. Handbooks designed to guide evaluation of results and effects of development aid (and R&D as a component), were found to be largely steeped in the neo-classical framework. This means

Table 1. Evaluation formats in development studies.

	Neoclassical paradigm	**Dependentia paradigm**	**Self-reliance paradigm**
theory of development	Neo-classical GNP-growth theories	Center-perifery theory of underdevelopment	Theory of comprehensive national-economic cultural deveplopment involving indigenous potentialities
associated evaluation "format"	CBA's economic criteria at rhe micro-level	Global economic criteria at the macro-level	Combination of macro- and micro-level economic, social cultural and political criteria

that there is a systematic eurocentric and ideological bias which tends to be obscured by the incorporation of a quasi-scientific universalism and quantification in evaluation discourses on research for development in many lead agencies in the West.

A main point that emerges here is the importance of clarifying the context, articulating various stakeholder interests, and delineating the perspective that basically informs the evaluation format. This is especially important in the case of research evaluation as a component part of the evaluation of aid to developing countries, since otherwise internal criteria of quality in science may become blurred with external criteria of relevance and accountability.

The ICSOPRU-file

Science policy is a relatively more controversial subject than science *per se,* and here the process of dilution to a common denominator of generalities built into the intergovernmental structure often does its job. Unesco's Unit for Science and Technology Policy has been no exception. Even surveys and studies where the science policy unit orchestrated sociologists and other professionals on contract tend to end up being rather uncritical. This may be seen in the International Comparative Study on the Organization and Performance of Research Units (ICSOPRU), a program initiated in 1971 to assist in improving the management of R&D in countries that elected to participate.

In its initial phase this important innovation helped disseminate experience in making inventories of scientific performance for purposes of management and planning. Various parameters like the size of research groups, funding and leadership functions were taken to be central to good research performance. However, as time went on, new insights were gained with respect the contextual nature of socio-cultural preconditions for scientific knowledge production. These emerged internationally in the new sociology of science, but they were not taken into account by the ICSOPRU project. Rather it continued on in its original rather positivistic mode inspired by systems theory and a scientistic view of the interface between science and society.

ICSOPRU involved data collection on research groups in different institutional settings in various countries on the basis of a set of detailed questionnaires. Between 1973 and 1986, seventeen countries were reviewed in four successive rounds, with seven countries in the first round, six in the sec-

ond, five in the third and four in a fourth round. From the outset the program assumed a positivistic decontextualized approach to studying research groups. Therewith the object of comparing performance indicators across national and cultural boundaries was fundamentally faulted; significant differences in externalist factors, and changes in these over time in any given country were essentially ignored.

At the outset it was mainly Western European countries and two Eastern European ones that participated. (Much of the credit for bringing on board the first round of countries belongs to the then Director of the Science Policy unit, Y. de Hemptinne, who had a wide net of personal contacts in this field.) In the second round it was only Eastern European and some Third world countries. By the third and fourth rounds Third world countries predominated. By 1986, furthermore, interest and the main legitimation of the exercise had also shifted from R&D managerial aspects and policy arguments to the third world countries' desire to obtain computer software and learn computerized aided survey techniques.(Elzinga & Jamison, 1987/88)

An evaluation made in 1988 concluded that, "in its nearly 20 years of operation it [ICSOPRU] has provided little, if any, input to policy-making, while reproducing frameworks of social analysis that fail to address the special problems of developing countries". (Baark, Cabral & Jamison, 1988) The gradual shift in the profile of countries participating in the ICSOPRU exercise is significant in that it reflects the more general tendency in post-1954 Unesco; Western industrialized countries were becoming less interested while Eastern Europeans as well as Third world countries were the ones that found most practical use for their affiliation.

The evaluation of ICSOPRU was called for by the Director of Unesco's Science and Technology Policy Program who had inherited this project from his predecessor and wanted to terminate it. This was in response to mounting criticism regarding increasing costs at a time when Unesco's budget was contracting, as both the UK and USA had withdrawn from this UN agency in protest against its alleged politicization under Amadou Mahtar M'Bow from Senegal (appointed as Director General in 1974). M'Bow's overt ideological profile reflected the increasing influence of Third world countries within Unesco. The departure of the UK and USA from the Organization was an attempt to cripple it and fall in line with Western interests. The effect of the evaluation in question was that the project was first shifted to another section of Unesco and later it ended.

EST at the Science Council of Canada

The Canadian experience is one of research evaluation inserted in a broader operational context. It went on about two years before it was terminated due to budgetary constrictions following upon a political regime shift at the level of federal government, from the liberal Trudeau to the conservative Mulroney. Now the latter has also been swept away in 1993 elections, which decimated conservative party representation on Parliament Hill in Ottawa from a majority to a few seats; now the conservatives do not even qualify for the state support enjoyed by larger parties; sometimes the will of the electorate is able to make itself heard, but only after much damage has been done.

The Emerging Science and Technologies (EST) operation as it was called used a matrix method to match new and emerging scientific and technological fields with potential social impact and utilization. (Elzinga, 1987) The matrix we developed in a large computer data base with information coming from nation-wide surveys with questionnaires, select telephone interviews with top specialists in various ESTs, specialized workshops and panels of experts. The operation succeeded among other in diagnosing Canada's lagging position in new materials research and recommended a number of measures to rectify this; some of these were since implemented. Broad surveys of other areas were also made, including micro-

electronics, biotechnology, aquaculture, forestry technologies, food irradiation and transport systems. In each case an assessment was made of the scientific competence and engineering skills existing in the country in various forms, structural and funding bottle necks there were, problems of attitudes and mentality, and how deficiencies might be overcome.

The interesting thing that came out of all this, I think,was how:

1) the foresight process was often just as if not pore important than the accuracy of the forecast; once initiated the deliberations involving many different actors, scientific and nonscientific, spurred a consensus building process that made it easier to effect various changes; it also helped focus the different actors' vision and harmonize their views as to priorities and approaches in relevant areas of science. Funding patterns, new institutional arrangements and relationships at the university-industry as well as university-government interface were in part made the subject of renegotiation.
2) the structure of the exercise, at what level of authority it was anchored within scientific communities and in governmental decision-making machinery as well as industry, was clearly important for the success or failure of the outcome, including subsequent efforts to effect change. Legitimacy and participation were two important elements in the whole process.
3) not only did the composition of panels and workshops influence the thrust and contents of the assessments, but the whole exercise too was dependent on political good will. This became evident from how easily a new government that did not see any need for research foresight but believed that the "picking of winners" would be taken care of by the market place, how this new government was able to ignore and proceed indirectly to dismantle the whole enterprise. The budget and number of personnel of the Science Council was reduced to one half, and some years ago the Council itself was shut down. This again tells us something of the latent political nature of evaluation, even if it is mostly limited to the level of micropolitics in rivalry between the advocates of different paradigms, disciplines, and large research establishments. In the foregoing it was intimately linked to national politics.

A Swiss Model

At the invitation of the Swiss Science Council (SSC) a panel of ten experts from different countries were invited to review the strengths and weaknesses of social scientific research in four disciplines at the Swiss universities: educational science, political science, psychology, and sociology. The model developed in this case has already attracted attention some in other countries facing related problems. The panel was asked to identify opportunities for developing existing research potential, as well as determine constraints and bottlenecks. The procedure was as follows:

First the SSC set up a Working Group on social sciences (Committee SOWI) consisting of (a) representatives of the four scientific societies concerned, as well as all parts of the Swiss Academy of Humanities and Social Sciences; (b) representatives of the SSC and its permanent staff. In a first phase the development and existing structure of research in the social sciences as well as the conditions in which this research is carried out (e.g., in the context of international research) were described and put into a number of reports based on deliberations within the scientific societies; in addition there were some commissioned surveys, including science-citation reviews for each of the disciplines. The first phase then was essentially one of self-evaluation by representatives of the four scientific associations concerned and the formulation of corresponding measures aimed at strengthening research in these disciplines. Its results in the form of six basic reports were the input

to the international panel, who also read a wealth of other material pertaining to the structure of Swiss higher education and research, science policy, data from the Swiss National Science Foundation (FNRS), etc. The complete list of preliminary reports counted nine items written specifically for the exercise and three or four more general items.

The following is excerpted from the editor's preface of the final report produced by the evaluation panel:

"The SOWI reports were sent out to all research institutes participating in the evaluation exercise in the end of May 1992. The members of the International Panel of Experts received the reports together with other relevant documents in June 1992. The panel members convened in Berne at two occasions, September 28 – October 2, and November 1920, 1992. During their first series of meetings in Switzerland the international experts made on-site visits and had discussions and hearings with representatives of the research institutes and with other representatives of the scientific communities concerned, as well as with representatives of the science policy institutions and with users of social science research results. A draft report was written by the panel members during this week. The report was finalized during the following weeks by the experts and approved by the International Panel as a whole. The document was then submitted to the representatives of the research institutes and the scientific communities concerned. These representatives were invited to discuss the findings of the international evaluation together with the members of the International Panel of experts on the occasion of the Concluding Conference November 20, 1992". (Swiss Science Council, 1993)

The exercise was quick, thorough and effective. For the on-site visits the panel met together that all concerned and then divided into small groups to pursue deliberations in parallel, singly with representatives of the different disciplines, whereafter everyone met again in plenum for further ventilation of problems that were identified. Altogether 216 individuals participated in these meetings, two thirds of them teachers and researchers located at 65 different institutions within fourteen different universities, higher educational institutes and research facilities. The remaining two thirds of this population were from the SSC, policy making agencies, sectoral agencies, the Research Council, the Humanities and Social Sciences Academy, and some ministries.

The report of the evaluation panel highlighted several structural problems emerging from the gap between teaching and research policy, the lack of coherent and university-level strategies for the development of social science research, the lack of critical mass of researchers, a problem compounded by linguistic fragmentation of Swiss social science, and a lack of career opportunities inside and outside academy, as well as funding difficulties, lack of cooperation across institutions and disciplines, and the underdevelopment of research managerial capabilities. The Swiss university system was found to be at a stage of development reminiscent of the oligarchic professorial chair system in Sweden thirty five years ago. The humanities still appear to have a stronger position than the social sciences. Consequently the craftsman mode of knowledge production with its individualism and non-modern mentality dominated. The question was how the university system might be moved in the direction of some model of a modern research university.

Three major institutional innovations were suggested, and a number of less dramatic changes. The major points were the formation of a National Colloquium for Strategic Social Science to develop a Schwerpunktprogramme in social science, the development of two Graduate Schools (one for the French on for the German speaking region) for the training of doctoral students, and the creation of a Swiss Institute for Advanced Study in the Social Sciences.

The report was well received and enjoyed considerable press coverage. It had a direct impact on the budgetary process in the responsible ministries of federal government,

and follow ups continue to this day. A National Colloquium has succeeded in defining a strategic research program that seems to have a good chance of funding, and work on the graduate schools is evolving.

I think much of the success, and the ability to use the evaluation as a lever for political action and reform followed from the high level of participation of the relevant social science communities, the SSC's ability to manage such a broad exercise, and give it high level visibility and anchorage. Also important were the tempo maintained, and the chemical mix of the panel members in terms of disciplinary composition and personalities. The geographical spread of recruitment of the panel was also a positive aspect since it brought together a wide range of experience and insights into the dynamics of science. There were two persons from each of the four disciplines and two science studies scholars to specifically focus on science dynamics, research planning and science policy aspects.

Reviewing Antarctic Science

In 1991 the General Committee of ICSU listed SCAR as one of the bodies to be reviewed. For this purpose a panel of experts was selected from the scientific community at large was appointed with Rita R. Colwell as Chairperson.(Colwell, 1993) The evaluation of the activities of the Scientific Committee on Antarctic research (SCAR) was consequently carried out by an international panel of five experts, four of them from various disciplines in the hard sciences and one with a knowledge of the policy dimension.(Elzinga & Johnsson, 1993) The panel was asked to review Antarctic Research, including the goals of SCAR, the quality of its work, financial support, and success of SCAR in the formation of networks, and activities of networks for interdisciplinary international research. The report of the review was presented at the 30th meeting of the ICSU General Committee in Jerusalem 1992, at which time its conclusions and recommendations were approved. It recognized that SCAR should conduct a detailed appraisal of its role and structure in the light of the points made in the review report.

SCAR is essentially a perpetuation of the research coordinating function that was introduced for Antarctic Science during the International Geophysical Year (1957/58). This origin was found to have left an imprint on the functioning of SCAR, among other in its character as an old boy network. It was suggested that SCAR should carefully sort out its different tasks and give priority to science, without for that matter discarding its role in respect of science policy advice to the regime of countries that make up the Antarctic Treaty Organization. SCAR scientists are seeking to break the traditional isolation of Antarctic science from international research, and there have been efforts to integrate more closely with global programs in other fields, especially the global climate program to interact more with other ICSU bodies. The reorganization of working groups and specialist groups in response to environmental concerns and commitments to international efforts such as the global change program has been carried out and more attention has to be paid to the type of research needed in the Antarctic. The secretariat should be strengthened and a strategy for fund raising developed, for example through the possible institution of an Antarctic Science Foundation. Among the many problems that must be addressed are: the complexity of the organization since many new countries have joined; the role of Third world scientists, both in participating in research and in training programs for their benefit; and the possible development of a relationship with IASC (International Committee on Arctic Science). Certain questions were raised about the interplay between internal, peer review and quality control criteria and external relevance criteria. The increasing pressure for relevance coming from environmentalists' concerns is having an effect on Antarctic science and the working conditions of SCAR. SCAR's strategy is to take more account of

strategic research, but at the same time maintaining a solid disciplinary scientific basis. This requires a conscious management of the interplay of external and internal criteria.

The evaluation contains many more points, but only some of the more interesting ones for our purposes here have been highlighted. It will be clear that the main thrust of the evaluation was to recommend certain organizational changes to strengthen SCAR's function as a research coordinating body at the international level, and to give greater prominence to peer review processes.

The response to the evaluation has been mixed. Within ICSU it was positively received, and likewise by some of the leading scientists concerned. From SCAR's leadership on the other hand there have been some sour comments to the effect that the panel manifested a besserwisser attitude and the panel's Chairperson, a well reknowned and highly productive scientist in her own right, and President Elect of the American Association for Advancement of Science, was petulantly referred to as a "lawyer". This is in fact a confusion, probably reflecting irritation over the hybrid role prof. Colwell has come to assume. A more immediate reason might be that the SCAR leadership was not asked to look at the report before it went to ICSU.

Again we learn here the importance of anchoring an evaluation in the scientific communities concerned. The SSC and the SCAR evaluations differ radically in this respect. The SSC evaluation was based on documentation generated from within the scientific communities, while the SCAR review was only based on a limited number of formal documents, individual contacts, plus a wealth of inside information and experience (tacit knowledge) that panel members had already accumulated over many years. The SSC evaluation report was used for a direct dialogue with representatives of the scientific communities concerned, while the SCAR evaluation was mainly for in house consumption inside ICSU. On the other hand there was a similarity in that neither of the evaluations made direct assessments of the quality of any branch of research; the focus was on the social dynamics, institutional and policy levels, with the odd comment on cultural dimensions or mentalities.

Managing the boundaries of the scientific – effects of evaluation

The Örebro evaluation concerns undergraduate education. (Elzinga, 1990) Since the constitution of Swedish universities stipulates that all university education must be based on science, the question arose if a course using ample literature from the New Age movement satisfies this rule or not. The evaluation in this case concerned the boundary between science, non-science and pseudoscience (i.e., non-science which is promoted in would-be-scientific dress). The approach I took was to distinguish three levels: an epistemic, a sociological and a moral one.

At the first level falsificationism was used as a criterion for scientific content, in the sense that courses that fail to promote critical thinking but foster dogmatic belief in esoteric claims without argumentation can be taken to fall outside the norm of science. At the second level the existence of a peer review process or collegial academic gatekeeping system to which the knowledge claims in the textbooks have been subjected is a prior requirement for scientific credence. At the third level it is a question of the "ethos" or ideology associated with the promotion of the knowledge claims, to what extent it is recognizable as "scientific" in some sense, or if it belongs to some other kind of value system. The Mertonian formulation of four norms were taken as an ideal typical reference point for a scientific ethos, even if we know that in practice scientists are apt to deviate considerably from them. The significance here was, not research practices but the rationale that lay behind advocacy of certain knowledge claims in textbook literature.

The knowledge base of the undergraduate course in question was found to be deficient on all three counts.

The effect of the evaluation was the immediate termination of the course and the teacher in question was not allowed to use the university's name to advertise his courses which now continue outside the academy, with considerable commercial success. It was apparent that one of the advantages of having the course in the university system was the stamp of quality this lent the product for purposes of advertisement. At the same time this detracted from the image of the College and its quest as a serious institution to obtain university status in the Swedish academic landscape.

More generally the effect of evaluation on academic life has several different dimensions which should be considered.

One is the *immediate effect* in terms of changes in funding patterns, institutional structures, research strategies or intensity of publication at the research *performer level.*

Another are the *changes* that may arise in decisions within *research councils or policy making circles* at various levels. These may not be seen immediately owing to inertia in the system.

A third type of effect has to do with the *management of boundaries* between science and society as well as boundaries between disciplines and specialties.

Finally there are effects that may appear in the *culture of research practices* and *reputational structures.* These latter two types of effects deserve some further discussion.

The effect of reaffirming existing cognitive boundaries or admitting of new ones arises as soon as the mandate of an evaluation committee is decided and its composition determined. Should an evaluation of psychology include research done in biology laboratories, or should it restrict itself to what is done in institutions found within humanities and social sciences faculties? What appears as linguistics at one site may well be called literature at another. Therefore, to what extent is an evaluation of say research in Germanistik to include post-modernistic studies of German literature? The list of problems is extensive. In each case an evaluation committee will have instructions to include certain things and leave out others as falling outside its bounds. Therewith disciplinary and faculty boundaries are affirmed or questioned. The resulting evaluation too will contribute to entrenchment or reconsideration of existing cognitive boundaries.

Through the pages of international evaluations at NFR in Sweden

Finally, let us look at some evaluations carried out under the auspices of the Swedish Natural Sciences Research Council (NFR).

An international panel on condensed matter physics recommends support to leading surface scientists, stating that "non-linear optics is a rapidly growing activity with major ramifications in fields such as communication technology and micro-electronics". They go on to emphasize the importance of making linear optics a part of condensed matter research. (Swedish Natural Sciences Research Council, 1986: 8) And: "we strongly recommend that a national committee be created to..../among other/.. create a small unit as evangelists for neutron scattering research; ...we recommend that a personal professorship is created for Dr...."; (Swedish Natural Sciences Research Council, 1986: 11) or "Lundqvist has had an effect on condensed matter physics that is so positive that it is impossible to quantify". (Swedish Natural Sciences Research Council, 1986: 45)

Another panel, dealing with historical geology and paleontology states that "in all three areas of research, evolution, biostratigraphy and paleoenvironments, there has been an expansion of activities in recent years"; and "....this country's geoscientists should feel welcome to participate in these EC activities as widely as possible"; or "...the physical separation of the department apparently promotes an isolation that at least in part accounts for the limited contact with other earth scientists"; further regarding a

senior researcher, "his approach of treating paleontological and biological data with advanced mathematical and statistical methods, represents a particularly original contribution to this field of science". (Swedish Natural Sciences Research Council, 1989: 5, 9, 27.)

It does not require much imagination to appreciate that such statements can be used by researchers in the field to promote their own specialties in rivalry with the representatives of other specialties that may not seem as modern or in line with the "international research front". It is with the help of such evaluations too that the international research front is socially constructed.

A panel on super-conductivity notes regarding a team focused on certain nuclear techniques that, "despite the recognized expertise of this group, its efforts to apply muon spectroscopy to HTS-materials is noncompetitive in the light of intense international competition in this area". And regarding another group, that it may be advisable for them "to find their own niche based on their expertise and in the same time to track closely the development in the field"; or cautioning a third group with, "the proposed shift to the study of so called 'hot' phenomena involving the low energy scale response functions with Raman infrared is admirable but ambitious... Competition will be strong". (Swedish Natural Sciences Research Council, 1991: 12, 27, 31)

Relating to ethology and behavioral ecology another panel notes how one researcher, being a good scientist, is attempting to shift the focus of her studies to behavioral endocrinology. "She should be encouraged to orientate her work towards the biomedical area and to seek collaboration and funding there, perhaps after some additional training". (Swedish Natural Sciences Research Council, 1991: 43) And in the field of genetics an evaluating panel remarks: "Several principal investigators submitted extensive bibliographies to the committee. These included substantial publications in leading journals interspersed with many minor articles in more obscure journals. While the results of all research projects should be published, many of these minor publications are of little value and do nothing to enhance the reputations of the scientists concerned". (Swedish Natural Sciences Research Council, 1992: 4) The effect of such a signal should be obvious – forget the grey literature, go international!

The committee also had a comment on the evaluation procedure: "A major concern is the possibly biased selection of projects placed in the genetics category. The committee had the impression that work in areas like development genetics, medical genetics and agricultural genetics was underrepresented because it either falls outside the NFR mandate (medical genetics or agricultural research) or because it is placed in other categories" (e.g., developmental genetics allocated to the category of molecular biology). (Swedish Natural Sciences Research Council, 1992: 4–5) This confirms what I already noted earlier about the boundary-drawing effect.

Further, regarding a specific specialty, the panel points the finger to say that, "it is unfortunate that molecular genetics is physically separate from population genetics at the University of Lund. The committee feels that Prof. Bengtsson would benefit from closer interactions with his colleagues". (Swedish Natural Sciences Research Council, 1992: 10) In Stockholm the panel finds that "Dr. Ryman is a major international scientist in the merging area of conservation genetics", (Swedish Natural Sciences Research Council, 1992: 23) while in Uppsala it debated whether an Agricultural College project "is sufficiently basic in scientific content to justify NFR support". (Swedish Natural Sciences Research Council, 1992: 31) A panel on soil ecology earlier, invited to evaluate this field by the Swedish Council for Forestry and Agricultural Research (SJFR), on the other hand recommended that a project at the same College be reoriented from a pest control to a more broadly environmental perspective, and that "one of the investigators try to obtain postdoctoral experience in a department where population biology (not nec-

essarily of nematodes) is strong". (Swedish Council for Forestry and Agricultural Research, 1987: 27)

A panel evaluating certain research relating to the lithosphere recently observed how one scientists at the Geology and Geochemistry Department at Stockholm was "becoming too narrowly focused. We therefore recommend that she consider widening her research field. Collaboration with researchers using different laboratory facilities could be worthwhile". (Swedish Natural Sciences Research Council, 1993: 16) Regarding a scientist at the Geology Department at Lund, the panel rates his "present research as good and notes that his research and productivity are on an improving trend. We recommend that he focus his research on the most important topics /as defined by the panel/. This would also involve a focus of his analytical work on specific tectonic and magmatic problems". (Swedish Natural Sciences Research Council, 1993: 30)

Here again we see the gatekeeping function at work, signaling the need for reorientations which would steer research efforts in a scientific community into the direction of the latest developments at an international research front and therewith providing legitimation for changes in the ecology of disciplines. An overtly policy oriented comment is also provided in the report: "The committee perceived a general need for greater mobility of scientists between institutions in Sweden, and to and from foreign institutions. We believe that more mobility would lead to a cross-fertilization of concepts among researchers, to more versatile use of all kinds of analytical techniques available in the geological community at large". (Swedish Natural Sciences Research Council, 1993: 8) This is fully in tune with some of the current themes one finds in science policy documents in most countries in the West today: the need for of mobility, internationalisation, globalisation and developing profiles around strategic areas of research. internationalisation and globalisation have become catch words, and research evaluations are suitable vehicles for operationalizing them.

Epistemic drift and the maintenance of reputational structures

Another theme, more implicit, is the need to strengthen the reputational system associated with the disciplines. This follows from the tendency to weaken academic structures in the course of strategic interdisciplinary profiling efforts and problem-orientation with an eye on future applications. In the heyday of sectoral science policy in Sweden the Natural Sciences Research Council there noted how in some fields "a scientist is put into a situation where he must provide relatively quick answers to complicated questions. For example, the questions of various metals in soil and water that would affect the microbes of soil and water; the project must be finished within a relatively short time, e.g., two years, because the /environmental/ law must be written. The next step is to hire young scientists who have just received their Ph.D. degrees and to tell them to get to work. They have no experience and no opportunity to receive advice from older and more experienced scientists. Their superiors cannot make scientific judgments. After the short project is over they are moved to another problem. After several years they are completely out of touch with current ideas and techniques of the field they were trained in. They have stopped publishing in reviewed scientific journals and are isolated at scientific meetings because their data is of limited interest (and of increasingly poorer scientific quality). After 5 years they cannot longer compete for other jobs". (Swedish Natural Sciences Research Council, 1981: 15)

I have quoted at some length because this passage gives a good description of the formation of hybrid research communities and their problems when these get locked into the operational activities of practitioner oriented agencies or enterprises. A new reputational system develops, outside the academic one, with a different set of literatures, norms, and career patterns. At a structural level when the external relevance criteria begin to dominate over the internal scien-

tific quality control criteria one gets the phenomenon I have referred to as "epistemic drift". (Elzinga, 1985: 191–220) Research evaluations initiated by research Councils or from within the academic system itself can provide an antidote to such tendencies. On the other hand evaluations initiated by other actors may go some way in reinforcing epistemic drift and the development of hybrid reputational and knowledge managing systems.

This means that evaluations may also be seen as fora or platforms where different stakeholder interests negotiate the thrust that should be given to research agendas. This may not only affect the direction but also the cognitive contents and preferred methodologies and conceptual apparatus of research in given fields, especially in the social sciences and humanities which are heavily overlayered by ideologies. This type of function we saw in the case of the EST-effort in Canada. The stakeholder interests that meet in such evaluation exercises may be rooted in different value systems and policy cultures. If one policy culture has a hegemonic position the socialization effect of the evaluation will tend to be favorable to its perpetuation. At the same time there is the danger that the pressure of current fashion in that culture will induce a kind of tunnel vision, so that alternative lines of development, or aspects, are not seen or mentioned. They fall within a blind spot.

Consequently it is important in evaluations to clarify the boundaries and interests entrenched in the different policy cultures concerned.

Policy cultures

At work are what might be thought of as four main "policy cultures", coexisting within each country, competing for resources and influence, and seeking to steer science and technology in particular directions. We may call them the academic, the bureaucratic, the economic and the civic policy cultures. These cultures, which stand out as representatives of the dominant voices in the literature, have different perspectives on the role of science, its preferred linkages with society, as well as different political and social interests and they draw on different institutional bases and traditions for their basic positions. Each policy culture has its own perceptions of policy, including doctrinal assumptions, images and ideals of science, and relationships with the holders of political and economic power. One might also say that they form the main constituencies wherethrough science and technology policy is legitimated. In any given case a policy framework will be the outcome of mutual conflict and accommodation between policy cultures, with the dominant actors leaving the most indelible mark. The mandate of an evaluation panel will be influenced by such filiations, which in turn will contribute indirectly to the shaping of the preferred evaluation format.

The academic policy culture stems from the university based communities or researchers and their committees in agencies for funding basic science, faculty organizations and professional organizations. The dominant interest here is the maintenance of relative autonomy and the promotion of a good image of science. Quality control combines with overseeing of research agendas. Essentially the policy that is articulated is a *policy for science* as distinct from the perspective of the non academic actors, who may be more interested in *science for policy* in relationship to other realms of endeavour. In the academic policy culture importance is attached to the preservation of traditional values of autonomy, integrity, objectivity, and the scientific communities' control over funding and organization.

The bureaucratic policy culture is concerned primarily with effective administration, coordination, planning, and organization. In many countries it is largely dominated by the military, and based in the stave administration with its many agencies, committees, councils, and advisory bodies. In recent years an array of bodies that are supposed to uphold the institutional motive of environmental protection and natural resource man-

agement have come to play a prominent role. The concern is with regulation, making public policy scientific, and thus science for policy (Jasanoff, 1990; Smith, 1990 ;Salomon, 1973). Research evaluations will emphasize relevance to a particular mission. (see Elzinga, 1988)

This may be contrasted to the economic policy culture related to business and management, based in industrial firms, and focusing its attention on the technological uses of science. At work here is an entrepreneurial spirit or ethos that seeks to transform scientific results into successful innovations to be diffused in the commercial marketplace (Dosi, Freeman, Nelson, Silverberg, & Soete:, 1988; Etzkowitz & Webster:, 1994; Gibbons & Wittrock:, 1985) Oil companies and power producers will be adverse to drastic cutbacks in CO_2 emissions, and therefore try to water down the specially tailored "policy makers summaries" and the executive summaries of, for example, the advisory state of the art in climatological research reports published by the Intergovernmental Panel on Climate Change (IPCC). On the other side there is the civic policy culture, which in its most dynamic form is based in popular social movements, such as environmentalism and feminism, and whose concerns are more with the social consequences and implications of science than with its production and application. The civic culture articulates its positions through public interest organisations as well as through campaigns and movements, and its influence is obviously determined by the relative strength of the civil society in a country's overall political culture (Almond & Verba, 1965; Blume et. al., 1987; Nowotny & Rose, 1979; Eyerman and Jamison, 1993). At the international level the more successful non governmental organisations promoting an environmentalist ethic have during the past decade reorganized to pattern themselves on multinational corporations. Greenpeace is an example; it is an organization that enrolls scientists, contracts research and makes systematic use of media channels to enhance their own visibility, and to disseminate their message. With regard to global warming these organisations continue to press for substantial reductions in CO_2 and other greenhouse gas emissions.

While the dominant non-academic cultures tend to draw science and technology policy into a "technocratic" direction, the civic culture stands for what has been called a "democratic strategy" for S&T policy (Dickson, 1984). The academic policy culture for its part represents the existence of a scientific "lobby", which in many countries has become a political force in its own right.

Shifts of policy doctrine

If we were to provide a name for the latest trend in science and technology policy it would be "globalisation" and "indigenisation". These are key words in the extension of OECD III for the 1990s. Science and technology policy, we are told by OECD spokespersons, "has to be related to both national contexts and global change. Governments must plan their action more carefully in the social and institutional setting of their own countries and simultaneously make better use of science and technology policies to help solve the problems emerging in a rapidly changing world" [Aubert, 1992: 4; see also OECD, 1992).

The first OECD science policy doctrine (OECD I, in the 1960s), then, spelled out science for "GNP growth"; OECD II in the 1970s revolved around the contractor-purchaser or sectorisation principle whereby science for policy came to dominate over policy for science; OECD III in the 1980's meant the introduction of what I like to call "orchestration policy", whereby government defines general frameworks in which various actors, including basic researchers, are asked to play together on behalf of one or another national objective; now we are entering a phase where this doctrine is being developed further with constant references to the "global" (read "national" or "continental"). In this connection a key feature in science policy discourse has become the ques-

tion of targeted or "strategic research". Some of the research evaluation that goes on today should be seen in this light.

Evaluation formats are socially constructed through conflict and negotiation over control involving sets of actors in the different policy cultures. Thus shifts in predominant policy doctrine will affect evaluation procedures. Behind these lurk the different groups or actors who contend over the mode of representation of science. There is a struggle over representation – whose picture is to serve as the base line for evaluations? (Elzinga, 1988; Weingart, 1991)

On the one hand we have the scientific communities' own picture, which is given in terms of disciplines and organization in institutions. This is a qualitative picture, as one and the same specialty may be "located" in institutions with different names across the landscape of universities and research institutes. On the other hand there are the policy makers and external stakeholder interests that want to objectify science in a way that makes it more easily subject to their control. Quantification in terms of numbers, be it in publications, citation counts or cocitation clusters is convenient since it disembodies the enterprise in question from taking account of the complex mosaic of local peculiarities which, because they are impossible to overview, would put the policy makers at a disadvantage and in the hands of the scientists. Bibliometric methods help condense scientific communication processes so that they appear independent of the variation of institutional boundaries and social recognition or reputational systems inside science. It is these latter that fascinate scientists themselves, who will resist externalized quantified descriptors as being off the mark. The argument is that such representations do not give the "correct" picture.

Essentially what we have here is a conflict over control. Who is to control who? This will be determined to some extent by whose picture is taken as point of departure in evaluations. All being social constructions, representation, and thus evaluation, becomes a question of jockeying for position. Sometimes a particular group of scientists will find advantage in bibliometric descriptions since these favour their particular field or paradigm over other contenders. In such a case alliances may be formed between disciplinary stakeholder interests inside and bureaucratic or other policy interests outside science. Therewith more credence is given to the bureaucratic policy representation of science. The outcome of struggle over the definition of relevance contexts is therefore also linked to the acceptance or rejection of the more general representation of science in the predominant policy doctrine.

With the shift to orchestration policy bibliometric indicators have begun to serve as a basis for dialogue and mediation between scientific communities and policy makers. Both sides participate in a learning process with mutual accommodation of perspectives. As with research foresight the process is often more important than the actual outcome in more formal documents. Once these documents exist however they can in turn be used by both sides to argue their standpoint, provided of course there is some interpretative flexibility in the modes of representation used.

The question of strategic research

Observe that "strategic research" is a relatively new category that is sometimes grafted onto the old R&D accountancy system developed by the OECD. It falls between curiosity oriented research and applied science. As with the sector principle, fields of research in this case are no longer defined out of an internalist perspective but rather on the basis of anticipated utility *vis a vis* external social and political goals. The difference is that the definition has been extended beyond the "applied" (the "D" in "R&D") to basic research, which was earlier left outside the compass of external direction. Therewith one has shifted further away from Vannevar Bush's original science-society contract. The "foresight" Martin and Irvine describe in their

book concerns experiences of several countries. It appears that this research "foresight", when it functions, is based on significant research on research, philosophy and sociology of science, the economy and organization of research, together with science policy studies and possible historiography of science under one umbrella.

An important aspect of foresight that is also characteristic for the new science policy is that we have a process the purpose of which is to internalize external political and social objectives into scientific communities at the level of basic research. This process can have a strong steering effect, although this *dirigisme* does not occur as previously, in attempts at rational planning. It now follows a model that rather resembles the Conductor who uses his baton to get a whole orchestra to play in unison, with the possible difference that the notes of the novel piece in this case are not written beforehand.

The transition to the new science policy implies a significant dilution of the mechanical in the earlier technocratic model. One tries now to orchestrate research on the basis of some needs- or demand-oriented and supply policies at one and the same time. The orchestration presupposes that the actors in this drama follow their own devices and take independent basic research initiatives within the general framework that has been determined. In this way too "peer review" and quality control may be incorporated, for example, into investment strategies. Martin Kenney in his book *Biotechnology, The University-Industrial Complex* (1986) has shown how this in fact is a more sophisticated form of steering – the autonomy problem is raised to another level. "Many observers have been concerned about the possibility that corporate funding of research may lead to control of the research agenda", Kenney writes, and goes on to consider a large number of academic-industrial consortia in the U.S. His conclusion is significant: "Critics ... are worried about the outside influence on the research agenda focus of the individual and his project – not recognizing that the act of funding itself creates the agenda. One need not ask or force an investigator to do specific research – one need only fund the proper scientist to do what he wants". This is a method for "management" in research that has been developed long ago and used successfully by the Rockefeller Foundation". (Kenney, 1986: 59)

Research evaluation when seen in a broader context is an important ingredient in the negotiations that lay behind research agendas, even in basic science.

Concluding remarks

In the foregoing I have tried to indicate a need for a bridging between science policy studies, evaluation research and science studies. This latter field has potentially a lot to offer when it comes to planning and policy making, and at the same time it can itself benefit from interaction with the world of R&D management and science policy. All too often there has been a tendency of science studies scholarship and operational work in planing and policy making to live their own lives, in mutual isolation. Consequently science policy studies too have tended to fall on the margins of science studies, unaffected by the cognitive turn of the 1970s and 80s in the social studies of science and technology, while at the same time strongly influenced by changing fashions and pragmatism in the practical domain.

My overall impression is that: 1) the science policy discourse is strongly reactive and phenomenological in relationship to the "reality" it purports to describe; 2) much of it is consequently ad hoc, with anecdotal evidence mustered to make claims with respect to various "trends"; 3) there is an almost total lack of self-reflexivity, of the type one finds elsewhere in science studies – this is most striking in Eastern and Central Europe; 4) there is no easily identifiable approach that might lead to a coherent conceptual framework to deal with the types of phenomena and questions taken up; and 5) there is a lack of consensus regarding the most appropriate unit of analysis – even the often

used term "the research system" is beridden with so much interpretative flexibility that it is difficult to pin it down, let alone generalize across country boundaries.

In the West the cognitive turn in the sociology of science has been an important corrective to scientistic thinking even here. It also helped constitute the social studies of science as a distinct field between the history and philosophy of science. At the same time by its success and professionalisation it contributed to a widening of the gap vis a vis science policy studies. Sociologists forgot about institutions to check into the fabric of life in laboratories and other research sites. Amongst policy analysts by contrast attention to macro-level events, institutional arrangements and socio-historical context continued to be relevant. Much of the present day project of recovering the politics of science for STS in the 1990s hinges on the possibility of reconnecting these two separate tributaries in the wider field of science studies. The focus on institutions and the socially constructed character of evaluations with their implicit element of a micropolitics of representation and concomitant struggle for control may well be a fruitful bridge.

In the literature it has been argued that, historically science derived its power and increasing dominance from its seeming purity, and that this rise to power coincides with the separation of fact from value. "Value-neutrality was associated with freedom and control, and ultimately with an eye to the prestige won by physicists and chemists in their mastery over nature". (Proctor, 1991: 67) The physics ideal became a strong regulative in the self-understanding of scientists as a community of professionals, particularly after World War II. Paul Forman has called it the myth of transcendence. (Elzinga, 1987) This self-image of disembodiment amongst scientists is reinforced by the current discourse of globalisation, which draws upon the rhetoric of scientific internationalism, thus driven from two sides, on the one hand with reference to the globalisation of policy agendas, on the other as part of the internalist quest for purity.

The interplay between science and politics, schematically represented in terms of two interfoliated credibility cycles, involves on the one hand scientific recognition, on the other money and power (Latour & Woolgar, 1979; Rip, 1988: 59–85; Barnes, 1985: 46). This is the stuff of the tradeoff between science and politics. Scientists are encouraged to engage in basic (strategic) research, since it is the results of such research that counts as hard currency in the political arena. Research framed within an environmental institutional motive, for example, has a symbolic-instrumental role in the wider political context. The two mutually reinforcing credibility cycles allows science to function as the continuation of politics by other means, without too seriously warping its internal dynamics. This is because the condition is that what comes out must be acceptable as quality science within an international scientific community. Scientific credibility is thus needed to underwrite political credibility (Elzinga, 1993).

Of course in cases of extreme relevance and accountability pressure, or in the absence of an internal research policy consciousness on the part of leading scientists involved, the preconditions for science to play its political role may collapse. The possibility of an extreme in "epistemic drift" arises.

Recognition is symbolic capital that gives power and prestige on the scientific arena, but it may also be cashed in on the political arena, in the struggle for fundability and in the context of advise to decision-making outside science. The stronger the purity and universality with which knowledge claims can be presented, the stronger the exchange rate at which the currency of science can be pinned. Contrariwise, results that are controversial, contended or lack the backing of substantial fractions of relevant scientific communities will figure poorly as currency in political decision-making. High level authority of knowledge claims in the internal cycle of scientific recognition, together with broad consensus in the scientific community will give high political legitimacy in the ex-

tra-scientific credibility cycle, and *vice versa* for low level establishment of knowledge claims, controversy and low consensus on science. The latter situation is countered by trying to broaden and deepen the anchorage of knowledge claims, internally, and therewith externally.[1]

Whether we like or not, research evaluation also plays an important role in this context. Here too the purity and power formula seems to be present. The more a given evaluation can be presented in terms that abstract from the particular and give a semblance of objectivity and universality (not infrequently with the help of numbers), the easier it may be for politicians, policy makers and planners to use the results in order to reinforce their control. The bottom line of course is that there is a reasonable degree of participation and consensus on the part of the scientific communities concerned. The different representations of science will never converge, but in the process of evaluation there is a lot of mediation that goes on, and therewith each side – ideally – gains a better understanding of the picture assumed by the other. In this process both internalist and externalist representations of science become modified. This is what much of what is called "strategic research" is all about. [2]

NOTES

1. In a forthcoming dissertation at the Department of Theory of Science and Research, University of Göteborg, Jan Nolin analyzes this process of what he calls internal and external "broadening" of concerned constituencies for the case or research and political action pertaining to ozone depletion. (Nolin, 1994)
2. This paper was presented at the Nordic Research Symposium: Studies of Research Evaluation, Copenhagen November 25–26, 1994

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