Chapter 9:

MAPPING OF SCIENCE: POSSIBILITIES AND LIMITATIONS

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Summary

Possibilities to map scientific fields and developments in science have been developed by scientometrics, and are becoming increasingly important for science policy in a strategic age. Technical and conceptual issues in constructing and using maps are discussed, and the relation to policy goals and utilization is emphasized. A comparison with the development of environmental mapping and impact analysis allows some further critical reflection on the status and policy role of maps of science.

1. Introduction

There is a lack of systematic, independent data characterizing current activity in science in a form that can be used by policy makers, and bibliometric 'models' or 'maps' of the literature output of particular scientific fields form a particularly promising possibility to overcome the lacuna. This diagnosis has been the starting point for the British Advisory Board for the Research Councils to explore and compare methods of science mapping; in the report, the use of such maps to evaluate alternative investment possibilities is stressed (Healey, Rothman, and Hoch 1986, esp. p. 234-235).

The possibility of producing 'maps' of science has been noted before, and ways of doing so were elaborated. Garfield has stimulated the development of techniques to exploit the data base of his Institute for Scientific Information also for the production of maps of science, and, eventually, an "instant history of science" (Garfield, Malin, and Small 1978, p. 189-193). The notion of mapping, according to them, implies

dealing with objects or entities that have a location in a space of some number of dimensions in which the distance between objects is meaningful and well-defined. Mapping science is an attempt to arrive at a physical representation of fields and disciplines - and, at a lower level, of individual papers and scientists - in which the relative locations of entities is depicted (*ibidem*, p. 192).

In spite of the emphasis on distances and their depiction, the authors realize that one can talk about maps also on the basis of measures of association without assuming a metric space, while depictions need not be of the "snapshot" kind, and may even be like mental maps (*ibidem*, p. 193). Such a "qualitative" or non-metric approach has been developed by Callon, Law and Rip (1986) on the basis of co-word analysis, in which the maps are primarily visualizations of network structures. Finally, bibliometric data are not the only possible building blocks for maps. It is possible, for instance, to aggregate cognitive-psychological data to give impressions of distances between concepts common in a particular field of science (Hagendijk and Cramer 1986). From these examples it is clear that mapping of science is being pursued in a number of ways. Sometimes in order to study science and its developments, in other cases with clear policy uses in mind. As a general definition, I shall take a map of science to be the visualization of the topology of relationships between elements or aspects of science. The basic data may have been produced for other reasons or are specially gathered to create "invented" indicators (Elkana et al. 1978, p. 3). The maps, however, are always constructed by the analyst. This raises issues of the validity of the representation and the occurrence of artefacts. I shall discuss these problems in section 3, before going on to the policy uses in section 4. In order to put the discussion in perspective, I shall first trace some developments in science indicators.

2. From performance and quality indicators to strategic mapping

In the "golden age" of science policy, the decade 1957-1967, the main problem appeared to be how to allocate the ever growing science budgets in a sensible way. Criteria for choice in science were discussed (Shils 1968), and structures for science policy making at different levels were set up. With the institutionalization of science policy and the routinization of science policy practices (cf. Brickman and Rip 1979), the attention shifted to issues of management of the national scientific capability. This focus was reinforced by the reduction in government spending on basic research and on R&D in general. Science and technology indicators should tell the nation about the "health of science". The US series Science Indicators,

produced by the National Science Foundation since 1972, has consistently taken this perspective, and the recent report by the Science and Engineering Policy Studies Unit of the Royal Society of London had to take up the Advisory Board for the Research Councils' request to measure the health of basic sciences in the UK.

As the Royal Society report notes explicitly, unpacking the concept of health is in practice reduced to measuring performance and identifying national strengths and weaknesses on a comparative basis (ABRC 1986, see also Smith, Collins, Hicks and Wyatt 1986). This type of operationalisation is widespread, not in the least because science and technology indicators tend to function primarily as part of a research management information "system" at the national level. Striking comparisons and changes over time will be an occasion for further inspection and debate, and sometimes decision making, rather than that health or quality (in the full sense of the word) is being measured. In so far as science and technology indicators are a decision support tool, it is primarily for decisions on budget cuts, selection and concentration operations, etc., and at the lower level of research organisations, to support research career and funding decisions. For a research management information system, rough indicators are often enough, and to legitimate eventual decisions, it is often necessary to include peer review in addition to bibliometric measures of output.

The availability of rough indicators for the national capability in science does not imply that one should be satisfied with present data bases and the reliability of available methods. Input data are not easy to get (cf. Irvine and Martin 1986) and throughput data, linked to processes within the science system, are sorely lacking. The point is rather that in terms of performance and quality indicators, some stability has been achieved: notions of what they are and how they can be used, i.e. "customer concepts", have been articulated, and "production methods" are available to meet this demand (Rip 1987). Since the early 1980s, strategic science policy making has become increasingly important, with concomitant shifts in the demand for science and technology indicators. National strengths and weaknesses remain important issues, but not just in the debate about decreases and increases of funding. They are now the occasion for specific priority setting and programming, at the national level, as well as in big research organisations and science funding agencies. More detailed indicators are now necessary, and they must also be structured in relation to strategic policy questions. Maps of scientific fields, once curiosities of interest to science scholars and bibliometricians only, are now becoming decision support tools. Bibliometric models based on co-citation analysis are used as sources of structured information in many countries, including USA, FR Germany, Spain and Australia (Franklin 1987, Franklin and Johnston,

this volume). They have been used to operationalize national strengths and weaknesses as contributions to the co-citation clusters (Van Heeringen, Mombers, and Van Venetie 1984, Mombers, Van Heeringen, Van Venetie, and Le Pair 1985). The US National Science Foundation is exploring the possibility of making bibliometric model data available on the desks of their program managers that oversee project selection and performance (study by the Center for Research Planning, Philadelphia; quoted after Franklin 1987, p. 1).

The same strategic interest in science policy has also led to a demand for "early indicators" (indicators that show up trends in an early stage), and for identification of emerging and promising or "exploitable" areas of science. Maps of science may be helpful here, sometimes even a necessary precondition. Co-citation maps have been put forward as a basis for early indicators, but their foundation on citations to the literature introduces a retrospective bias. Co-word maps reflect the structure of the research front directly, but the difficulty in interpreting them has made their strategic use a promise, rather than an accomplishment (Bauin 1987).

What used to be a scientometrics or bibliometrics push (for instance, the Institute of Scientific Information pushing co-citation maps), is now met halfway by a policy demand, as is visible in the increased interest of policy makers to interact with scientometricians and other science scholars. Bibliometric maps are still the dominant offering, but there is more variety -- e.g. also journal-journal network maps (Leydesdorff 1986) --, and there is increasing interest in interactive mapping, where users can ask for further details, specific cross-sections and actors and strategies behind the maps (cf. Callon, Law, and Rip 1986). The whole field of science and technology indicators is becoming more sophisticated, and more resources are invested in it. Whether this development will lead to substantial progress, or just to proliferation of certain techniques (each country wanting to have its own co-citation map) and fragmentation, is a question that certainly deserves attention.

3. Possibilities and limitations of maps of science

Instead of reviewing the strong and weak points of specific mapping techniques, I shall give an assessment, based on the notion of mapping in general, and on the possibilities and limitations implied by using different kinds of data as building blocks.

3.1 Technical limitations

The nature of the database used to construct maps is obviously very important for the value of the final product. Apart from the two main categories, data bases using keywords, and data bases using citations and other bibliometric linkages, there are finer distinctions, e.g. national, linguistic and/or disciplinary differences, and the possibility of creating specialized data bases for policy purposes (cf. Van Raan 1986 for an example). The latter possibility must be considered seriously in the case of technical fields (and applied research), because publications are not the main output of research in such fields. The effect of differences in databases has been explored in the ABRC report (1986, pp. 108-112) and by Barre (1986). The latter distinguished 16 domains of science, encompassing 107 specific themes, and made comparisons of the results of using different data bases. In the "scientific" fields (like physics, chemistry, biology), outcomes were similar, while more applied fields diverged according to the national or linguistic bias of the data base. The data in the ABRC report indicate that within scientific fields, physics is more "international" than biology is. The data bases of the Institute of Scientific Information (Science Citation Index) focus on "scientific" fields, and offer good access to current international science (even if developing countries, and peripheral domains in general, feel that they are underrepresented -- but this is more a problem of being on a periphery, than a particular bias in the data base).

Given a particular, and reasonably satisfactory data base, there is also the issue of the quality of the data in the data base, especially their completeness. This issue has been debated extensively in relation to performance and quality indicators, because incompleteness (due to various possible errors) can have important consequences for the judgement of performance/quality of an individual or a group. Examples of such errors have surfaced (Moed et al. 1985, also Hicks 1987). In addition, definitions used in the database may not correspond with the purpose of the indicators, for example the definition of what counts as a publication. In practice, 95% coverage, or, equivalently, 5% or less errors, is taken to be sufficient. This is a pragmatic decision, given that most data bases do not offer higher accuracy, and as the Leiden Indicators Project (Moed et al. 1983) has shown, the costs of improving accuracy, in their case to 99%, are prohibitive for routine applications. The ABRC report (1986, pp. 147-149) reaches the same conclusion. Although Moed et al. (1985) propose a standard of 99% for evaluation of small group performance, even this is no guarantee that there will be no mistaken judgements. For maps of science, however, the main thing is that global trends are represented accurately. 95% coverage must be sufficient for that purpose. Only in the case of interactive use of maps, when one wants

to go down to the actors behind certain trends, the results should not be taken at face value. Checks with other methods have to be made before a decision is taken.

A very important, but until recently neglected issue, is the quality of the techniques (statistical and other) to construct maps out of the data. In spite of the sophisticated statistical methods that are being developed (e.g. Tijssen et al. 1987), techniques that have very real flaws continue to be used without reflection. The possibility of artifacts is a continual danger, for example in multi-dimensional scaling techniques, where the sum of squares between multi-dimensional distances and the projected, two-dimensional distances is minimized. Even with low stress factors, the nearness of two specific points cannot be taken as the immediate reflection of their nearness in multi-dimensional space. Another problem is that the single-link clustering method used by the Institute of Scientific Information will often create a "garbage can" cluster, because linkages (above the threshold) must be used, whatever their weight. Leydesdorff (1987), who has pointed out this problem, prefers to use factor analysis and dendrogram representations. Apart from the statistical problems, there is also the question what the nature of important linkages between scientific articles is. Callon, Law and Rip (1986) argue that linkages in science are chain-like, e.g. going from one keyword, or one literature reference, to another. Courtial (1986) has shown that a factor analysis of structures of keywords cannot reveal any pattern, while both inspection of the articles, and co-word analysis, show up series or chains of linkages.

Even with adequate statistical techniques, a number of somewhat arbitrary decisions have to be taken: stipulating thresholds to filter out noise (e.g. only often cited papers, or words occurring at least x times), and using indices (again with thresholds) to highlight significant linkages. Both types of decisions have effects on the maps.

For instance, on the early co-citation maps of science, mathematics was not visible (e.g. Garfield, Malin, and Small 1978, p. 192), because the amount of referencing in the mathematics literature was too low to pass the thresholds that were set to keep the biomedical literature (with heavy referencing) manageable. More recently, fractional citation counting allows inclusion of small fields besides big fields, and variable level clustering (adjusting the co-citation threshold to an "optimal" size of the clusters to be found) avoids unnecessary fragmentation and the appearance of global, "garbage can" clusters (Small and Sweeney 1985). With the additional refinement of iterative clustering, mathematics can now finally be made visible on the map. (Small, Sweeney, and Greenlee, 1985, p. 334) Such improvements have real value for the work of interpretation

of the maps, but also introduce a new kind of arbitrariness, which will have to be taken into account. Co-word analysis suffers from the same problems of threshold setting and the separation of signal from noise, but variations can be tried out more easily and probably less expensively. According to Oberski (this volume), including linkages just below threshold can change clustering drastically in co-citation analysis; Hicks (1987) gives an example of a spurious link in a cluster. For co-word analysis, Rip and Courtial (1984) performed sensitivity tests to check for such possibilities.

The problem of interpretation, although not purely technical, is closely related to some of the technical issues: the more pragmatic decisions go into the construction of the maps, the more difficult it becomes to have a direct interpretation of the results. Validation by experts is tried (e.g. Healey, Rothman, and Hoch, 1986, p. 241-247) but is itself difficult to interpret. For one thing, experts evaluate interest and relevance rather than representational validity: "A paradox exists in the validation of science policy indicators. If the results of the work are counterintuitive to experts they are considered invalid; if the same as their usual intuitions, they are considered valid but uninteresting -- they reveal only that which is already known." (ibidem, p. 247). One could, in fact, take a further step, and argue that maps aggregate data about scientific fields in a way that no individual expert, with his or her own background and perspective, would be able to do, and may not even recognize. Non-recognition by experts undermines the legitimation of the use of maps in science policy, but may actually be taken as an indication of the value of constructing such maps! This point holds even when a group of experts in the field is questioned, unless procedures of interaction and aggregation are followed that are more or less equivalent to the steps in constructing the map.

3.2 Conceptual and theoretical issues

What do maps actually represent? To answer this question, one has to understand not only the details of the construction of the maps, but also the nature of the elements or aspects of science used as building blocks, and the way these function in the dynamics of scientific developments. This, obviously, is a major question; social studies of science are only recently making progress in this direction. Limiting myself to co-citation and co-word maps, and comparing them with the traditional, cognitive, way of charting scientific fields and their course of development, some first insights can be outlined.

The basic point to be made is that representations of scientific fields, as they occur "naturally", for example in review articles, or in the

introduction to a research article or a research proposal, do not picture a state-of-the-field, but are accounts of it, as it were the story about it as the authors want to tell it. Such accounts are directed to specific or general audiences, and aim to enrol the reader in the perspective on the field that the author proposes. But the author cannot push whatever perspective he wishes. If he departs too far from what others perceive as relevant in the field, he loses credibility and impact. In order to create a plausible representation, the author will include material that readers will recognize and be convinced by. Thus, referencing is a form of persuasion (Gilbert 1977), and there is now agreement on this point, even if some authors point out that it may not be the whole story (e.g. Small 1978, see also Cozzens 1981 on citations as rewards, as persuasion, and as cognitive symbols). So, citations to important publications, as well as the deployment of key words common to the area are means to exert force on the reader, and building blocks of accounts that can be taken over by analysts to use in their accounts of scientific fields. (There are other building blocks of scientific articles that cannot be used in this way, e.g. conventions to be followed, for example general rules like the avoidance of personal comments or the pronoun "I", or area-specific rules like using a particular notation.)

Both citations and shared key words are linkages between articles. Thus, aggregating their occurrence over a body of articles will provide access to the network structure of the scientific field at a particular moment. This can be done by scientometric techniques, if a body of articles has been delineated. Review articles also aggregate, in a qualitative way. However, such articles rarely discuss the linkages between articles in the field explicitly. This is related to a general feature of scientists' use of building blocks and linkages in their accounts. Words are taken to stand for natural or experimental entities and their properties and relations, and citations indicate the force behind the statements. (In the humanities, it is much more common for an author to discuss the literature and its evolution, sometimes as a topic by itself.) Thus, a difference emerges between scientists' accounts of the field, which appear to offer a structuring of a natural or behavioural/social reality, and the aggregation of data that is the basis of an analyst's map, where network relations between building blocks of individual accounts are presented explicitly. This does not imply that the analyst necessarily creates artifacts. On the contrary, they make visible network relations (like often cited or co-cited articles, important key words and their co-occurrence) that already exist, and are used as resources by scientific authors in constructing their own accounts, even if they do not present the network relations as such.

The networks that are produced in a mapping exercise should not be taken as the representation of a scientific field "as such", nor as just

reflecting the accounts given by scientists, but seen as constructions in their own right. Their link with ongoing science, and their value, consist in the way they relate to (building blocks of) scientists' accounts, and in the aggregation of data that occurs and creates a view of outcomes of scientific accounts at the collective level.

The nature of these constructions depends on the features of articles, the building blocks of the scientists' accounts, that are chosen as the raw material. A co-citation map brings out relationships between articles published in year t , by depicting the pattern of linkages between heavily cited and co-cited articles, themselves published in earlier years. One view of what such a pattern represents has been offered by Small and Greenlee (1980, p. 287): it is a snapshot of the mental furniture (at time t) of a hypothetical and ideal researcher in the field. Citations, according to Small, must be seen as standard symbols, relating, in principle, to the concepts from the cited article that are used in the citing article. Small has elaborated this point by checking citation contexts (Small 1978, Small and Greenlee 1980). A co-citation map must then be interpreted as presenting the symbols for the central concepts of a field in year t (insofar as these can be captured through reference to earlier articles).

Further consideration is necessary, however. Looking at maps of a field in successive years, it turns out that many changes occur (Small 1977, Small and Greenlee 1986). The occurrence of (sometimes disconcertingly large) changes can be explained partly because the central concepts evolve, partly because the fashion in citing articles will change, while there is also a requirement of citing recent literature. However, the co-citation maps do not capture the full network in year t : the articles that cite into the co-citation cluster are often less than 50% of all the articles in the field. Before interpreting the map, it would be important to know (i) which articles are "lost", (ii) what citations-as-concept-symbols the "lost" articles use, and (iii) whether they can be seen as peripheral in some sense. Only then, the relation between the field and the co-citation cluster can be specified sufficiently to interpret the maps.

The general recognizability of the co-citation clusters to scientists in the field cannot be a blanket argument that there is, in fact, no problem. Citations, especially citations to often cited articles, should primarily be seen as having the function of legitimating the account of the author. Thus, in contrast to Small's view, a co-citation cluster will represent the legitimacy repertoire of the field, rather than its central concepts. (Small (1978, p. 339) comes very close to this point when he notes that citations serve as a kind of language system.) Scientists can recognize this repertoire, as represented by the co-citation cluster, because they use it themselves to give an account of their field to students and (relative) outsiders: they introduce the

exemplary achievements with a brief (and compared with the actual history, retrospectively distorted) sketch. The recognition given by scientists to (at least some) co-citation clusters constructed by the analyst does indicate something, but only that these accounts have parallel functions. This alternative perspective on the role of citations and co-citation maps can explain the loss of 50% or more of the articles in year *t* when one assumes that legitimacy resources are very varied, and that the co-citation maps can only trace the more coherent sub-groups with shared legitimacy tactics.

Co-word maps show their network character more explicitly. They are also presenting a repertoire, but now one of (often) occurring key words, and of links between key words that are made by a significant number of authors (relative to the frequency of occurrence of the words themselves) in order to make their accounts forceful. A co-word map for year *t* thus reflects the repertoire of key words available to authors in the field, as well as the network of forceful linkages that these authors have made in their articles in year *t*. The claim of the co-word analysis is that such a map shows what are overarching terms, how these relate to more specific terms, and what relationships are being articulated in year *t*. It should not directly reflect an account given by any particular author. Rather, it could be compared to a map of a difficult terrain, giving routes, and indicating ease of travelling along them, as these evolved through the habits and partial explorations of the natives. In the case of hierarchically ordered co-word maps, there is a similarity with the account of a section head in a big research organization, reporting to his director. First, the few main topics in the field are sketched, then for each some further detail, and if asked, the section head must be able to elaborate particular questions further. In order to report to his director, the section head is forced to aggregate issues in the field in a way comparable to what the co-word map analyst tries to do (Callon, Law, Rip 1986, p. 117).

Recently, co-word analysis has been extended to construction of so-called strategic maps, which present relations between clusters of keywords ("themes"), as it were the non-uniformity of the scientific enterprise. At any moment, there are focal areas in science, which are more articulated, and more central with regard to other themes of research. Techniques are being developed to capture these aspects in maps (Bauin 1987, Courtial 1987, Law et al. 1987). Such maps, like the high-level co-citation cluster maps (Small, Sweeney and Greenlee 1985), are further removed from the accounts that scientists themselves can give.

Both co-citation maps and co-word maps do not just picture what is out there. This is not a matter of their specific limitations, but is a general point to be made for all maps of scientific fields. Of

necessity, maps add and subtract, and reduce complexity (hopefully in a useful way). The main point is that it is not very productive to see science as something "out there", to be mapped. Science is an evolving and heterogeneous system, where scientists are actively concerned to create patterns and linkages. It is not a jig-saw puzzle, as one of Derek de Solla Price's favourite metaphors would have it (quoted after Small, Sweeney, and Greenlee 1985, p. 322), with parts of natural reality as the pieces of the puzzle. Rather, it is an evolving (often through negotiations and conflict) construction, a precarious and shifting stack of linked (and leaky) black boxes (a metaphor, with some theory behind it, drawn from Callon and Latour 1981, p. 285-286). Given the continual constructive activity of scientists, the aim of mapping can be rephrased, e.g. in the terminology of Callon, Law and Rip (1986, p. 216): "... the goal of co-word analysis is not to photograph a field of knowledge but to reveal the strategies by which actors mutually define one another, relate and place their problems in a hierarchy. If one is to grasp the strategic dimensions of a co-word analysis, the maps cannot be considered statically." For co-citation maps, the link with actors' strategies is more indirect, but attempts to produce co-citation clusters for the body of articles produced by a particular actor, e.g. a big industrial research laboratory, show that a similar approach is possible.

In principle, this perspective allows the development of strategic mapping, that is, mapping that reflects the strategies of actors, or the overall directions in which fields develop, as they result from the interactions between actor strategies. It will be clear that strategic maps have even less to do with the accounts that scientists themselves offer. They are not only specific constructions by the analyst, but also, in their construction, should reflect particular policy questions. For co-citation maps, examples would be the actor-related co-citation clustering mentioned above, and the use of general co-citation cluster maps to evaluate national visibility ("strength") and relationships to central clusters (Mometers et al. 1985, Franklin 1987). For co-word maps, examples have been produced where centrality of keyword clusters, and the degree of internal articulation are indicators of ongoing developments, as well as showing up opportunities and constraints for policy actions (Bauin 1987, Law et al. 1987). Journal-journal maps have been used to follow developments in policy-relevant domains (Leydesdorff and Van der Schaar 1987), and also to trace the role of technology-oriented journals (Van Steijn and Rip 1988).

In general, maps are artifacts, but useful artifacts. This then leads to the question: what sort of use? In the social studies of science, maps of science were, at first, a way to discover structural features of science, e.g. the occurrence of specialties as "natural" units

(Garfield, Malin and Small 1978, p. 185). Now that questions of dynamics of scientific development are being studied, maps are an increasingly important way to trace the dependent variable in questions like: which factors influence the growth of knowledge and/or the structure of the scientific community in a particular field? Is it possible to find impacts of scientific policy on scientific developments? (Rip and Hennekam 1985, Leydesdorff and Van der Schaar 1987)

For the present paper, I shall only consider uses in science policy, and again do so in a general way.

4. Possibilities and limitations in the policy use of maps of science

As decision support tools, maps of science may be seen as unproblematic: just another, and interesting, way to present data about the object of science policy. To chart the state of science, scientists in the relevant fields have always been asked for their judgment on the state of the field, on strength and weaknesses, and on future developments. Having available, as additional inputs, maps that are produced independently from expert, and always interested judgment, can only improve the decision making (although the legitimacy of the new tool may be contested by the experts).

It is certainly important to have additional inputs in science policy, if only, as Irvine and Martin (1985) put it, to keep peer review honest. Their point is well taken (even if their own method relies to some extent on indirect peer review, cf. also Krige and Pestre (1985, p. 529)). For the judgement of performance and quality, which is a traditional task of peer review, the use of indicators as an additional instrument has become widespread. For identification of strengths and weaknesses, and for characterizing present patterns and perhaps indicating emerging developments, maps have definite utility, in addition to, and sometimes instead of, expert judgment. Further development of the techniques, and increased understanding of what maps have to offer, will undoubtedly increase utility. The only, but important, proviso is related to what maps cannot do without additional data: capture the cognitive, social, institutional, and increasingly also economic and political dynamics of developments of fields. Even if one could construct further indicators for these dynamics, the problem is that such dynamics cannot be neatly packaged. New interactions and structures develop continually, and partly in response to policy activities. Reduction of this complexity has to anticipate on the outcome of such processes. Therefore, when maps of science, especially if they are of the non-interactive kind, are used exclusively as a basis for intervention, they may well work

as a technical fix that actually hinders a deeper understanding of the dynamics - an understanding, I add, that is now limited by the common sense of scientists and science policy makers.

Having said all this, the question arises why the technical limitations and conceptual problems discussed in the preceding section have not received more attention. The technical limitations of the maps are non-negligible, and their effect on interpretation and policy usage can be large, but it is still exceptional that mention will be made of them. One answer is that maps can be a visual fix: with their immediate impression on the senses, they create the feeling that all relevant information is now available. In addition, the fact that maps result from scientometrics push rather than policy pull, will be a partial explanation of the neglect of the technical and conceptual problems, because addressing them requires reflection on the kind of usage that is envisaged.

I would submit that there is also a feature in science policy itself that is responsible: The reluctance to take a real hard look at science, in combination with the neglect of consideration of effects of science policy making on the science system itself. Science is taken to be good, of and by itself, while policy is perceived as only helping and orienting science, not harming it. This argument can be further developed through a comparison with the evolution of environmental policy, where at one stage, environmental mapping was seen as a major decision tool (Rip and Steenkamp 1985).

Environmental mapping was at first (in the early 1970s) seen as an important tool in achieving environmental and conservation goals, and only technical questions were discussed: how to make sure that all areas were mapped in the right categories. Rather quickly after this first phase, the aggregation of the various indicators of environmental quality and nature conservation value (e.g. species diversity, uniqueness) into one index was debated. Apart from the problems of multi-dimensionality (which could in principle have been solved by multi-attribute, multi-criteria analysis, cf. Nijkamp 1986), the political issue of the use of environmental mapping surfaced: was environmental mapping really singling out areas to be sacrificed? In this second phase, the different goals were made explicit. A meadow, for instance, could be evaluated in terms of cattle grazing, in terms of recreation, for its role in supporting meadowbirds, and for conservation as an ecosystem as such. The debate over goals then led to a third phase, where conservation strategies were critically analyzed. Segregation strategies, which had been the exclusive focus for a long time, and which had led to the establishment of nature reserves separate from areas of intensive farming, were contrasted with integration strategies, where modified, but still intensive farming and limited protection of birds would go together. It was realized (partly through further research stimulated by the debate) that

meadowbirds could thrive on intensively-farmed meadows, if certain precautions were taken. With this broader view of possibilities, and with the experience of reflection on different policies, a fourth phase evolved, where the question was not anymore how to characterize a region for its natural and environmental qualities, but how the impacts of a proposed policy, or an infrastructural project, could be evaluated. Environmental impact assessments, already required in the US, were one form of policy impact analysis.

For science mapping, the debate has not evolved into the fourth phase of science policy impact analysis. In fact, it appears that some issues apparent in earlier phases of the environmental debate are not taken up in science policy in any systematic way. The first phase technical discussion how to allocate areas of science (and groups working in them) correctly is taken up, especially if mistakes threaten the resources of certain areas of science or certain research groups (e.g. Moed et al. 1985). Aggregation and indicators of strength and weakness have for some time been considered to be technical problems only, but policy aspects are now drawing more attention, and should lead to the second phase of systematic debate of goals, and a third phase of analysis of strategies. So far, this occurs only in a limited way.

The focus of debate is still rather technical, in the sense that participants pose the problem as one of finding better techniques, qualitative and not just quantitative measures, etcetera. What is mentioned, but not discussed systematically, is the heterogeneity of goals. Is excellence in science what one should strive for, or balanced development? Should innovativeness be stimulated, or can that be left to the internal workings of science? And if it is a policy concern, should it lead to a general support of diversity, with dangers of fragmentation, or to focused support, with dangers of "mono-cultures" and of wrong investment decisions? Should the reproduction function (teaching) be optimized as such, or remain integrated with the research function?

Although such questions can, in principle, be answered by systematic studies of the working of science, and such studies are by now indeed attracting increased attention from scholars and from policy makers, there is also a reluctance to be very explicit about the inner workings of science. Social studies of science have already achieved understanding, e.g. of the variety of dynamics in different disciplines, and the necessity of differentiated policies, even if these insights are not always sufficiently taken into account. There are even attempts at analysis of relationships between policy activities and developments in science (Cozzens 1986, Rip, Hagendijk and Dits 1986, Hagendijk and Rip, 1987). In the past, the tradition has been for policy implications of a social studies of science finding to stimulate brief debate, but little or no sustained analysis. For example, the

role of rank-and-file researchers with little visibility as discussed by Cole and Cole (1972) showed that perhaps half of the published articles were never cited. No definitive answer was produced, but some dimensions of the issue have been articulated (cf. also Turner and Chubin 1976): are "dwarves" necessary so that excellent researchers can stand on their shoulders to become "giants"? Or can the non-visible researchers be sacrificed (if they can be identified as such)? Unfortunately, the issue only created heated debate, and for a short period at that.

Understanding of the inner workings of science appears to be the victim of a conspiracy of silence. From the policy side, in order to continue with undifferentiated, "blanket" policy measures. From the side of scientists, to protect status and legitimacy. As examples, compare the furore over Cole, Cole and Simon's (1981) diagnosis of peer review as unbiased, but random, and the continuing attitude toward fraud as something done by deviants, instead of related to the working of the contemporary science system. In such a constellation of factors, the possibilities of maps of science will not be fully explored. They will be condemned to remain technical fixes -- or to just be irrelevant to ongoing policy struggles.

The fourth phase, of policy impact analysis, will thus not be reached through evolution of the debate. Impacts of policy measures are indeed discussed, but primarily in terms of complaints by scientists, e.g. lack of funding (the goose with the golden eggs is not being fed properly), insufficient scientific instrumentation, the lost generation of promising young researchers. Responses depend on political constellations and the common sense of policy makers. The issue of young researchers has received attention, and special fellowships and "young blood" schemes have been created. Scientific instrumentation has become a matter of policy concern and measures, and surveys are made of the nature of the problem. Lobbying and the climate for basic research appear to be the determinants of outcomes, rather than the use of decision tools that are adequate to the situation. While maps of science cannot, by themselves, solve such issues, their further development will certainly be an important contribution, because they can, in principle, be related systematically to other findings about the dynamics of science.

As an example, take the recent interest in strategic basic research, and the concurrent attempts of actors like big firms and powerful government departments to appropriate not just research results, but access to ongoing research, e.g. by privileged relationships, secrecy requirements etc. Maps that picture institutional bases of research and actors' strategies are indicators for such developments, as well as an essential step to study the effects in the aggregate. Thus, the impact of current developments in the context of science can be traced, and decisions can be taken whether policy action is necessary

or not.

More speculatively (and using ecological terminology), such decisions will be related to the background issue of the resilience (or "carrying power") of the science system with respect to external impacts. The comparison with the example of environmental strategies can actually be continued further, because social studies of science are able to say something about the possibilities for segregation or integration strategies, for instance when the emergence of "hybrid communities" is discussed (Van den Daele, Krohn, Weingart 1979; Rip 1979, 1981). Is science served by segregation? Although such arguments are voiced, history of science shows little segregation. So the question should be rephrased: how can science be productive in spite of (and perhaps thanks to) external "interference"? This is obviously a big question, that cannot be answered with the help of mapping alone. But longitudinal studies with the help of sophisticated mapping techniques will help to transform what is now an ideological debate into a discussion of strategies building on understanding.

This example serves to make the final point: the limited use of maps of science as a "visual fix", combined with the bracketing of discussion of goals and strategies, renounce real possibilities of understanding of science and of prudent science policy. For instance, it must be possible to use accounts produced in "hybrid" situations (like research proposals, "early promise" declarations by scientists) as building blocks for maps (Rip 1986), or at least develop ways to aggregate them into functional equivalents of maps.

Such opportunities to develop mapping of science in a creative way must be actively explored.

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