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The republic of science in the 1990s

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Summary. Research councils began as channels for state patronage of science (a widespread phenomenon after World War II) and were captured by the scientists: peer review of proposals, panels, board membership. In this way, they became an important organ of the 'Republic of Science' (Michael Polanyi's concept). Being awarded a grant is now as important for the reputation or status of a scientist as the money value per se: research councils have become part of the reward system of science. Credibility-cycle analysis (Latour and Woolgar) is used to show this; and then applied to the research council itself, between the State and the national scientific community. Current concerns about proposal success rates and conservatism are analysed in terms of dynamics of this research world. This sociological approach to research councils allows analysis of changes in the reward system of science (where 'relevance' is becoming an accepted criterion world-wide) and of the complex environment of research councils, where many actors now compete for the intermediary role. Research councils must also become entrepreneurial – or become obsolete.

Introduction

Research Councils can be described, in the words of David Williamson (1992, p. 31), as hesitating between 'a parliament of scientists and a government bureaucracy'. As the former, they embody the ideology, and to some extent the practice, of what Michael Polanyi called the 'Republic of Science' (Polanyi 1962). As the latter, they reflect their origin, and raison d'être, as a government agency dispensing public money. Their hesitation between the two poles of their existence is understandable, if one knows their history and sociology. In this article, I will focus on the dual nature of research councils, and offer a sociology of the research council world as the necessary basis for an assessment of their present situation and actual and potential trends.

Before I do this, I have to locate my object, the research councils, in relation to science as well as in relation to the self-image of scientists. There is a lot of projection of ideals around, and this is a barrier to realistic assessment, both of trends in the practice of science, and of the opportunities for research councils to play their role in the 1990s and beyond.

Research councils and the limitations of an ideology of the Republic of Science

I will use 'research council' as a generic term, denoting a body or set of bodies (or set of arrangements in another body, say, an Academy of Science), which mediate between state patronage of fundamental and strategic research, and the research

world itself: the world of scientists, their immediate institutions, and the research going on there.[1]

All 'research councils' have an internal structure (see Figure 1), with horizontal strata (top level, level of divisions according to broad areas of science, and the level of smaller domains and their panels) and vertical demarcations between areas of science, and sometimes also special programs or types of research (e.g. a special 'column' in the scheme for strategic research, as happened in Switzerland). The horizontal and vertical demarcations may refer to separate bodies, or to internal structures of one or more bodies.

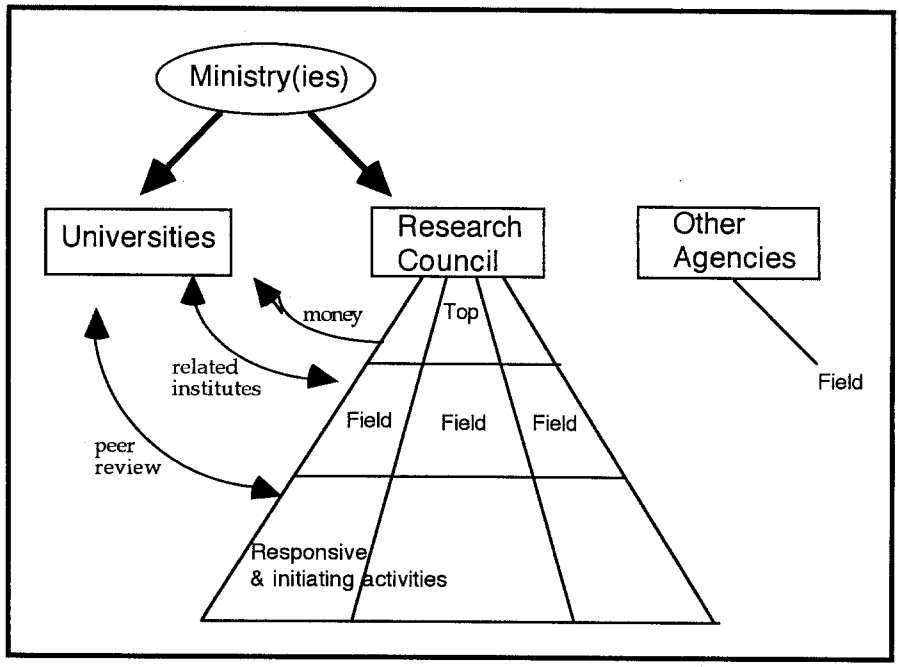

Fig. 1. Research council structural schema.

It is already an interesting comparative exercise to fill in the general scheme of Figure 1 for different countries, and at different time periods. For while reshuffling occurs occasionally, it stays within the limits of the scheme. Recently, there is a tendency for changes to go in the direction of greater unity and a stronger role for 'the top'; this is undoubtedly related to the perception, and the reality, of greater external pressures on the science base.[2]

Another characteristic feature is the set of strong linkages with the university world, particular at the working level of grant recipients, referees for the research council, and joint research institutes. Linkages with the top of universities as institutions (which are increasingly active in the research world) are still rare, although one sees a shared concern with Ph.D. training and some attempts (e.g. by the Economic and Social Research Council in Britain) at consultation and joint action.

In addition, there is a pattern of linkages with government ministries: a Ministry responsible for Science (often in addition to other tasks, especially Education), but also with other Ministries, e.g. a Ministry for Industry (and/or Trade, Commerce, Economic Affairs), and incidentally a Ministry for Cultural Affairs, playing secondary roles. While the details of the pattern of linkages may differ across countries, the general shape is the same, and is related to the functions of the research council, as these have emerged over recent decades.

There is no 'parliament of science' yet in this description. Is there indeed a polity of science that can relate to the work of grant giving and have some delegated representation in a research council? It is possible to think of the research going on within a nation state as being done in a world populated by a national scientific community (I avoid the concept of 'science base' here, because this already prejudices the issue, introducing a particular view of the role science should play in society). If the members of this world are a scientific community, it is not the usual scientific community that carries a discipline or a specialty, and is identified with such a scientific field internationally. It is a scientific community of science-in-general (note that in quite a number of countries, science includes social science and humanities, as in Germany where the language seems to necessitate this already, the term *Wissenschaft* covering both), and bounded by the nation state. This national scientific community has all sorts of ties with the national research council, as I will discuss in more detail later. It is because of those ties that the research council can speak, with some justification, for the national scientific community.

In the discourse of science, there is also the idea of an international and cosmopolitan scientific community, transcending the disciplinary and specialty communities. If the latter are the weft, the national scientific communities are the warp of its fabric. If one allows for some sense of unity across national borders, the national scientific communities, and their precarious aggregation,[3] are the nearest equivalent to Polanyi's idea of a Republic of Science.

Polanyi did not think of a parliament (rather of open and benevolent ruling elites), but his views on the workings of science can still be used to justify arrangements and processes in the research councils:

> So long as each allocation [of money, and from whatever source] follows the guidance of scientific opinion, by giving preference to the most promising scientists and subjects, the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole (Polanyi, in Shils 1968, p. 8).

The invisible hand of scientific opinion is made visible, in a sense, in the way a research council draws on the judgements of scientists. Thus, the quote could well be a motto for research councils, especially for those like the Deutsche Forschungsgemeinschaft, which take pride in being an organ (i.e. a cross between a parliament and a benevolent elite) of the scientific community (national as well as international).

Polanyi's approach is definitely one starting point for looking at peer review, and how it should function. Polanyi, interestingly, was thinking already about the issue

of how knowledgeable the peers can, and should, be. In his view, scientific opinion is coherent across science because the 'neighbourhoods' of immediate expertise overlap. While he could, in his paper, posit this, and leave it at that, for research councils it becomes a concern, and one that should be checked empirically: how much overlap is there between neighbourhoods, and will the resulting common denominator still be an interesting scientific opinion?

The Republic of Science in the 1990s, as represented by the alliance between scientific communities and research councils, may not be what Polanyi was looking for; but its political and economic theory is worth pursuing – if based on a sound sociology. Polanyi's approach remains important, partly because there is real value in it, partly because many of his ideas remain part of the self-image of the scientific community, or better, of the benevolent elites that rule the world of science.

In the self-image of scientists, the fact that research councils are government bureaucracies, is a necessary evil. One should follow the rules and regulations, have annual reports, go through paperwork, and generally be accountable – but it really is a drain on time and effort, which had better be spent on research. This tells us more about the way scientists think than about the nature of the research councils. For scientists, the Republic of Science has a right to get the necessary funds, and without ties attached. Everything else can only undermine the health of science and limit its advancement.

This view becomes a rallying cry in times of political inroads pressing for relevance and short-term goals ('value for money'). I am not implying that these pressures do not undermine the health of science at all; they certainly change science as we know it.[4] However, the Republic of Science in its present form is not sacrosanct. When research councils identify more strongly with the role of 'government bureaucracy', and become pro-active for example, this should not be equated with external pressure on science. If we do that, it becomes a foregone conclusion that all problems we have, in grant-giving and grant-management procedures and processes, and in the external role and linkages of research councils, are caused by our leaving the straight and narrow path of the Republic of Science. Such a reification of the idea of a Republic of Science is dangerous, for the research councils as well as for the scientists, even while it is a tempting position to take, at least for the established elites.

The dangers are of two kinds: 'blinders' and missed opportunities. One blinder is the assumption that state patronage of science, without ties attached, is a right, and one that does not have to be earned again and again, in changing circumstances. A second blinder is the assumption that the procedures and processes 'to divide the spoils' are a matter of the scientists alone, or better, of the charmed circle running the Republic of Science.[5] And thirdly, there is the assumption that there are no other relevant actors in the science scene. Finally, there is the question if scientists themselves are not changing their preferred ways of doing science, and their resource mobilisation strategies, so that the spokesmen of science are defending a science that does not exist anymore.

Reflection on assumptions is necessary, otherwise reactions may be inadequate and opportunities will be missed. A concrete example (which I will discuss later) is

the possible new role that is emerging for the research councils. In addition to the roles of a government bureaucracy and a parliament of the Republic of Science, some research councils are taking initiatives as independent bodies, defining priorities and seeking opportunities to create and execute strategic programs (up to co-funding arrangements). Is this an aberration? Or, as I will contend, a third role, that of entrepreneur in an intermediary level of the research system?[6] Whatever the final assessment, it will be clear that the ideal of a Republic of Science should not, in the 1990s, be taken as the standard against which to evaluate what is happening, nor should it be projected simplistically on the history of the research councils, as a myth of origin.

The history of research councils

The history of the several individual research councils, insofar these have been written, creates a complex picture, with national variations, especially before the 1950s. The key point to draw out is that they were primarily set up as government bureaucracies to handle state patronage. After 1945, partly because of the role science had played in the war effort, and partly because of the role modern states were according to science more generally, research councils were established everywhere.[7] There were scientists' initiatives also (especially by physicists, and because of their link with atomic physics), but these fell on fertile political ground and were incorporated into the new system.[8]

Across the spectrum of sciences, as these are now served by the research councils, state patronage in the generalised sense was a new phenomenon that, in a sense, intruded on the earlier freedom in poverty. And scientists were not always willing to become involved. At first, the agencies sought advice from key scientists, as had been usual in earlier patronage of science (of the state, but also of the big foundations, with the Rockefeller Foundation as a well-known example).[9] It was gradually broadened to become what we now call peer review of proposals. Indeed, Nathan Reingold has argued that peer review of proposals in the US National Institutes of Health (NIH) was set up to overcome the resistance of the medical establishment to NIH (Rip 1985). The involvement of distinguished members of the scientific community in the work of the funding agency has more roots, however, including ideas derived from the experience with industrial research in the interbellum.[10]

The important point to get across is that peer review of proposals – this 'higher form of nonsense', as John Ziman (1983) has called it – did not originate in the scientific community. Government bureaucrats (sometimes ex-scientists) needed advice, and legitimate advice, in their new activity of disbursing funds to basic science. Their discretionary power was formally only limited by the structure of the bureaucracy they were part of. In seeking advice that was legitimate to the scientific community, they accepted a second constraint, that of acceptability to these scientific communities and their ruling elites. The bureaucracy of science funding agencies became thus permeated with scientific values.[11] Conversely, the

way the scientists constructed their funding advice, would to some extent reflect the political culture of the country.[12]

Over time, scientists *captured* the research council system, with peer review of proposals, and with positions on advisory committees and on governing boards. And the research councils became legitimate to scientists in this way, and were often considered to be an obvious part of the world of science. Hence, the possibility of seeing the research council as a parliament of science.

The capture of the research councils, and thus conditional self-patronage, left traces in the way science was being done. These effects can be seen on two levels. The reward and reputation system changed: grants from the national research council, with its (particular) peer review system, became indicators of quality and credit ('credibility'). In job applications and tenure decisions, it is important to show the grants one has been awarded.[13] So the research councils have become part of the reward system of science, and the reward system had to change to accommodate the presence of the research councils. One effect is that peer review of proposals has become an accepted part of the culture of science, so much so that the refereeing effort is seen as 'a good citizen chore that comes with membership in the scientific community' (Chubin and Hackett 1990, p. 83). Indeed, requests for a referee report get a high percentage of positive responses (70% in the UK, and in other countries, the order of magnitude appears to be the same).[14]

The second level of change is in the research practices. One effect is the new emphasis on relatively small, principal-investigator led research projects, often with junior researchers or Ph.D. students as the main performers, and research articles and/or Ph.D. theses as the main outputs. Depending on the set-up of the research council and the situation of the country, there can be other effects. For example, Harvey Brooks (1971) has noted how individualism and recognition of young researchers has been enhanced by the availability of project grants, in conjunction with a national standard of quality judgment. The mobility of (young) researchers, now seen as characteristic for the USA, depends on the existence of NSF and NIH, and how these allowed them to circumvent the traditional science-internal patronage by (and thus dependence on) established professors and heads of institutes, that were (and sometimes still are) so characteristic of European science.[15]

From this brief discussion, it will be clear that the introduction of research councils in the research world has had epistemic effects. Elzinga (1985) and other authors are concerned about epistemic drift because of relevance and accountability criteria becoming more important in science, and they contrast the present situation with that of the 1960s. The lack of recognition (with these authors, and generally) of the earlier 'epistemic drift' that research councils have introduced into the research world goes to show how much the research councils have become integrated into the research world. They are part of the culture and the structure.

Similarly, peer review of proposals seems to have become a right of scientists, rather than a duty. Something that cannot be taken away, or even criticised, without scientists starting to invoke dire consequences. I am not criticising peer review of proposals here (this type of mechanism to mobilise advice may well remain useful), but just pointing out how the way it is treated shows how much it has become part

of the culture of science itself, rather than a procedure that helped serve the specific purpose of a government agency.

The sociology of the research council world

Polanyi's analysis of the Republic of Science identifies some, but only some, elements of the sociology of a world of science in which research councils are an integral part. In relation to peer review, he emphasised professional standards of evaluation throughout science, and argued that 'overlapping neighbourhoods' allowed for coherence, and some uniformity of standards. There is indeed such a shared competence if one focuses on the craft of doing research. But questions of relevance of a proposal for scientific advancement, the other main criterion for funding, may well get widely divergent answers. Peer review works, but it works better for judging the minimum requirements of adequate work, than for assessing overall quality in relation to contributions to scientific advance.

Because Polanyi neglected the issue of strategies to acquire resources (although any realistic political economy of the Republic of Science should address that issue) and similarly, the permeability of the boundaries between the world of science and the rest of the world, he could not analyse the role of the research councils. To do that, I will start by considering actions of scientists and how these link up into overall patterns.

Scientists, in their research practice in a concrete institution, come up with knowledge claims which they offer to forums (like scientific meetings, scientific/scholarly journals) in relevant scientific fields. Both local research practices and more cosmopolitan scientific fields evolve, coupled to each other because of the effort to get knowledge claims accepted. (The left-hand side of Figure 2 visualises this analysis; see Rip 1988 for further discussion.) Prospective authors of a knowledge claim (empirical findings, analysis, theory) struggle to have it become more like a 'fact', i.e. widely accepted as valid. At the same time, others are trying to show its limitations or even complete failure. This 'struggle for facticity' (Latour and Woolgar 1979) is the sociological version of Popper's philosophy of conjectures and refutations, and many more ploys are allowed in the 'war games' in the real world than Popper (and Merton in his discussion of 'organised scepticism' as a norm of science) would accept. But it is not a case of anything goes: the struggles become institutionalised, and a repertoire of professional standards emerges to judge contributions, and to judge who is the winner of the game, in the particular area in which it is played out.[16] Thomas Kuhn's concept of a 'scientific paradigm' captures part of the notion of area-specific standards of judgement; the concept of 'repertoire' is broader, and can better be related to the mix and make-up of the institutions linked with the scientific field (cf. Figure 2).

Now add the research councils to this picture. After the early and tentative procedures and processes in the interaction between grant givers and potential grantees, a 'struggle for fundability' became institutionalised. How to get a research proposal accepted as 'fundable' by the funding agency, most often with

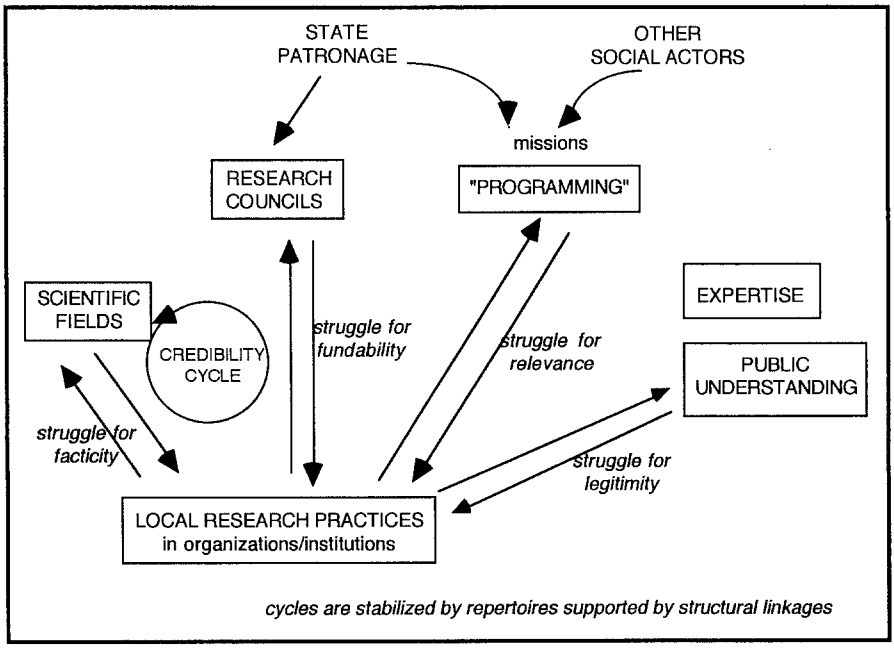

Fig. 2. Struggles for relevance and legitimacy on top of the traditional struggles for facticity and fundability.

the help of peer review, was not an ad-hoc affair any more, but based on justified mutual expectations. While Ziman is right, in principle, in saying that the attempt to judge the quality of future research is 'a higher form of nonsense', in practice a workable repertoire of how to handle proposals has emerged, as well as a certain competence of how to write fundable proposals (see for example Myers 1985).

The repertoire of fundability has, in fact, become sufficiently articulated and accepted to allow for strategical action (how to turn it to one's own interests), and to poke fun at it (Dan Greenberg's (1966) Dr. Grant Swinger remains the classic example). I note in passing that the articulation (and the nature) of the repertoire differs across disciplines and research areas, with physical and life sciences having progressed farthest in this direction.[17]

The notion of 'repertoire' has another advantage: one can get behind official procedures and general criteria. Ongoing practices in the funding world are what determine what happens. The repertoires involved, rather than formal rules, shape these practices. Formal rules and criteria, however, are necessary in the funding world, already because a government bureaucracy needs them, and more generally, because external legitimation can be supported by pointing to criteria.[18]

The fundability repertoire has introduced a second set of professional standards in science, since the funding agencies have become an integral part of the world of science. The two struggles, for facticity and for fundability, are fought in one and the same practice of scientists. One can analyse the situation with the help of a 'credibility cycle': individual scientists or small research groups accumulate

credibility, that is, the credit (and legitimation) for knowledge claims, which is transformed into reputation and the chance of having further proposals funded. Eventual funding is transformed into resources like apparatus and research assistants, and thus into the production of data for further knowledge claims. And so on (see Figure 3, adapted and extended from Latour and Woolgar 1979). Scientists work in such credibility cycles, their career depends on the success in accumulating credibility (as cultural capital), and the more entrepreneurial scientists actively develop strategies and optimise the accumulation of credibility.

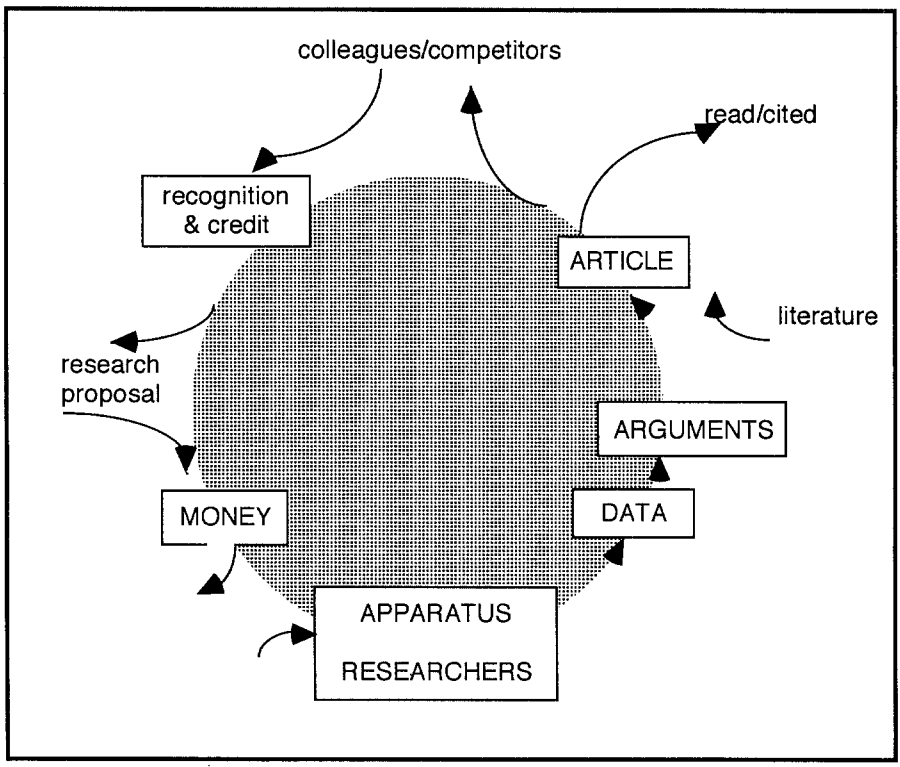

Fig. 3. The credibility cycle for a scientist (adapted from Latour and Woolgar 1979).

The conceptualisation in terms of 'credibility cycles' is a useful tool to understand the present situation of science, especially in fields with high mobility, like biomedical research in the USA. That is, in fact, the field where Latour and Woolgar (1979) drew their empirical material from; and they took the existence of mobile money, especially NIH and NSF funding, for granted. So while the research council is present in the cycle, it remained a black box in their analysis.

When the research council and its linkages are introduced explicitly, these then include linkages to the state: the council's budget derives from state patronage. It turns out that one can, in fact, describe the situation of the research council in terms of a credibility cycle of its own. Figure 4 (which also visualises the way peer review has become added to the activities of the scientific communities) shows how

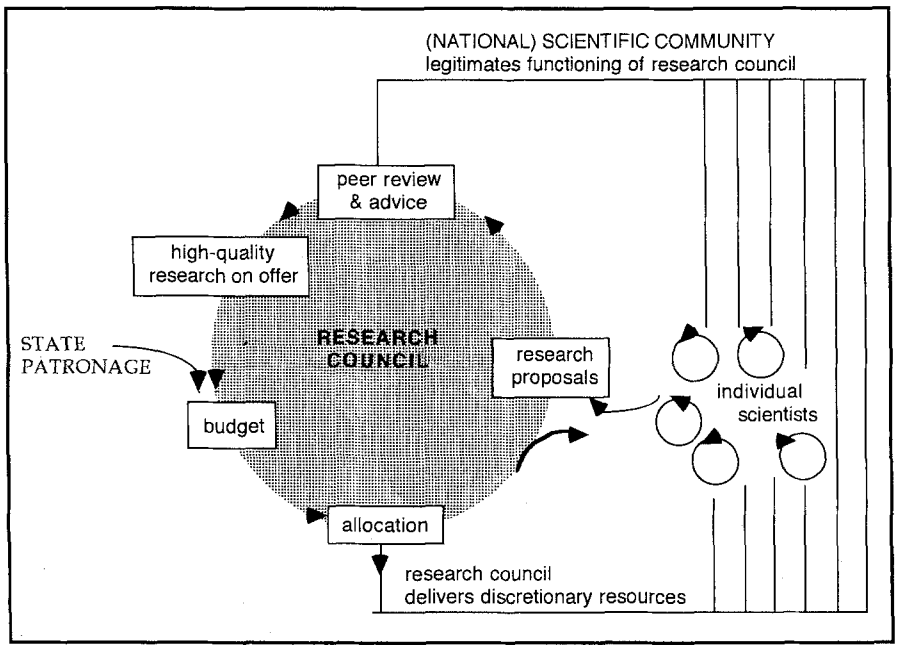

Fig. 4. The credibility cycle for a research council.

research councils must legitimate next year's budget in terms of an attractive portfolio of projects-to-be-funded, while the money they get must be disbursed in a way that is acceptable to the scientists. Individual scientists may grumble. However, one might well neglect such grumblings as long as they remain confined to individual cases, as scientists also tend to exploit whatever funding possibilities they can find. Acceptability to the scientific community is important, since their elites will often be involved in the workings of the research council. The cycle continues through the submission of new proposals and the advice offered through peer review. So while scientists depend on research councils for at least part of their funding, the research councils depend on scientists for new, and interesting, proposals and for time given freely to prepare advice.

Polanyi, for his own (and ideological) reasons, emphasised that there was no central authority in the Republic of Science, nor was one needed. Here, we have a situation where there is centralised decision-making (it cannot be otherwise, because of the accountability requirement of a government bureaucracy). The form of the decision-making by the research council, however, as it appeared in the 1960s and 1970s, is that of 'setting the law' rather than making concrete judgements. Thus, the legitimacy of the central authority depends on it being shared, through the linked credibility cycles, with scientific communities and their elites. So it is, to some extent, a dispersed authority, and Polanyi's analysis remains applicable, as long as the research council world retains this structure.

One can read the scheme of Figure 4 also in another way: because they have two patrons, the state and the scientific community, the research councils are relatively

independent with respect to either of them. While I emphasised how the funding agencies were captured by scientists and became part of the world of science, this does not imply that they necessarily lose their independence. Rather, so long as governments were happy to hand out money without asking for actual or symbolic returns, the links with the scientific community became the only important ones for the research council to cultivate. This, then, created institutional momentum, which produced achievements, but also a particular structure and culture (including what I called the ideology of the Republic of Science), which can be a constraint when the context changes. As it did in the 1970s and 1980s, when governments wanted to influence the nature and direction of funding. The effect of such constraints can be seen in the Netherlands, where it took the research council a long time to become more independent from the scientific community (and also to start building up new links with the government). In Germany, the Deutsche Forschungsgemeinschaft still seems to hold out, while Australia may be a case where the research council came in late, took the 1960s and 1970s research councils as a role model in a context in which this model no longer fitted.

While research councils can exploit their relative independence if they want, they are not completely free. In the terminology of principals that try to get agents to do what the principal wishes them to do (a terminology derived from rational choice theory, and suitable to analyse this aspect of the workings of this three-level system),[19] a research council is agent of the state, which as patron of science is its principal. Research councils themselves are (uncertain) principals with regard to the scientists, who have to do the research the councils are paying them for. The uncertainty derives not only from the open-ended character of grants, and the tradition of few checks on the actual research and its outcomes, but also from their being dependent on scientific fields, disciplines, scientific specialities, for their evaluative repertoires (Van der Meulen 1992). Because of these dependencies, research councils cannot depart too far from what is customary in a scientific field, on pain of doing their job less well (in addition to a loss of legitimacy with the scientists).

The research council world

This whole system works, but precariously. Take for example the success rate of proposals: in the USA concern has been expressed about the rate becoming lower than 30% (Chubin 1990), in Australia the rate is of the order of 20% and scientists complain (see Brennan, this volume), while in the Netherlands in several of the areas covered the rate is 20% without scientists/scholars complaining over much. One point is that it is necessary to analyse the situation in relation to other possibilities of obtaining funding: in the USA, for example, NSF with its investigator-initiated grants program, covers only a small part of basic research funding, and mission agencies are the big providers with the National Institutes of Health located in between (Teich, this volume; see also Smith 1990, p. 46). But it is not only a matter of money. There are good reasons why scientists are concerned

about lower success rates with the research councils *per se*, because for a long time, obtaining these grants was necessary to be qualified as a good scientist. An Australia Research Council dollar, it has been said, is worth three times as much as other dollars, because it is a high-status dollar (and when used to assess status, and confer credibility, will have a multiplier effect). Thus, an integral part of the reward system of science is being threatened when there is not enough money to fund all the 'good' proposals. For research councils, the concern is that they will not be able to continue to attract good proposals, because scientists give up putting effort into writing and submitting them. This would endanger their own credibility cycle.

Indeed, one can think of a spiral down. With a success rate of 30%, one needs three submissions to have a real chance of getting at least one funded, and this amount of effort may just be manageable.[20] When the success rate falls further this will force scientists to write and submit more proposals. With the same amount of funding available, the success rate will drop further, requiring even more proposals to be written and submitted to give oneself a chance. And so on, until the system breaks down. (The process can be slowed when the research council decides to lower the average size of grants, but that has its own problems, and does not change the direction of the spiral.)

There is a mechanism, though, that could contain the down spiralling. When a 'social world' of interacting, mutually dependent scientists has emerged, access to funding can be self-regulated. In the case of physics and parts of chemistry, the scientific communities are tightly knit, and their link with the funding agency is well articulated, both as to evaluative repertoires and because of an assured social memory, i.e. a fair distribution of funding over time, because considerations who had got funds the year before play a role in the advice and decision making of this year. This is collegial allocation of money, which can, without external control and self-control, become indistinguishable from old-boys' networks and 'mafia's'. But it also allows self-regulation of the number of proposals submitted, as long as continuity in relationships is expected. One can thus also understand the veering away (and up) of the NSF proposal success rate from the magical bottom-line of 30%, which it was working on for some years, as related to the two scientist-initiated processes I described: scientists giving up on NSF and going for other funding (see also below, on new sources of money and reputation), and scientists being more selective in their submissions (McCullough, this volume).[21]

The emergence of a 'social world' which allows long-term considerations to play a role, has a further implication: the advent of new actors will break up the coherence, at least for some time. This seems to be happening in Australia, where the Unified National System has brought in many actors who, already for institutional reasons, apply for grants. The Australian Research Council consequently is swamped with submissions and it will take time for the situation to sort itself out.

Another part of the social system dynamics becomes visible if one considers the additional effort that has to be expended to let the system work: the visible and invisible burdens of responsible funding. In Figure 5, the several types of effort are indicated, which imply costs to the funding agency and to the scientists, respectively (I have added the other activities in the science world which are not

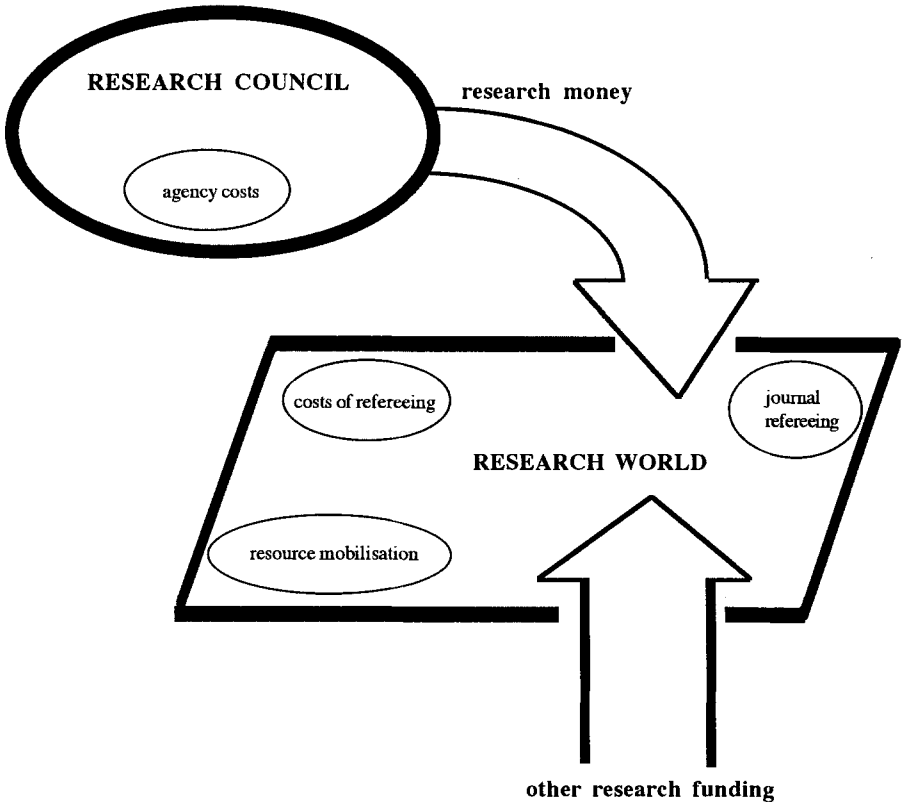

Fig. 5. The burdens of responsible funding.

immediately research related and can thus be seen as an add-on cost). I have also tried to put numbers to them. Agency costs are often quoted as between 2 and 5% of the budget for responsive funding.[22] Then, there are the costs of researchers who referee proposals, and are rapporteurs for end-of-award and other evaluations – another good citizen chore that goes with membership of the scientific community, and which can thus be seen as a membership fee for the research council world. For the UK, I have estimated that 30 man years are involved each year. For the USA, data from Chubin (1990) allow an estimated 100 man years for NSF refereeing work, which is the money equivalent of 10% of the total amount of awards (year 1988).[23] It is much more difficult to come up with an estimate of the fraction of time of the researchers which is spent on refereeing and panel work. Abrams (1991) quotes a member of the NSF Ecology Panel as saying that he spends up to 10% of his research time per year on the reviewing and panel discussions that he has to do. To transform these data into figures for the research world, at least those researchers involved with research councils in one way or another, requires a choice of the size of this population, and the extent of research time they would have available. When building a model, one can just put numbers on the different parameters and variables; when one includes figures for time spent on resource

mobilisation (which includes the writing of proposals for funding agencies), one can try to simulate the magical 30% bottom line for success rate of proposals. To check whether the total effort involved in acquiring funds from a research council and contributing to its refereeing process is about the same (moneywise) as the total funding flowing from the research council to the researchers (a concern which has been voiced sometimes) one needs empirically supported numbers.

The structure and culture of the research council world, in relation to the research world in general, emerged because of the peculiar combination of funding and peer review of proposals, and now shape these processes. The issue of fairness, while important if there is concern about how the spoils are divided and what to do with newcomers (especially in the USA, because of its political culture, this is also a concern of outsiders like congressmen) is not the main substantial issue. To my mind, innovativeness is a more important issue, because there is a trade-off in the workings of the research council world: coherence and continuity is necessary to function productively, but this creates difficulties for novel approaches, rather than stimulating this aspect of excellence in science. This is a widespread concern. Because the problem is recognised, there is effort to compensate, e.g. by the research council taking special measures to support young researchers. Special effort can also occur in peer review of proposals, especially if there is a close-knit community interested in its own intellectual reproduction, as is for example the case in physics.

The complexity of the issue is well brought out by Polanyi (1962) when he observed: 'The professional standards of science must impose a framework of discipline [that is, the discipline of following existing criteria and approaches] and at the same time encourage rebellion against it.' The disciplining is typically done in peer review of manuscripts and other research products, and in the sub-culturally defined 'organised scepticism'.[24] Peer review of proposals reinforces the disciplining, and does so at a very early stage, when access to resources is decided. One way of allowing 'rebellion' to get funds and show what it is worth, is to have a plurality of funding sources each with their own style and criteria of decision-making. In a monopsony situation, or in a funding agency that takes Polanyi's observation to heart, the risk-taking strategy of those in a position to decide on resources is the key factor. As the director of the Dutch funding agency for technical sciences emphasises: if all the projects that I fund turn out successful there is something wrong; a risk-averse strategy must have been followed and I should do something about it.[25]

But this requires discretionary power, which does not sit easily in a bureaucracy (cf. the need for accountability) or in hierarchical structures in general. Patrons of science, who are not accountable to anyone but themselves, have discretionary power, or in other words, can be arbitrary in their decisions, and can thus introduce room for novel approaches, lateral thinking, and protection of mavericks.[26] An approximation to such patrons is the staff officer of a research council who has (or allows himself/herself) some discretionary power. This can reside in the structure and culture of the research council (as with NSF), or in personality and/or long-term thinking which leads to the creation of a broad portfolio, which includes

projects that fail. Conversely, the 'timid bureaucrat' reinforces the conservative tendencies of peer review (which are backed-up by the diffuse accountability of the research council to the scientific community in general).

Somewhere in the system, risk-taking must occur. Scientists themselves will bear part of it (and in larger research programs and institutes, the director can play the role of patron for a few novel approaches). Research councils should also take part, if only because their set-up by itself necessarily reinforces conservative tendencies.

Recent changes and policy implications

The advantage of the analysis in term of linked credibility cycles is that it allows for tracing influences on research councils and what the effects might be. For example when government agencies start to set priorities and/or want the research council to be pro-active, rather than responsive, this must be accommodated in the research council credibility cycle. There may well be resistance, but at the risk of losing credibility with the state, and with the public. In the USA, for example, NSF has always been sensitive to its public legitimacy, fearing a recurrence of the 'Golden Fleece Award' (bestowed by some members of Congress on the most idiotic project funded by NSF) and the effects a loss of legitimacy could have. On the other hand, the research council has to maintain credibility with the scientific community, and even if it wished, it could not completely go with new government priorities and/or prevailing political fashion.

But the system is dynamic, parts of it adapt to internal or external changes. And if the way science is being done changes, the research council world will definitely shift – and have to shift, on pain of being bypassed. There are a number of developments that indicate that change is in order.

Especially in Europe, the new science and technology stimulation programs (nationally, and at the level of the European Community) are definitely an important presence. They are becoming part of the reward system of science: If you're not in ESPRIT or RACE, you can't be good, that's what the world looks like in areas of physics, electronics and telematics.

Everywhere university/industry linkages are visible, universities are 'on the move'. For an academic career, publications remain important, but the amount of money s/he brings in, and the kind of network linkages, are also important when hiring a professor. In addition to struggle for facticity and fundability, relevance is becoming an accepted criterion and ways to judge what is relevant are being articulated. (Cf. Figure 2) The category of 'strategic science' (which has a history of its own) has been taken up to formulate the long-term relevance of basic research in opening up opportunities for innovation and other 'relevant' activities. The 'centres movement', which took off during the 1980s, with examples like the Engineering Research Centers in the USA and Cooperative Research Centers in Australia, is an indication of the new goals. Once institutionalised, they become loci where criteria for relevance can be articulated, and where scientists can accumulate credibility, now also in those terms (Rip 1990).

The world of the *scientist* is becoming more complex. This is easily demonstrated if one thinks of publishing by press conference, about public controversies over research, about the concern over public understanding of science, and a range of further issues, now centring on public legitimacy. For research councils, the world becomes more complex as well. First, because they must relate to a world of science that is changing. Second, because governments become more directive and press for value for money (whatever 'value' may be). So on both sides of the credibility cycle of the research council, there is pressure for change.

A second source of complexity is that the research councils are not alone anymore in the intermediary level between the state and ongoing research 'on location'. Not only the new strategic programs for science and technology (increasingly with cross-linkages with (some) research councils) operate as actors in the intermediary level, trying to attract good research. Universities, apart from influencing their own researchers, have become more active as institutions, and move on the market of strategic research (e.g. in the substantial power politics of trying to get a Centre attached to them). Research institutions appear outside the traditional university/research council combine, with the Wissenschaftszentrum Berlin and the International Institute for Applied Systems Analysis as two examples located in Europe.

The ecology of the intermediate layer has been important all along for the orientation of the research councils. Now the ecology is changing, and the research councils respond, depending on the niche they created and grew up in.

In the Netherlands and Germany, there were already actors oriented to strategic science since the 1970s, so the research councils could continue to focus on the advancement of science. For Australia, Max Brennan observed: 'The fact that a lot of basic research is done in government institutions relieved pressure on research councils to target research funds to priority areas' (OECD/DSTI 1992). In the UK, in the 1970s, there were no actors like National Programs or sector councils, so the research councils were exposed to the full pressure, became more active, and took 'relevance' up as one of their criteria (and with success). In the USA, NSF is somewhat protected by the fact that it is only one of the actors funding basic research. But in US political culture, every agency is exposed.

Internally in the research council world, peer review should not be the key issue, but initiative, discretion and competence of staff and/or scientific decision-makers. Instead of 'improving' peer review as such, one should improve the orchestration of peer review. Informally, peer review was always orchestrated by agency staff and/or a scientific director. But this should be recognised, and done as well as possible. Part of that is to recognise referees' reports and panels' conclusions as advice, rather than a final verdict. Another part is to have good support systems, including a data base on referees (to improve the social construction of peers) and bibliometric data on research performance.[27]

The accountability issue implies that evaluation is becoming more important: not only for external reasons (trends toward science under scrutiny and value for money), but also for internal ones. An active research council wants to know what is happening in its world. A management information and decision making support

system is not a luxury. This does require funds which cannot be spent on research. (In general, evaluation is expected to take up 0.5 to 1% of the budget for research.) It is interesting to speculate about implications, for example, whether the availability of information on track record will make that a more important criterion in judging proposals.

What I have been saying about the overall evolution of the research system and the role of research councils in particular seems to point, again and again, to a more active role of the research councils. More active vis-à-vis their clients, but also vis-à-vis their patrons, the state and the national scientific community. And we should go one step further. Envisage a strategic world in which research councils are only one of the actors. Universities, 'Centres', consortia, new government programs of whatever shape are actors, and compete for attention, for scarce funds, and for scarce talent.

There will be resources for strategic research, and, even more importantly, for strategic interaction oriented toward the definition of opportunities and important directions in which to go. For the former, the research councils can build on their tradition and expertise, for the latter they will face the difficulty of being newcomers. In the UK, one does see the research councils taking part in agenda-building and in co-funding arrangements. In this strategic world, research councils will have to bid for money, and for a role to play in strategic networks. While they may do this already, tentatively, informally, and often not wholeheartedly, it will become more important in the future: a future in which the Republic of Science becomes part of an international political economy of science and technology rather than a cultural reserve.

This implies that research councils must indeed see themselves as an independent actor, rather than a channel for funding of research by the state. Does this require a mutation? No, research councils can also build on strength. Their distinctiveness, i.e. comparative advantage, is their access to evaluative repertoires and the 'quality' judgement of the Republic of Science. But as I have been emphasising, the reward system of science is not static. What was a competitive advantage in 1970 need not be one twenty five years later. So research councils have to remain part of the reward system and follow its evolution.

Grant-giving and grant-management processes and procedures are important to get right; they are the day-to-day business of the research council and the major link with the scientific community. But if the research council were to focus only on that, and not on its role in a wider, strategic world, it would be bypassed on all sides. Would there, then, not be a Republic of Science in the 1990s? In any case, its traditional spokesman, the research council, would have disappeared together with the tradition.

Notes

1. The draft OECD report (OECD/DSTI 1992) from which I quoted Williamson, also emphasises the generic use of the term, and adds that the names for such bodies (or arrangements) might differ in different countries; e.g. research foundation, academy (in Finland), national centre (in France and

Italy). They also list features of the generic institution, but then focus on desirable features, rather than empirical characteristics.

2. Norway merged its separate research councils into one overall structure in 1993, Britain (which had independent research councils with a federative Advisory Board for the Research Councils acting as spokesperson in some dealings with the government) will now, according to the White Paper of May 1993, not only reshuffle the domains of the research councils, but also bring them under the direction of a Director-General for the Research Councils who is part of the Office of Science and Technology in the Cabinet Office. In the Netherlands, smaller changes (the introduction of area councils above the several foundations (see van de Kaa, this volume), a more pro-active role of the Board of the central organisation NWO) go in the same direction.
3. The aggregation is precarious, because the sense of being part of an international effort is counteracted by competition and comparisons, themselves induced to some extent by the strategies of national scientific communities to bid for larger chunks of money from their respective governments. Similarly, the fact that national research councils, in spite of the ideology of working for the advancement of science, can give grants only to applicants in their own nation, is both applauded (why dilute the sharing of the spoils) and deplored (if it limits opportunities).
4. The issue, in the end, is whether there is the wrong kind of 'epistemic drift' (Elzinga 1985). In contrast to easy complaints about bureaucracy and relevance, I have argued that epistemic drift occurs all the time (Rip 1988). In a sense, advancement in science is itself epistemic change, drifting or in concentrated bursts. Political pressures could sometimes help, rather than hinder.
5. I am not being flippant, when talking about dividing the spoils, but pointing at established practice. For example, peer review of proposals is very tight, while there is very little concern about changes in the research after it has been funded. Clearly, closely regulating access to funding is what matters, rather than using the funds, once obtained, well. State funding for science is treated as 'spoils', and scientists fight (primarily with arguments) about their division.
6. The intermediate level consists of all the organisations, program bodies, advisory councils and institutionalised procedures between the state level and the level of ongoing research. (See Rip 1988 and 1987b for discussion of emergence and functioning of the intermediary level.) The research councils are early examples of such intermediary-level bodies.
7. In Britain research councils were established already after 1918, and in France CNRS was set up in the middle 1930s.
8. For governments not used to patronage, the lack of accountability in the research council system takes some time to accept. In 1947, USA President Truman held up the establishment of the National Science Foundation because he thought taxpayers' money should not be spent by the Federal Government without knowing what it would get in return (Smith 1990, pp. 50, 52). During the 1950s, and especially in the first half of the 1960s, the research councils were, for a time, given money without strings attached. This 'golden age' for scientists, epitomised in the phrase of Vannevar Bush, 'Science, The Endless Frontier', which helped to set up the research council system in the USA and elsewhere (Bush 1945), is now of the past. Accountability cannot be shrugged off with arguments about the unpredictability but essential fruitfulness of science anymore. The scientist's complaint is that 'the endless frontier' has turned into 'a bureaucratic morass', but as Don Price (1978) has argued, this is partly because politicians and the public have taken the promises of scientists seriously, and are now (finally) pressing for delivery of the goods.
9. See the studies of the role of the Rockefeller Foundation (and its scientific director Warren Weaver) in preparing the ground for what is now called molecular biology. Also the support of science in the interbellum by the Kaiser-Wilhelm-Gesellschaft in Germany. Patronage of science has a long history; the changes occurring in the 20th century relate to the fact that increasingly, the advancement of science rather than the whim of the patron became the criterion for support – with the opinion of the patron, and his advisers, about what constitutes advancement of science still being an important determinant. (Cf. again Warren Weaver and the Rockefeller Foundation.)
10. Bud (1978) emphasised the role model of industrial research on cancer research institutes just before and after World War II; part of the research council world are institutes (in physics and in health, primarily) which are, in a sense, the embodiment of a view put forward in the debate at the time: 'We are convinced that industrial methods (organisation, planning, competent direction) are more

likely to teach us how cancer can be controlled than the kind of empiricism and floundering that passes for cancer research in the universities' (*New York Times* editorial, August 1945, quoted in Bud 1978, p. 429). In Germany, some of the industry-related foundations in the 1920s and 1930s introduced peer review to judge the merit of requests for research support.

11. Indeed, the officers of the funding agencies were often drawn from the scientific community, although not always from the ranks of active scientists. This is a reflection of the difficulty that the government apparatus has had with handling science systematically. So they attracted, and incorporated, scientists, as it were, to create a competence. See Brickman and Rip (1979) for a similar point about science policy advisory councils.
12. Compare Dirk van de Kaa's (this volume) point about the consensus ideal in the Dutch research system.
13. Wood *et al.* (1992) quote the National Science Foundation's Task Force on Merit Review (1990): '. . . institutional and peer pressure on individuals has grown since professional 'success' is now increasingly measured by NSF awards'. They also note 'that the increasing emphasis on externally funded research is creating an academic class system with detrimental effects on both scholarship and teaching, as well as disfunctionally splitting academe into haves and have nots (. . .).' (*ibidem*, at p. 6). As Don Aitkin observed: research council dollars are high-status dollars.
14. The British research councils monitor the refereeing, as well as other aspects of their management of science funding. My data is based on information from officers of the British research councils, and discussions with knowledgeable observers in other countries.
15. Germany, of course, is the main example, where research schools led by a dominant professor continue to be important, and have been, in a sense, enshrined in the way the Max-Planck-Gesellschaft is set up. In such a set-up, conformity to the paradigm is important, while margins for innovation are at the discretion of the elite scientists heading the schools, institutes, and panels of the funding agencies. Their discretion can be larger than in a more public and accountable system (arbitrariness has its good sides!). So the system is not necessarily conservative. Indeed, the American system, with its emphasis on fair treatment, may well be more conservative, in the end, than the more elitist system of (in this case) Germany. In the USA, much depends now on the margins that program officers allow themselves.
16. Polanyi's idea of 'overlapping neighbourhoods' can be operationalised and checked for its sociological realism by checking the content of the repertoires.
17. Compare Wood *et al.* (1992, p. 21): 'Those disciplines which already have a well-established culture of grantmanship will clearly fare better in competition for ARC funds than those where this culture is relatively undeveloped.' Such a 'culture' will relate to the overall situation in the discipline, but also be geared to the specific funding agencies and set-up of the funding system in the own country.
18. The situation is fully equivalent to what happens in the 'struggle for facticity', compare my analysis (inspired by Gilbert and Mulkay) in terms of a contingent repertoire (for daily work and understanding of the inner workings of the system) and rationalistic repertoires for external presentation (e.g. in scientific articles). The role of criteria, especially the influential Weinberg criteria, can usefully be analysed this way.
19. This idea draws on work by Van der Meulen and Rip, originally in relation to another three-level system, that of disciplinary review panels established by the state, and drawing upon evaluative expertise, as well as legitimation, from the disciplinary scientific community. See Van der Meulen *et al.* (1991) and Van der Meulen (1992).
20. It is manageable because scientists can often use their proposals for other submissions as well. Note that the probabilities actually do not work out this way (30% is an average, chances are related to quality, and an ecological fallacy is involved). But it is the kind of strategic reasoning that scientists will follow, and are known to follow, at least in the USA. See Chubin (1990) and Chubin and Hackett (1990).
21. Note that the success rates are different in the various divisions of NSF. Without data on the breakdown of the average figure, and without sociological data on the processes in the relevant scientific communities, one cannot do much more than give the general argument. There are, however, already interesting data on the way scientists submit and re-submit proposals. Myers (1985) is a case study. McCullough (this volume), in his survey of proposal submitters to NSF,

found that 50% of the unsuccessful applicants did not resubmit in any form – which implies that up to 50% did resubmit, if not to NSF, then to some other funding body.

22. NSF spends 5% on agency costs (McCullough, this volume), the Australian Research council only 1.4%, and this implies that it is under-resourced (Stokes 1993).
23. McCullough (this volume) quotes a figure of 100,000 reviews a year. At an average of 3 hours per review, this amounts to 150 man years or more.
24. Polanyi himself has recounted an interesting experience where he developed a theory (on adsorption) in isolation and was criticised later that he was going against the paradigm. He argued that the criticism was justified – even if it turned out, two decades later, that his theory had actually been correct, after all (Polanyi 1963).
25. Cees le Pair, Director, Stichting Technische Wetenschappen.
26. The distinction between discretion and arbitrariness is tenuous. I would not mind adding a lottery component to the funding of proposals.
27. Note that there is no principle contradiction between peer review and bibliometrics, because the bibliometric data have been created through peer judgement (of manuscripts, and in giving citations). It is interesting that funding agencies use bibliometrics much less than universities do. This has everything to do with bibliometric indicators becoming weapons in the collegial war game.

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