AN EXERCISE IN FORESIGHT: THE RESEARCH SYSTEM IN TRANSITION--TO WHAT?

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As this set of papers shows abundantly, the R&D system is changing, and "steady state" is only one possible label to characterize what is happening. The "steady state" is a system-level description; but at a lower levels, specific trends appear, such as internationalization and shared facilities, the emerging linkages between university and industry, and the interest in strategic science. As with the label *steady state*, new terminology is being offered to capture aspects of these lower-level trends. In response to the perception of pervasive change, issues are raised: a reduction of autonomy for researchers; shifts in the role of universities; the need to revise earlier implicit social contracts between science and society.

In this concluding chapter I want to step back from the specific trends and issues to look at the changes in a long term perspective. My aim is to fill in scenarios for future developments. Such an exercise in foresight will necessarily be speculative, and in places abstract. To contain the speculation, I will limit myself to an analysis of the *dynamics* of the changes, or more modestly, to one possible sociological approach to these dynamics. Having done this, I shall draw out the potential policy implications of the exercise by raising the normative issue that appears as soon as we understand where the transition is leading us: Do we *want* to go that way? What is a desirable future? I will also begin--but can only begin--to address the further question of *action*: Can we do something about our choice? As Leonardo Cannavó formulated it in a panel discussion at the Institute: Is there a meaningful space for science policy?

1. Approach to the dynamics of change

Again and again in lectures and discussions, we heard that there really are *no* national science policies: not in the pluralistic U.S., not in corporatist West Germany, not in quasi centralist France. There is no central actor who determines policy. Instead, "pushes" and "pulls" are

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exerted at various levels, which together *add up* to a science policy "after the fact." For example, in the U.S., the federal interest in university-industry linkages (an implicit industrial policy, as Henry Etzkowitz noted), and the university interest in the glamour of such linkages, plus local goodwill, plus (perhaps) additional income, together lead to changes in the way R&D is done, organized, and directed--as if there had been a policy working toward such changes. Such "push" and "pull" interactions, however, are general sociopolitical processes. They are, in fact, history in the making. Is analysis of such changes possible at all, and can policy practitioners learn something from such an analysis?

The Advanced Study Institute reported in this volume tried to create an approach that is at least heuristically valid. We took as our starting point the idea of the research system, a system with its own dynamics of development but one that is definitely also context dependent. Then we asked about transitions in the system and its contextual relations, extrapolated to possible futures, and discussed science policy on that basis.

While there is no single and definitive analysis of the dynamics of the R&D system, the several contributions to the meeting do add up to a coherent picture. I shall start with Helga Nowotny's notion of the essential tension between the individual and the collective, played out differently in different periods. The issue of autonomy that recurs in our debates is not limited to science. Specific to science is the unusual preference for the *new*, which guides its search processes. This requires special selection processes: one cannot work with a simple market or selection by a patron himself, as would be possible for services that can be judged by users. In the case of science, when its products are primarily aimed at scientific audiences, the reward has to be in the form of recognition and reputation, and has to come from colleagues who are also competitors. These two features explain much of the social and cognitive dynamics of science.

Nowotny presented us with a two level scheme, where search processes ("researching" in her terminology)[1] are conducted in concrete practices, say in a laboratory or an institute, and the products of such practices are presented to the scientific community as audience, through conferences, journals and informal communication. Activities at the level of the scientific community ("scientizing") create resources for scientists, like recognition of contributions, scientific prestige, or attractiveness to students and postdocs. Thus they lead to the stabilization of knowledge (as evidenced by textbooks, and the occurrence of a paradigmatic approach in a scientific field).

To complete the analysis, I shall add to this scheme in two ways. (My additions will also take away the impression of autonomy, or at least of science functioning in a social vacuum.)

(1) The concrete institutions in which "researching" takes place should be filled in. It makes a difference for search processes, and the way these are linked up with other practices, whether they are conducted in a university, a government research institute, an industrial research laboratory, or an independent R&D firm or institute. The pattern of linkages, and thus opportunities for the interactions called "scientizing," will depend on the kind and mix of institutions in the scientific field.

Also to be added are the resources necessary to continue, and the resource mobilization strategies that research groups develop and follow. In addition, for individual scientists, professional careers are important; this part of the dynamic cuts across the institutions and adds specific reputational strategies (extending as far as working for a Nobel Prize!) to the mix. The notion of "scientizing," which is used to highlight the aspect of self-organization of science, must thus be expanded to include the need to "manage" all the local linkages to institutions, resources, and careers. This makes self-organization a much more precarious matter.

(2) A third level is necessary to analyze the dynamics: general niche building and resource mobilization in the wider world, not just in the scientizing world of scientific journals and conferences. If we wanted a label, we could call this "politicking." The level of "politicking" blends into the active mobilization of the environment by individuals and groups for their specific purposes, as discussed under (1). The difference becomes evident when we note the change of meaning of the term *scientific community*. At the third level, it no longer denotes colleagues and competitors who evaluate and use scientific findings, but rather a societal group that acts through its representatives in political forums, can lobby for its interests, and can feel threatened by overall budget pressure or negative public images of science.

In the approach set out here, the viability of the R&D system has to do with what happens at all three levels and how these fit or do not fit together. If their alignment is strained (for example, because available social niches for science are not compatible with the traditional goals of self-organized "scientizing"), this is not only a matter of concern, but also an indicator that a transition may be occurring. The desirability of a trend (or a policy, for that matter) could, in principle, be evaluated by relating it to alignments within the R&D system. One should be careful, though. The goal of a smoothly functioning R&D system is not necessarily the only, or even the main, goal. Transitions, while disrupting smooth functioning, may in the end lead to a better R&D system.

This is the question facing us--in Europe even more than in the U.S.--in one of the important issues mentioned earlier, the new role of universities and the market orientation of science in general. Existing, stabilized institutions at the third level, the institutions of state patronage of science, seem to be changing and giving way. This shift is partly due to internal developments: intersubjective criteria (systematic peer review) and quasi objective criteria (indicators of performance) have become important in allocation because of the pressure for accountability. But in part the shift has occurred because patronage is now combined with market-type allocation, which creates further strains in the system.

In order to understand what is happening in this and other issues, and to respond wisely, we should think of the three-level system approach as showing us what the game of science used to be, how new rules are emerging, and what the new game might be. Some parts of the game go back to the 19th century; so we must begin the exercise in foresight there.

2. A look backwards

Imagine a meeting like the Advanced Study Institute reported in this volume, but one hundred and fifty years ago (any year between 1830 and 1850 would do), and held, say, in Florence. One could not *speak* of a research system then: the word *research* didn't exist, nor the word *scientist.* But there certainly was a *transition.* Predictably, some were concerned, while others enthusiastically embraced what they thought was a new age of science.

Science was still dominated by gentlemen, but besides the academies, which were based on a privilege from the King or other patrons, there were now general associations (like the Deutsche Gesellschaft für Naturförscher und Artze) more fitting to a bourgeois society.

At the "scientizing" level, one could just see the differentiation into disciplines starting, including a rearrangement in which "pure" mathematics became independent, and mathematical physics joined forces with experimental physics. Epistemologically, the ideal of "world system," or grand theory, was given up, and "regional" theory development, separated out in disciplines, became acceptable--though not without struggle, as the German *Naturphilosophie* tradition indicates. Just emerging was the notion of "scientific method" as something to be articulated and taught.

At the "researching" level, an increasing number of people were able to *do* research in return for income: in chemical analysis, in working with instruments, in lecturing and writing books, and gradually also through university positions. So a certain amount of *professionalization* was being introduced into the search for "new learning" or natural philosophy. After 1850, disciplinary groups and professional societies became institutionalized. The emerging role got a label of its own: *scientist*. The distinction between basic and applied research was increasingly drawn, and was turned into institutional demarcation criteria and support for the protection of a core of "pure" science.

By 1870, the new profession had become sufficiently self-conscious to present itself as an important part of bourgeois society, and as an endeavor that deserved state support because of its rationality, progress, and service to humankind. Examples, all more or less at this time, are the Endowment of Science movement in U.K., the "politicking" of Pasteur in France, and the defense of pure science by Helmholtz in Germany. (Indeed, some of their arguments return in Vannevar Bush's 1945 report, *Science--The Endless Frontier*). Note that there is more to this change than successful lobbying of those speaking for science. States themselves were changing. They could use general science for reasons of prestige, and use "researching" for expeditions and other field work of the life sciences, for national laboratories, and for advice on public works and hygiene. In fact, by 1870 when the first signs of a welfare state emerge, the argument about the welfare of science became legitimate, and general state patronage was sought and won.

When science got *some* support, and thus also official recognition, a typical form of professional control--state-mediated control--could be exerted, even though in other ways science, not having immediate clients, was not like a profession (see Cannavó, this volume).

For my analysis of the new game of science in the late 20th century, it is important to note how contributions to science have become an argument for career promotion (especially in the academic world), for acquiring resources (from the state and from other patrons), and for establishing professional status. Henry Cavendish, in the early 19th century, had no need of publishing his findings, and after his death his notebooks turned out to contain discoveries that others had made in the meantime. The transition that occurred during the 19th century, which one might call the professional transformation of science, created a generalized *linkage* between contributions to science (when they were recognized as such), and the acquisition and preservation of resources for science (including legitimation). It was thus during this period that publishing became necessary for many professionals. The functional argument that publication is conducive to the progress of science neglects the fact that at the time it was established, it was a side effect of professional transformation. It is only in retrospect that one can point out the important function of publication for the advancement of knowledge.

The participants in our fictional 1839 meeting in Florence, of course, did not know all this. But they could see that things were changing, and they could try to understand the dynamics. With the benefit of hindsight, we can do better, by using some of the analysis of dynamics in the 19th century to look at our own situation. In particular, the *linkages* between internal and external activities, between roles and institutions appear to be important.

3. A first step: State patronage of science after 1945

The historical development of science in its context shows other transformations. Given the importance of state patronage, its further institutionalization through the emergence of funding agencies for basic science in all Western countries after the second World War is particularly interesting. This transformation is of interest not only for analytic reasons, but also because, as I noted above, some of our present day concerns are related to changes in this part of the system.

Originally, funding agencies were seen as external and directive. They were accepted (and in fact, sometimes lobbied for) only by "progressive" segments of the scientific community--that is, those segments that sought to profit from the new opportunities. Some features of government patronage that now seem obvious, like peer review of proposals, were at the time compromise solutions to get cooperation from "conservative" segments of the community. Peer review of proposals, in fact, is not only a form of quality control, but also a way of "dividing the spoils." Furthermore, seen from the point of view of the state, funding agencies that give out money without any specified product in return are contrary to all rules of accountability (although this is more true in the U.S. than in Europe, where patronage traditions are strong). Thus, the existence and functioning of funding agencies was precarious at first.

In contrast, they are now seen as an intrinsic part of the scientific community, which clearly has *captured* the funding agencies, at least in terms of their internal functioning. Indicative is Susan Cozzens's diagnosis of the U.S. situation (this volume), with the striking difference between the prevalence of preperformance evaluation, done by scientists, and the almost complete absence of postperformance evaluation, that might be done by others and thus threaten the hegemony of the scientists. In Ron Johnston's words (this volume), "Funding allocation is now deeply embedded in the social control and reward system of science."

For a time, this mutual differentiation of the state and the R&D system in the form of "captured" funding agencies created a stable alignment of the levels of the R&D system. In Europe, external stability was derived from the patronage tradition (compare the large fraction of government R&D expenditure devoted to general science) and reinforced by concerns about the national capability in science, while in the U.S. the Vannevar Bush/Endless Frontier rationale provided an umbrella legitimation.

During the Institute, lectures and discussions offered examples and analysis showing that this relatively stable situation has come under pressure, internally as well as externally.

One issue is that of Big Science, as discussed by John Krige,[2] where autonomy turns out to be an obsolete concept, and the concepts of niche-creation and linkages not only allow science scholars to describe the situation better, but are also descriptions that are recognized by the scientists themselves. On the other hand, the *image* of autonomy must be maintained to legitimate science as independent, and worthy of respect, and to ensure recruitment of enthusiastic students. Sometimes autonomy within institutions and with regard to resources may not be very apparent to the practitioners of science. But in some cases, where society is liberal with its resources (biomedical research in the U.S., for instance), the normative diagnosis might be that there is too much (strategic) autonomy, rather than too little, with respect to the overall direction of work.

Another type of pressure derives from new relationships with industry and with big R&D programs, initiated either by single national governments (or the European Commission), alone or in joint action schemes with other funders. Again, one should not be too hasty in concluding that the R&D system is under attack from outside forces. At the level of concrete resource mobilization, researchers are, as Björn Wittrock points out, eager to trade research results for research resources, whether this extends the "endless frontier" of science or not.[3] And this is not a new situation. Henry Etzkowitz (this volume) analyzes how specific financial constraints and niche building in the 1920s, 1930s, and 1940s created institutional forms and procedures for external relationships of universities. Now these institutional "matrices" are being filled in by the new entrepreneurial academics, and offer niches for the academic "quasi firms" (as he characterizes present-day academic research groups).

Big government R&D programs are a new phenomenon. They are state patronage, but now goal-directed and strategic, rather than mission-oriented as in the Apollo program (a U.S. program to put a man on the moon by 1970). It is interesting to see that these programs (as with the funding agencies that preceded them) are being captured, to some extent, by scientists--at least by the so-called progressive segments of the scientific community--in the definition and articulation of the programs, as well as in their implementation and evaluation.

But this is not just a repeat performance of what happened with funding agencies. The criteria, and thus in time also the practices, are different. Funding agencies and their peer reviewers have developed criteria for contributions to the advancement of knowledge and for originality--that is, in general for the promise of scientific quality that makes a research proposal fundable. Under the R&D programs, however, intended research must be shown to be promising for some sponsor or for a program goal. And because of the public or semipublic nature of the program funding, there is pressure to develop generalized relevance criteria, which can then also be used in other parts of the R&D system.

This is an example in which the rules of the game may well be changing dramatically--or better, one should think in terms of a new *game* that is emerging now, with some recognizable rules already.

4. The new game

Let us try to sketch the outlines of what the new game may be. At the micro level, a fruitful focus is on the rewards that move scientists. (Rewards include monetary resources, but sought for the opportunities these provide, rather than as private income.) This focus is particularly important in the approach laid out in this chapter, because rewards link "researching" and "scientizing" with external resources and institutions.

Many examples of the new reward structure are mentioned in the contributions to this volume. John Holmfeld identifies a key point: Publishing is not the only criterion in academic careers anymore. Institutions are placing more emphasis on the amount of external money a professor brings in. External funding is found materially as well as symbolically important, and is taken into account in job interviews and career decisions.

At first, this new emphasis is limited to local, institutional criteria. But if the phenomenon is widespread, a secular change occurs in which the norms of science become more like PLACE (to use John Ziman's acronym). The products of science begin to be defined as proprietary, i.e., related to employers or clients. When the new external linkages become institutionalized, however, and industry and other sectors linking up with fundamental science adapt to the situation (for example, by allowing some exchange and attendant reputation building among scientists), a "new cosmopolitanism" emerges that transcends particular local criteria. Reputation and status can then be acquired at the level of the cosmopolitan network, just as reputation and status in traditionally organized scientific disciplines can be built up at the field level only if there is sufficient exchange and mobility.

This is not idle sociological speculation. In the case of professional engineering, for example, engineering firms and their members often have high professional status in spite of the relatively private and proprietary character of their work. In these engineering fields, there is enough personal mobility, exchange at meetings, and comparison of performance (including promised performance, when proposals for a project are submitted and evaluated) to make reputation and status building possible. This implies that engineers can actively work to acquire such reputation, because it lends them engineering credibility that they can exploit, e.g., in further resource mobilization. In fact, there are even further similarities with traditionally organized scientific fields in the matter of "organized skepticism." In the new customer-contractor networks, as in the old colleague ones, the quality of products is evaluated by competent peers before clients have to use the products and take real risks.

Thus professional engineering has evolved its own kind of "scientizing" over a period of a century or so. A similar argument can be made for more recent developments, e.g., in expert advice on health, on environment, and on global issues. Here, it is understanding relevant to decisionmaking that counts. Some of the work (sometimes most of it) is not made public; and if it is distributed openly, it is often in reports rather than "regular" scientific publications (thus the increasing prominence of the grey literature). Still, there is recognition of performance, and accompanying rewards in terms of resources and careers. So scientists can (and will) move in this direction. Here, too, one can see a "new cosmopolitanism" emerging, through hybrid institutions like mixed scientific-policy conferences and organizations like the International Institute of Applied Systems Analysis near Vienna. These hybrid forms create career resources and mobility. In this way, the contributions of experts are visible, and a functioning reward system can evolve.

I can summarize my argument to this point with two observations. One is that autonomy (tactical or strategic), although much debated in some quarters, is not a key issue; what is important is the structure of the reward system and the subtle changes it is going through. Second, new organizations, institutions, and relationships allow the emergence of nontraditional reward systems, while their establishment allows new institutions to be integrated into the R&D system. University-industry centers, for example, may evolve from purely local institutions into *sites* where reputation and status at the field level can be acquired. Or, as government R&D programs become a permanent feature of the R&D system, participation in them will count as important in the *vitae* of a researcher. "Expertise brokers," exemplified by engineering firms, may appear in more and more areas, as they are doing in the environmental area already.

Sketching the outline of the new game at the micro and meso levels does not relieve us from considering normative issues, in particular the question of what all this implies for quality control mechanisms. Is peer review obsolete, or should one re-create the disciplined behavior of the good old days? Marcel LaFollette points out that the criticism of peer review is really scapegoating, since the problems are systemic, i.e., related to a transition in the R&D system.[4] The systemic perspective also allows us a fresh approach to the issue of copyright and other forms of intellectual property. The experience of engineers and their right to designs (e.g., when submitted in a proposal but not honored) must be relevant, given the structural equivalence of their situation with the newly emerging reward systems.

5. A new role for universities in strategic sciences?

There is another important aspect, which is specific to the meso level. The pattern of institutions, and interactions between scientists that are allowed by these institutions, creates possibilities (and of course also constraints) for *coordination*. Scientific disciplines as concerted efforts are possible only if there is coordination; Lakatosian research programs and Kuhnian paradigms are examples of coordination. They are, in fact, the tips of a sociocognitive iceberg, and neglect the social and institutional infrastructure that enables and constrains coordination.

The phenomenon of coordination is not limited to scientific disciplines. For example, there is (agonistic) coordination between high-tech firms about their strategic positioning in scientific-technological areas; in fact, participation of firms in the new government R&D programs is a way for them to get information about competitors and find mutually workable positions. Coming from the other side, research groups also find it important to participate in R&D programs to position themselves with respect to other research groups in the same general area.

So coordination now occurs in the R&D system, not with respect to some fundamental research front, but with respect to research fronts defined in terms of strategic societal goals. The concept of *strategic* science has become popular in many countries (see Barré and Johnston, this volume), and is a label that resource-mobilizers try to attach to their proposals because it gives them visibility and priority. The argument given here suggests that strategic science should not be seen as denoting a particular type of research--(disciplinary) science with long term relevance to important economic or social goals--but as a *site* for coordinating strategic action, cognitively and institutionally, among relevant actors. The experience of the British Alvey program (see Rip, this volume) is not unique. Here it turned out that the process of working together (both for research groups and firms) was more important than the specific products of the program.

The emphasis on coordination, and on the networks or other sites that enable coordination, also has implications for how we view universities and the new role they can play. Henry Etzkowitz pointed out that universities (and parauniversity institutions) can be *sites* where the scientific-technical linkages that have become strategic to firms *and* other actors, can be maintained and can function in relative independence from any particular actor. He added that this is a specific advantage of universities. But as soon as the notion of *site* is introduced, there is no reason in principle for such a function to be limited to universities. This is not only because scientific research is (and has always been) broader than university research. (The central role of universities in the production of scientific knowledge may thus be a historical accident.) It is also because if we see strategic science as a label for sites and opportunities for coordinating strategic action, a variety of institutions can compete with universities for that central place. Then the proliferation of "centers" connected to universities, of parauniversity institutes of different kinds, and of institutes fully outside the university system (like the Wissenschaftszentrum in Berlin) becomes a significant phenomenon.

In other words, it may seem that universities are only creating new linkages for themselves, e.g., with firms. But in fact they are competing with other institutes in the new "market" of strategic science. Their specific advantage may still be that they also do advanced training--but even there, as Douglas Hague points out in his contribution to this volume, competition may come up. (A shift of this sort cannot be dismissed, even though earlier ideas in this direction, e.g., scientific training in industral firms, or through a consortium of firms, have come to nothing).

Of course, universities are not limited to the function of competing on the "market" of strategic science. But from the perspective of the R&D system, the mere existence of this function is a striking development, and a possible indicator of the direction of change in the new "game." Clearly, legitimation of basic science in terms of state patronage free of relevance criteria has lost its force. Whatever new social contract emerges will be accompanied by a reshuffling of institutions and their functions. This point is not limited to the role of universities. National laboratories, for example, are also becoming more active and may well take up new positions, depending on their ties to government, but also depending on the opportunities offered by the changing R&D system.

At this macro level, at least one other indicator of systemic change can be noted. The position of science policy at national and international top level decision making is changing again. After the institutionalization of science policy in the OECD countries in the 1960s, in every self-respecting nation, science policy was present at the Cabinet level (in the U.S., with a President's Science Adviser in the White House). The 1970s were a period of reconsideration; but the 1980s showed a revival, with science and technology actually being discussed at meetings of heads of state.

What this implies is that new sociopolitical linkages are being secured at the top level of the R&D system as well. Support for science and technology may well continue and even grow, but this will always be in relation to sociopolitical goals and control structures. Thus, after the "endless frontier," science need not be limited to a "steady state"; the new game might be better captured by a label like "science under scrutiny."[5]

At this level, the analysis should also take into account the state and its transformations, as was exemplified by the discussion of the late 19th century development of state patronage for science in relation to the welfare state. This aspect of the dynamic is often neglected in science policy analysis, probably because the focus of science policy decisionmakers, and thus of science policy analysts who identify with their problems, is so focused on the specific issues of science.

6. How to develop policy implications

In a general exercise in foresight, drawing out concrete policy implications is of little use, but some general considerations can be given to guide further thinking. One point highlighted through my R&D system approach is the *ambivalence* of science policy. Is its task to identify, and then follow trends, and make them come true as it were? Or should it try to make room for policy, and change trends? Actual policy practice often amounts to following trends, because national governments tend to shape their policies in response to what other nations are doing (or what they think is being done), and then take the politically safe decision to imitate, rather than strike out on a new path. The Alvey program in the U.K., and similar programs in other countries, were reactions to Japanese programs. They were therefore set up in such a way that they reinforced trends in computing, artificial intelligence, and VLSI (Very Large Scale Integrated circuit) technology.

But governments and other policymakers who want to lead instead of follow, must still take all sorts of constraints into account. So an active "shaping of the future" must often be limited, and will reduce to the exploitation of opportunities as they occur--incremental policymaking, but (hopefully) with long term goals in mind.

Keeping in mind the irreducible ambivalence of science policy, some way must be found to evaluate trends and set goals. There are three ways this normative part of science policy can be articulated.

(1) The articulations are based on the responsibility of a specific actor: a Minister for Science Policy, or the Board and staff of a Research Council, etc. Such actors have to make difficult choices, and will want to draw on their "charters" and on enlightened self-interest, i.e., long term survival goals. Whether the "charter" should change, and how self-interest had best be defined, are metaquestions that should be answered with the help of the kind of analysis of dynamics I have given above.

Part of the problem is that organizations must cope with a turbulent environment. For actors within the R&D system, like research groups and funding agencies, the R&D system is the primary environment, and analysis of the R&D system is immediately relevant. It is often possible to develop policies of accommodation to the existing situation, and create some room for renewal at the same time. But the "charter" of a Minister for Science Policy is different. Besides his or her responsibility for the functioning and productivity of the R&D system, a prominent concern will be the linkages between the R&D system and other sectors.

In the new game that is emerging, the political and symbolic importance of such linkages is high, but not always supported by adequate understanding. The relation between science and technology, for example, can certainly not be described by the linear model; Keith Pavitt (this volume) quoted Price's metaphor of "dancing partners" instead. Still, the linear model, where science leads to technology, drives policy measures--because the corresponding demarcations are built into the institutional setup, in the same way that the 19th century distinction between pure and applied science is maintained because of the way it is institutionally embedded.[6] Thus, for this kind of science policy actor, the environment is larger and more complex, and less is known about relevant interactions and dynamics.

(2) Normative articulation of policy can also be derived from values that should be preserved, or achieved. Such values can relate to central aspects of the R&D system, for instance, to the human or civilizing value of science. They can also relate to actors and institutions in the R&D system. One example would be the value of a critical role for the university, which should be set against its activities in strategic science discussed above.

In the discussions during the ASI, it was clear that such a critical role should not be taken at face value. For one thing, institutions are never critical, but they can provide individuals with a space to be critical. In developing countries, the critical role of universities and their function as a haven (or breeding ground?) for critics, may be very important. In Western industrialized countries, this function is often taken for granted, and neither articulated nor exploited. This situation may change, however, when the competition for strategic science creates counterreactions that are more than a conservative wish for a return to the good old times.

(3) Articulation of policy can also be derived from the importance of alignment within the system; this is a generalization of the idea of "management" of the R&D system, which should be thought of in dynamic terms.

One starting point would be to look at the problems that arise. For example, publication of scientific findings by press conference, instead of first in a scientific journal (after peer review), occurs more frequently now, and with explicit connections to resource mobilization. There is criticism of this practice, but forbidding it does not help when the problem is really the strain between the old "scientizing" level and its relation with "researching," and the "real world" of researchers competing for public attention and attendant access to resources. So it is a systemic problem, and new forms of "scientizing" must be developed. Who knows? Perhaps these will even include quality control by the media.

One can also start from systemic analysis. The discussion of funding for science and of allocation procedures is linked to the fact that funding agencies, research councils, and other institutions are "on the move" in the R&D system. (This includes the U.S. National Science Foundation with its hope for a doubled budget.) These are *intermediary* institutions, because they (and the functions they fulfil) create links between levels of the R&D system. They will therefore experience alignment problems acutely, and can be used as an entrance point for analysis and diagnosis. In addition, since their emergence and further evolution has helped to solve alignment problems, one can also design therapy in terms of further evolution of intermediary institutions.

Systemic analysis can be taken further, for example by considering that actors at the "top" of the system have a different relationship with the environment (see (1) above) and are freer to look around. So they engage in foresight exercises, or profit from their discretion as political decisionmakers. At the bottom of the system, actors can work from enlightened self-interest in their niche-building strategies without getting into legitimation problems.

7. An agenda for science policy research

In the course of the Advanced Study Institute, many critical and constructive suggestions for science policy research and analysis were made, as the preceding chapters attest. In this section, Susan Cozzens and I make some general comments, based on the analysis in this chapter and on contributions in the final session of the conference.

Non-decisionmaking--what doesn't get on the policy agenda--is present in many areas of political life; science policy is no exception. Political scientists use this term to describe the active efforts of groups with special interests to suppress certain issues, but it could also describe issues that are neglected inadvertently, through a strong focus on something else. Science policy research should be concerned, in part, with drawing attention to non-decisionmaking in science policy. The kind of analysis represented in this volume can help identify important topics that are not yet on the table as action items.

Standard rhetoric, for instance, can often blind us to real issues. One example is the defensive tone in much of what is said about "science under scrutiny." Will science as we know it be lost irrevocably because of the changes in the game that we are witnessing? How can we save science from this fate? Such blanket indictments of change do not help very much, and may actually hinder understanding, including understanding of issues that are dear to the critics themselves. With new rewards for research, and new forms of intellectual property and intellectual exchange, there may indeed be a danger to freedom of science or to academic freedom. But rhetoric alone will not be enough to save it. An effective defense must rest on a thorough analysis and theory of academic freedom, and an examination of exactly how and where it is threatened. Without such analysis, we have no way to distinguish surface concerns and self-serving protestations from substantial issues.

At the meso level, the rhetoric of the linear model for the relation between science and technology (and probably also for expertise and decisionmaking) may be equally misleading. For one thing, in a particular area of science and technology, there may well be a derivative relationship from science to technology. But when the linear model is applied across the board, we will never find out where it holds and where it doesn't. Successes and failures will be ascribed to other factors instead of to a failure of understanding. In fact, the economics of technology and innovation has, by now, a lot to offer; the problem seems to be that policy based on such insights is hard to formulate and defend.

At the micro level of "researching," one barrier to understanding and wise action is the rhetoric that good science can only flower under special sociopolitical conditions. If for a moment we take the notion of "open marketplace of ideas" to summarize a cluster of ideal practices like those proposed by John Ziman--fostering individual creativity, space and time for ideas to grow, hospitality to novelty, openness to communication and criticism, respect for individual and collective expertise--then we quickly realize that this "open marketplace" has been facilitated under diverse institutional forms: in priestly hierarchies and in groups of aristocratic amateurs, in guilds, in state bureaucracies, and in industrial corporations. So we cannot say, "Let science self-organize, and everything will be right!" We have to find out empirically what kinds of practices hinder and frustrate good science--taking into account that what is *good* science changes over time and across disciplines, and will to some extent be defined by the institutions that have emerged.

Finally, when the relation between the R&D system and society is phrased in terms of a "social contract," for example according to the "Endless Frontier" rationale, this rhetoric, too, may hide basic political issues. According to political theory, a "social contract" is concluded between governed and governors. In this example, however, science promises to deliver goods to society in return for patronage without strings attached, so the contract partner "society" is left out. The 1970s and 1980s have seen a variety of interventions by and interactions with this contract partner, who did not want to be left out anymore. By now, there seems to be a balance of opinion that too much public intervention and control would not be desirable either. But how much is desirable is decided pragmatically, in spite of the fact that political theory should be able to articulate some basic principles. There is no reason in principle not to apply political theory here. Why should issues of distributive justice, public good, and democracy not apply to the domain of science policy?[7]

There are many who fear, not the intrusion of any social influence, but rather the specific instrusion of private industry into the direction of science. While science is publicly supported, this argument goes, scientists feel obligated to serve the public interest; but when private support enters, the public loses any chance of influence. This rhetoric, of course, ignores the very low level of true democratic control of publicly-supported science now. But it also directs attention away from the opportunity provided by the growth of university-industry relations, opportunity to learn about effective ways to use scientific knowledge for publicly-defined practical ends.

The key question is perhaps not that of ownership, public or private. After all, the problems of industrial application arise in economic systems as diverse as China, Sweden, West Germany, and the U.S.. The key underlying problem is how to mobilize expertise in complex organizations for social problem-solving. Current university-industry relations can be used as a laboratory to produce general knowledge about that problem. The trial-and-error involved in incorporating industry into foresight and evaluation processes can also be used in this way. When we know more about how industry can participate effectively in decisionmaking for science, we will also know more about how other interest groups can do so. Used in this way, current experience can help to increase the chance of true democratic control of science--or, to translate into a different rhetoric, to keep science in the service of the public.

A similar learning opportunity is provided by the current trend toward internationalization. In spite of the attention it gets from policymakers, internationalization as a phenomenon is severely underanalyzed. What are its dynamics? What scenarios are open for its future? It is clear that science policy can no longer be just national; but there is little attempt to face the challenge of working toward an international science policy. Harvey Brooks (this volume) lays out some directions, but his remarks remain visionary. If one looks at actual sites and movements of actors--not in the least multinational companies and other transnational patrons--it is clear that new patterns are emerging, both at the level of the reward system of the scientists (see above) and in terms of setting directions for science. This development may well overtake national science policies, which will fall down like a house of cards. Policymakers may see this coming, but do not know what to do, while science policy researchers have only just started to identify the problem.

At present, as John Krige points out (this volume), international collaboration is established to serve national interests. Yet despite their origins, international organizations tend to create their own momentum after a while. What can we learn from those experiences about how to build transnational structures that put science in the service of the global community, rather than national ones?

There are, of course, many other opportunities for science policy research as well. We have not tried to present an exhaustive list, but rather to point to issues that were prominent in the discussions on which this volume is based. Any actual policymaking can be treated as quasi experimental, and analyzed systematically (if science policy researchers are willing and policymakers are prepared to give them access). Unfortunately, the sometimes formal but mostly informal learning that occurs in the practice of science policy tends to get lost. Methods should be developed to reconstruct it and make it more widely available.

At the most general level, science policy practitioners and science policy researchers could join in a learning process about science and policy. The process would be conflictual, full of lacunae and wrong turnings, but it would inevitably show some progress. Exercises in foresight would be an essential part of this learning process. Science policy researchers may analyze trends and correlations, but in foresight exercises they also take, and have to take, the risk of coming up with propositions about possible and desirable futures. To contribute to the general learning process, they have to speak out and interact. In a sense, being a good analyst requires one to be a political actor as well.

This chapter, in its attempt to combine analysis, scenarios, and ways to address normative issues, has profited from the way teachers and participants in the ASI spoke out and interacted, often with no clear distinction between researchers and practitioners. Part of the learning that occurred this way is reflected in my personal synthesis in this chapter. Indeed, this is just the type of learning that has to continue in the "real world" of science policy.

Notes

1. The terminology, and elements of the approach, are derived from Krohn and Küppers (1989).

2. See Krige in "Organizational Roles and Individual Autonomy in Science," this volume.

3. See Wittrock and Elzinga (1985), p. 16.

4. See LaFollette in the panel "Peer Review in Evaluation," this volume.

5. The label was introduced, with a similar argument, by Guston (1989). The thrust of such a label, however, is much wider: think of controversies about expertise, and debates about biotechnology and about science and national security.

6. It is interesting to speculate what will happen with scientific expertise, e.g., in relation to government regulation and to environment and global problems. Expertise used to be seen as relating to individual scientists, but new institutional forms and relationships are now emerging (e.g., environmental consultancy bureaus). The dominant legitimation, however, may well remain that of a linear model, where expertise shapes political decisions, instead of a more realistic interactive model.

7. There is clearly a lacuna here, and it is, in fact, part of a larger "blank space" in science policy research: the potential contribution from political science.

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