SUMMARY

Recent work in the social studies of science has emphasized the importance of studying both the social and cognitive aspects of the evolution of scientific specialties and disciplines. This has implications for science policies that aim at the direction of scientific fields toward external goals: the cognitive state and dynamics of the field have to be taken into account. Such a cognitive approach to science policy has been elaborated by a number of German science scholars. The three-phase model of scientific developments and the finalization thesis of the Starnberg group is discussed, and the policy implications are critically reviewed. A group based in the University of Bielefeld has published case studies designed to trace the role of cognitive factors in explaining the impact of science policy programmes on scientific fields. It turns out that mutual adaptation processes occur in the course of formulating the programmes which reduce conflict and resistance. In conclusion, some perspectives for further work are noted.

A COGNITIVE APPROACH TO SCIENCE POLICY

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1. Introduction

Science policy has devoted itself to the problems of organization and of allocation of money [1]. Although scientific advisers are called in to review the state of a science and its potential for new and useful developments, the substance of science has normally been out of bounds for policy making. Such a set-up is legitimated by, and through its continued existence supports, the view that science, which concerns cognition, has no intrinsic connection with politics, which concerns power.

The limits to growth of the science budgets, as well as pressing technological and social problems, have led policy makers to take a second look at this division of territory. More or less at the same time, developments in the history, sociology and philosophy of science occurred emphasizing the necessity of looking at social and cognitive aspects of science together; one example is the renewed discussion and study of the role of external factors in the development of science. When it is also realized that politics is not only the play of power and clashes of interests, but also includes cognitive aspects like assessments of situations and outcomes, reasons for goals and priorities, etc. the original contrast between cognition and power is amplified to a 2×2 matrix (see fig. 1).

Because of the interactions within the rows and columns of the matrix of fig. 1, external influence on science is possible, decisions in the political sphere eventually affecting the outcomes of scientific research. From this perspective, insight into the cognitive, as well as social state of a science becomes a prerequisite for a wise policy. Recent work in the social studies of science has made the 'black box' of science and the dynamics of its develop-

Fig. 1. The social and cognitive aspects of science and politics.

ments somewhat translucent, thus providing material for what I shall call a cognitive approach to science policy.

Often, social studies of science are not designed with any science-policy relevance in mind. A group of German science scholars (Wissenschaftsför-scher), formerly based at the Max Planck Institut in Starnberg, has made a concerted attempt to construct a model of the development of science with explicit policy implications [2–4]. Using the same cognitive approach, some members of this group have collaborated with science scholars from the University of Bielefeld in analysing the impact of policy measures on the development of sciences [5,6]. In this paper I give an outline of some main points and a critical review of the two recent books, *Die gesellschaftliche Orientierung des wissenschaftlichen Fortschritts* [4] and *Geplante Forschung* [6]. To set the scene, I shall briefly refer to recent trends in the social studies of science.

2. The dynamics of the development of science

Fig. 2 shows how Thomas Kuhn's model of the paradigm-guided development of science has led to a flood of research in the social studies of science. A lot of empirical material on the development of scientific specialties and research areas has been accumulated [7], with some attempts at systematization [8]. Separate approaches that can be distinguished at the moment are scientometrics, especially with the help of citation analysis [9], an anthropo-

Fig. 2. Recent trends in the study of the dynamics of science.

logical approach [10], and a cognitive approach, where the cognitive developments are not treated as a black box, but are amenable to sociological analysis [11].

Kuhn [12] has shown, on the basis of historical evidence, that the development of science proceeds with periods of normal science — where scientists in a discipline or specialty share a common paradigm and work towards its further articulation and refinement — alternating with periods of revolutionary science — where concern about anomalies and proposals for alternative paradigms throw the discipline into a crisis until one of the alternatives wins out and a new period of normal science ensues. The paradigm provides cognitive norms or regulatives for ongoing research in a discipline, and through the shared commitments of the members of the discipline also a sense of community. According to Kuhn, successive paradigms are in a sense incommensurable, and the transition between them requires a *Gestalt* switch that is likened to a conversion.

Although the function of a paradigm in providing regulatives for research and building a disciplinary community has been widely accepted, the other features of Kuhn's model have not. Paradigms, even if they are called 'central dogmas', may be abandoned without fight or crisis symptoms [13]. The incommensurability thesis has been a challenge to the philosophy of science to come up with new theories of rational progress in science [14]. The Starnberg group has pointed out that old paradigms may sometimes live on besides their successor and continue to be used, although with a clear sense of their limitations. Their criticism of Kuhn is a stepping-stone toward their own model for the development of science.

The paradigm of classical mechanics, although replaced by relativistic mechanics, and on the molecular and atomic level by quantum mechanics, is still valid for most purposes, and has a certain self-sufficiency that leads us to expect that it will remain valid. Heisenberg has introduced the notion of a 'closed theory' to describe such cases, and the Starnberg group has tried to develop more precise criteria for such cognitive stability [15]. From the example of classical mechanics, and others like electromagnetism, thermodynamics, and molecular chemistry, they conclude that paradigms may become *mature*: the regulatives for research are no longer directed to articulation of the paradigm, but toward application and specification for new or more complex object domains. Examples would be thermodynamics of living systems, mechanics and electrodynamics of plasmas.

A second difference with Kuhn's model is that the Starnberg group recognizes that the pre-paradigmatic stage, mentioned by Kuhn, has its own characteristic research strategies, and can be recognized in many contemporary

disciplines (for instance, microbiology until quite recently, medical and public health science). The Starnberg group thus distinguishes three possible cognitive states of scientific disciplines, and in addition claims that they represent successive phases in their historical development [6, pp. 13–16].

(1) *Exploratory phase*. There is as yet no theory that organizes the field, empirical, trial-and-error strategies abound, discovery is more important than explanation.

(2) *Paradigm phase*. A fundamental (*grundlegende*, i.e. fundamental with respect to the domain of the specialty) theory is proposed, accepted and articulated further. The problems posed by such theory development regulate the activities within the field and so take precedence over other concerns. Kuhnian revolutions or gradual evolution may exchange one fundamental theory for another, but theoretical explanation remains the overriding concern.

(3) *Post-paradigmatic phase*. The fundamental theory is stable (according to cognitive and/or social criteria) and the mature paradigm can be exploited for other goals, internal or external to science.

The policy implications of the Starnberg model for the development of sciences are bound up with the value the Starnberg group attaches to developments in the third, post-paradigmatic phase. Compared with the paradigm phase, the internal regulatives lose much of their force and there is room for external regulatives, i.e. external goals that are translated into guidelines for research and specific theory development. The river of science has, as it were, reached its delta and can now be made to change its course very easily. The internalization of external goals is called *finalization*, and the resulting specialized developments are called finalized specialties or disciplines [3; 16, pp. 210–220; 17].

An example of a finalized discipline is hydrodynamics, a branch of physics in which classical mechanics is developed specifically to account for the phenomena of flow. New theory development is necessary because the equations of classical mechanics cannot be solved for the complex processes of flow. Vital to the specialized development is the introduction of a guiding concept (*Anwendungsgrundlage*), in this case the concept of a boundary layer adhering to objects or surfaces along which the flow passes. Around this concept, the basic equations of hydrodynamics are built up, and the resulting theory continues to apply the conceptual apparatus of classical mechanics, is legitimated by it, but cannot be derived from it [19]. The regulatives of hydrodynamics are external to classical mechanics, although they can be considered to be internal to physics. Historically, they are related to attempts to systematize the technical knowledge of hydraulics [20].

Another example is provided by plasma physics, itself a specialized devel-

opment from mechanics and electrodynamics to treat the behaviour of ionized gases in electromagnetic fields (plasmas). Again, guiding concepts are necessary, e.g. to obtain the equations of magnetohydrodynamics. External goals have provided additional regulatives in the specialty of thermonuclear fusion research, where the behaviour of plasmas in specific geometries and with surface effects are treated. The specialty was born during the 1950s, when the crash programme to develop a fusion reactor failed and fundamental knowledge had to be gained to advance further. The external economic goal of delivering energy dictated possible geometries, as well as the inclusion of the effects from the walls of a reactor. Having become regulatives of the new specialty, they didn't need to be enforced by management, but were integrated in the workings of the scientific community of the specialty [21].

The last example shows why the Starnberg group emphasizes the importance of finalization. In finalized specialties, a middle-of-the-road alternative appears to be realized between the extreme positions in the science policy debate, namely complete autonomy of scientists versus centralistic planning. In principle the research can proceed autonomously because the external policy goal is internalized as a regulative in the specialized paradigm of the of the finalized specialty. In practice, additional direction will often be necessary [22], while the creation of such a paradigm is not a trivial problem but a scientific challenge by itself.

When a discipline does not possess a mature paradigm, finalization is not possible. Attempts to direct the development toward external goals cannot profit from the insight into the nature of the object domain that is provided by a stable fundamental theory, and are called *functionalization* by the Starnberg group. Screening for functions and input–output relationships would be functional strategies. An example would be the screening of chemical substances for pharmacological effects, while a more theory-oriented strategy would be to develop structure-activity relationships to find explanations of the effects in terms of mechanisms. Other examples of functionalized disciplines are econometric modelling, based on extrapolation of trends, and system dynamics (e.g. Forrester and Meadows world dynamics).

According to the Starnberg model, functionalization is easy in the exploratory phase of a discipline, because then there are no strong internal regulatives that compete with the external direction. In the paradigmatic phase, scientists resist external direction and it may be argued that external direction in this phase is counter-productive. Here, as with the notion of maturity or ripeness of a discipline, the Starnberg group takes over the 'common sense' of scientists in fundamental scientific research: functionalization is condemned as over-exploitation of a scientific discipline. They also point to the danger of

un-critical uses when a fundamental theory lacks or is neglected [16, pp. 234–240].

The policy implications of the Starnberg model can now be summarized as a twofold advice: science scholars should diagnose the cognitive state of a discipline, and then policy makers should decide on light exploitation in the exploratory phase, stimulation in the paradigm phase, and finalization toward suitable ends in the post-paradigmatic phase. These implications have been drawn by the Starnberg group members, and have led to acrimonious and often misdirected debate [23]. It should be noted, however, that the policy implications are programmatic: the Starnberg group itself has not analysed the effectiveness of policy measures in relation to the cognitive state of the disciplines concerned, but has concentrated on historical studies illustrating and further articulating their views on the development of science [19, 24–27].

Attempts to make the policy implications more operational will face the difficulty that the concept of finalization is still rather vague (as has been noted by others [28]; in my presentation I have emphasized one possible interpretation in terms of guiding concepts; compare also [17]) and that it does not seem possible for science scholars to determine the maturity of a field unless they accept the judgement of the scientists working in it [29]. But these scientists, or the elite that frequents science policy circles, are already consulted on the issue of the ripeness of the field in many cases, so there seems to be no special role for the science scholar except that he may contribute a non-interested opinion. In fact, when diagnosing the state of a discipline, the Starnberg group uses an implicit criterion of maturity, namely a reductionist relation to the central theories of physics, and more specifically, a 'micro-theoretical' explanation of phenomena, i.e. in terms of invisible particles and their interactions. The reductionist slant has been noted by others [28, p. 333] and is apparent in some of the articles (e.g. [3, pp. 310–311]). The micro-theoretical explanation is explicitly used as a criterion of maturity in the analysis of the disciplines contributing to cancer research [30] and to biotechnology [31], and mentioned in passing in [6, p. 42]. Symes has noted the same point, and tried to build a definition of maturity on it [32, esp. pp. 339–340].

For policy makers interested in manipulation and control, the Starnberg model has little to offer. The identification of finalization and the conditions of its occurrence provides better insight into the working of contemporary science, but no direct policy advice. Furthermore, the model cannot be generalized very far; technological disciplines and 'formal' disciplines (like mathematics, computer science) may show three phases, but even then have differ-

ent types of regulatives. And many important cases of problem-oriented research do not fit the pattern because the main issue is the integration of different disciplinary contributions (compare the case studies in [6], and the examples, as well as the general argument of [33]).

Viewed as a first attempt, however, the Starnberg model fares better, and some more modest uses can be noted. Although the Starnberg group did not follow up the possibility, the importance of fundamental research is, in their model, not a belief or ideology but an arguable and partially researchable proposition. If one accepts the reductionist view, contributions of different disciplines to the solution of a problem can be assessed and corresponding policy measures devised [34]. An important result of the work of the Starnberg group is also that they have developed a vocabulary to analyse cognitive developments that is adapted to the language of science policy. The case study of fusion research programmes in the US and FRG profits from the use of the concepts of internal and external regulatives, [35] while functionalization and *Anwendungsgrundlage* can be applied in analysing biotechnology [31,36]. Surveys and perspective reports of scientific disciplines will improve their analysis and recommendations by explicitly using such cognitive-policy language, as can be illustrated in the case of chemistry [37].

As a concrete example of the use of the Starnberg model, I present some results of Hohlfeld's analysis of the cognitive state of cancer research [30]. Table 1 shows his diagnosis of the cognitive state of contributing disciplines. Looking more closely at the possibilities for mission-orientation of disciplines in the second, paradigm phase, Hohlfeld notes two research policy strategies; coincidence research and transfer research. (Examples are given in tabel 2.) In coincidence research, a system relevant to the overall mission (in this case, cancer prevention, control and therapy) is sufficiently manageable and representative to be used in the laboratory to develop the fundamental theory. The 'dirty' systems normally found in practice can sometimes be studied using results obtained on model systems as guidelines. Attempts to articulate and modify such guidelines are called transfer research. The practice of coincidence research and transfer research has, I am sure, been part of the folk wisdom of research management for a long time. Their conceptualization in terms of a model of scientific development is the first step to further rationalization. The assessment of the contributing disciplines may also be useful, but it is harder to draw conclusions here, because Hohlfeld does not use his assessment when studying the cancer research programmes in the US and FRG [38] (but compare [34]).

Another use of a first model for the development of disciplines and its policy implications is that it leads to improved models. One would expect

Table 1
Disciplinary contributions to cancer research

From stable theories

- pathological anatomy (classification of tumours)
- chemistry (identification of carcinogens)
- classical genetics (somatic mutations)
- biochemistry (metabolism of tumour cells)

From theories being articulated

- molecular biology of cells of higher organisms
- developmental biology of cells, tissues, organs

From fields in an exploratory phase

- human biology
 - medical science
 - public health science
-

Table 2
Mission-orientation of fundamental research (examples from cancer research)

Coincidence research

- differentiation in the system of hemopoietic (blood-building) cells, including malignant growth and its effects
- induction of differentiation in lymphocytes by immunological reactions, including reactions to tumour cells

Transfer research

- *in-vitro* cultures of 'dirty' systems, e.g. lung cancer tumours
-

the case studies of science policy programmes in FRG [6] to be an occasion to apply and refine the model. Although the vocabulary is used in some case studies, 'finalization' is never mentioned – because of the controversies that have sprung up around it, I suppose; compare [23] – and the model is used sparingly, if at all. The emphasis of the book is on the resistance and receptivity of scientific developments toward political direction.

3. The political direction of science

In *Geplante Forschung*, the question of the cognitive conditions for successful mission-orientation of scientific disciplines is tackled from the empirical side. The cases selected for analysis have to extend beyond the application of existing knowledge; the policy goals have to be attained by institu-

tionalization of special scientific developments, i.e. new specialities or research fronts with their own dynamics. The empirical question is about the conditions of success of a dynamic initiated through the political direction of science – which implies that finalization may or may not turn out to be the label of one of the successful dynamics. In fact, the frame of reference of the case studies is not the Starnberg model for the development of disciplines, but a theoretical study by van den Daele and Weingart on the resistance and receptivity of science to external direction [5].

The case studies look at biotechnology, information science, cancer research, environmental research, thermonuclear-fusion research and heavy-ions research, and have been selected because these fields have been subjected to explicit science policy programs by the German federal government. Such a selection is necessary to distinguish between general promotion of science and actual direction, and to provide an empirical basis for the identification of the content of the political direction. The study would then proceed by analysing the mechanisms of the transformation of the external goals into regulatives of the research process, and by observing symptoms of resistance, if any, to the external regulatives [6, pp. 239–240]. From the detailed study of the case a judgement has to be derived whether the resistance is institutional (e.g. traditions, inertia of organizations) or cognitive (cognitive deficits, conflict with internal dynamics). Successful political direction is defined as the achievement of a change of direction of the scientific developments, not in terms of the fulfilment of the mission (which can never be a criterion given the nature of scientific research).

It turns out, however, that science policy programmes and the social and cognitive state of the sciences at which they are directed are not independent. Scientists participate in the transformation processes that lead from external, political goals to science policy programmes and their implementation, in such a way as to adapt the political demands to the perceived scientific possibilities. Other adaptation processes within the political as well as the scientific systems play also a role. Conflict potential is reduced by this process of mutual adaptation, but makes the task of the analyst who studies the impact of political direction on science more difficult. The process has to be traced in detail (this has been done thoroughly for the case of environmental research [39]), but even then conclusions have to be based on comparisons with what happened in other countries and counter-factual speculations about what could have happened if institutional structures or cognitive developments had been otherwise.

A number of steps can be distinguished in the transformation process from external goals to research regulatives. In fig. 3 the analytical scheme devel-

Fig. 3. The transformation of problems.

oped on the basis of the case studies is presented and illustrated with some of the steps taken in the course of creation and implementation of the National Cancer Program in the US [6, pp. 22–23; 40, p. 223]. The scheme consists of successive translations, which become transformations because of the intervening filters (labelled ‘political resistance’, ‘restrictive structures’, etc.). In addition, the most important scientific inputs are depicted.

The social problem, the cure of cancer, is already the result of transformations of problem definitions, e.g. the expectation of a cure based on the experience of other successful therapies developed by medical science. To become transformed into a political programme, the social problem has to struggle for priority, in this case in public health policy. Besides the seriousness of the problem and the amount of lobbying, a scientific input is necessary: science should promise at least a partial solution in the near future. In the actual formulation of the political programme, scientific consultancy will influence which items are given prominence, and this will reflect the state of the art in the relevant disciplines, as well as their institutional strengths and weaknesses.

By singling out scientific research as providing the main answer to the political demand – which decision may have substantial reasons, but also just follow the trend of considering science as a catch-all problem solver, or be a manoeuvre to side-track the problem – the political authority has to provide a translation into a science policy programme. Scientific consultancy is very important here, and disciplinary and institutional divisions will determine part of the outcome. Administrative divisions and territorial struggles of government bureaucracies should not be underestimated, however (environmental programs are a clear example [39]). Lower down in the scheme the implementation of the science policy programme is transformed by institutional resistances, traditional disciplinary attitudes, etc. as well as by the successes and failures in the ongoing research.

In the opening essay of *Geplante Forschung*, van den Daele, Krohn and Weingart emphasize (even more than in the earlier version [40]) the importance of so-called *hybrid communities* as the carriers of the mutual adaptation process between politics and science [41, pp. 26–27]. In many cases studied, groups were formed composed of scientists, administrators and representatives of interest groups to effect the translation/transformation of the political programme into the science policy programme, and sometimes also to act as advisory committees or steering groups. In the exceptions that are noted (fusion research, one of the information science programmes), one could still point out that those scientists were selected that had an affinity for policy making [42]. The hybrid communities, of which the officially constituted groups may form only a small, but visible part, are the carriers of the process of transformation from external goals to research regulatives, and may perhaps also keep the research 'on track', i.e. take over the function of the traditional disciplinary communities. This will only happen, however, if a reward and career structure can be created that is accepted by the scientists that are to do the research.

As overall conclusions from the set of case studies, van den Daele, Krohn and Weingart note two main points. First, that the appearance of hybrid communities is an indicator for an attempt at political direction of science, while its success depends on the institutionalization of the external regulatives in a manner compatible with the internal regulatives. Secondly, that the cognitive state of a discipline and the nature of its internal regulatives are independent variables determining the attitudes of scientists towards external orientation. This implies that resistance of scientists cannot always be reduced by the institutional measures that administrators can take (e.g. money, opportunities, career structures), and that science policy has to take the cognitive aspects of its policy object into account [41, pp. 34–35, 57]; compare also [35, p. 289].

4. Concluding comments

It was noted already that the case studies in *Geplante Forschung* do not aim at a further articulation of the Starnberg model. In spite of their heterogeneity of scientific fields studied and analytical frameworks used, however, an important common aspect can be singled out. All fields studied combine contributions from different traditional disciplines, and struggle with the problem of integrating them, cognitively as well as socially. Van den Daele, Krohn and Weingart view these problem-oriented fields as complementary to traditional disciplinary science [41, pp. 55–59]. This implies to my mind that the concept of a finalized science will only apply to contemporary problem-oriented science in a few exceptional cases. The parent disciplines have to be oriented towards an external goal only in cooperation; that is to say, that instead of a guiding concept (*Anwendungsgrundlage*) an integrating concept (*Integrationsgrundlage*) is necessary. The concept of an ‘integrating concept’ has been used by others in another context [43], but I think it would be useful to extend the Starnberg model in this way and see if some of its limitations can be overcome.

A difficulty that will remain, and one that has been criticized already, is the assumption that science is homogeneous. The assumption is facilitated by the physicalist–reductionist outlook of the Starnberg group, which can also be recognized in some of the case studies in *Geplante Forschung*. Because of the more empirical approach in the case studies, the assumption does not play an important role and, in fact, different kinds of disciplines are discussed. My impression is, however, that the case studies do suppose a social and political homogeneity of science, or at least neglect possible divisions in this respect (with the exception of a few side-remarks in the study of heavy-ions research

[6, p. 352]). With increasing democratization of decision-making, divisions due to social stratification in science (including the technical assistants) will make their influence felt. And the controversial nature of many science- and technology related decisions will activate latent political differences between scientists, as has happened already with recombinant-DNA research and in the biomedical field [44].

The politicization of science policy will also influence the composition of the hybrid communities. To obviate accusations of one-sidedness, advisory committees on recombinant-DNA research have to contain at least one certified opponent. When this trend continues, the study of hybrid communities will become an even more important tool for science policy studies than van den Daele, Krohn and Weingart have shown it to be.

In spite of its limitations, the Starnberg model has rendered an important service to science policy studies: it has focused attention on a cognitive approach to science policy. *Geplante Forschung* was inspired by it, and used some of its conceptual apparatus to demonstrate empirically that it is necessary to take cognitive factors into account in the analysis of science policy, and also, I would add, in science policy itself. A better model for the development of scientific fields has not been proposed, but it is more important to continue this research tradition. In terms of the Starnberg model, it is in its exploratory phase, and it may even be exploited lightly for the external goal of improving science policy.

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