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Coping with Trouble

How Science Reacts to Political Disturbances of Research Conditions

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Coping with Trouble as a Complex Constellation of Political and Research Actors: Introducing a Theoretical Perspective

Uwe Schimank and Andreas Stucke

In this introductory chapter we will offer a theoretical frame of reference for all of the contributions to this book. We will do this in three respects. We will begin by describing the growing practical and – somewhat neglected – theoretical relevance of the topic of "Coping with Trouble." Then we will clarify the main variables which constitute the topic – trouble and coping – and present an actor-theoretical frame of reference focussing on the aggregate effects of coping within complex constellations of actors. Finally, we will characterize the research approach guiding the organization of this book as a search for a "grounded theory of the middle range," and provide a preview of the case studies to follow.

Thus, this introduction sets the stage for detailed empirical studies. It does not anticipate their findings – we have saved that "harvest" for the conclusion. This chapter does, however, develop theoretical concepts which we hope will enable the reader to perceive familiar empirical phenomena in a new light.

1 Political and Theoretical Relevance of the Topic

Anticipating the more detailed discussion in the following section, the kinds of trouble we are concerned with here are *violations of certain vital interests of state-financed researchers or research institutes*[1] *by political action.* Many

1 We exclude industrial research from our consideration, even where it is mainly financed by the state. This is certainly not to deny that industrial researchers are faced with trouble, too.

contemporary examples come to mind quickly. One kind of trouble is certainly the situation which John Ziman has portrayed in reference to the United Kingdom as "science in a 'steady state'" (Ziman 1987). The money for the promotion of research which had been provided quite generously by the state for more than two decades became increasingly scarce during the 1970s and 1980s in many of the major Western countries. A "period of affluence" turned into a "period of scarcity." Attempts to establish more rigorous evaluations of the quality of the research promoted institutionally or by project grants were typical of this phase, most visibly in Great Britain. No matter how useful such evaluations may be from the point of view of the state, for the researchers and institutes involved these new conditions of their resource acquisition were obviously often troublesome.

Another frequent source of trouble was political demands to increase the societal utility of scientific research by directing it toward areas of vital concern for important societal groups. Environmental problems, or the development of technologies needed by major domestic industries suddenly had top priority. The trouble implied in this was aptly described under the heading of "science as a commodity" (Gibbons/ Wittrock 1985), which means the danger of research becoming increasingly instrumentalized for the realization of very narrow or even – in the case of military research, for example – dubious societal interests. The dependence of researchers and institutes upon increasingly scarce resources from the state made them increasingly vulnerable to such pressures.

Thirdly, in some fields, research faces trouble or might be facing it soon because of political regulations forbidding specific scientifically promising research themes or methods. The most spectacular recent cases have been in certain subfields of genetic engineering. But the restrictions imposed upon empirical social research by data-protection laws, for example, also hamper research. Again, as with the other types of trouble, there might be – and often are – very understandable societal and political concerns motivating these political interventions. As private citizens wanting to protect, say, their own health or privacy, even the affected researchers themselves might be in favor of such political measures. But for the researchers' work, they constitute trouble – and it is only this fact that is of concern to us here.

Finally, there is a type of trouble which results from fundamental institutional rearrangements of research institutes or even the research system as a whole. The ongoing transformations of the societies in Eastern Europe serve as a particularly dramatic illustration of comprehensive change throughout the system. The rapid political, economical, social, and cultural changes occurring in these societies confront their research systems with entirely new demands and expectations. A very special subcase of this is the unification of Germany, because the entire society of the former German Democratic Republic was integrated into the totally different societal structure of the Federal Republic of Germany, with strong repercussions in the research systems of both the East and the West.

For all of these types of trouble – resource scarcity, political instrumentalization, political regulation, and institutional rearrangements – countless stories from the history of scientific research in different countries could be told. Trouble, and coping with trouble, are nothing new. What may be new about the current and, as may be expected, future situation is the cumulation and interrelation of at least the first three kinds of trouble. This estimation becomes plausible within a long-term view of the development of the research system of modern societies.

Since the Renaissance, the differentiation of scientific research as an autonomous societal subsystem has emphasized the character of scientific knowledge as an end in itself. Serving at first mainly as a legitimatory device to ward off interference by the church into the production of scientific knowledge, it later became more useful for defending research autonomy against extrascientific demands from the state, the military, and industry. The greater the researchers' and research institutes' autonomy is, the greater their freedom is to act according to the research system's own inherent logic of action. This logic demands from a scientist that he acquire a scientific reputation by contributing new scientific knowledge which the respective scientific community evaluates as being important and true (Polanyi 1962). Admittedly, not all researchers follow this orientation all the time; but it is, although probably not the dominating orientation within the research system, without doubt the one which ultimately distinguishes research activities from all other kinds of social action. Accordingly, this orientation shapes the vital interests of most researchers and research institutes; it is from the *conflict between this internal logic of action and the research system's dependency on external resources* that the trouble to be discussed here originates.

For centuries, research activities were most often financed privately – either by the researchers themselves, if they could afford it, or by wealthy patrons. As long as the financial demands of research were small, these arrangements worked. In the nineteenth century, however, they began to be replaced by the functional combination of research with higher education at the universities. In this combination, the resource demands of research remain inconspicuous and profit to this day from the high societal legitimacy of teaching. Research at universities shares a common pool of resources with teaching, which is considered legitimate because the teaching is supposed to be based very directly on the results of the continually progressing research in the respective disciplines. But, since the mid-nineteenth century, research has gradually grown beyond the sheltering embrace of higher education. Outside of the universities, research institutes were founded which had to justify their resource demands in other ways. At the same time, the resources required by university research grew to such an extent – particularly in the experimental sciences – that they could no longer be acquired under the cover of teaching needs. Thus, the mobilization of the necessary financial resources became one of the major problems of state-financed scientific research – and, thereby, one source of trouble to the respective researchers and research institutes.

The best way to legitimize one's resource demands has always been to promise that important societal benefits will emerge from one's research. But such promises are inherently ambivalent. They are undoubtedly persuasive, fostering within society certain expectations about future benefits. Sooner or later, however, such expectations have to be satisfied, at least to a certain extent. If they are, they may even grow; then, still more will be expected from an increasing range of research fields. Thus, by solving short-term problems of legitimizing resource demands, the long-term problem of insatiable societal demands for useful research is created. This is precisely what has happened during this century. Since the 1950s, at the latest, Western societies have been portrayed as science-based societies which owe their continual progress to an extensive scientification of an ever-wider spectrum of societal problem solving – from industrial production, political decision making, and health care to such realms of social life as child-rearing or sexuality.[2] This is the logical conclusion drawn from the cultural tradition of Western modernity,

2 See, among other similar concepts, Helmut Schelsky's "scientific-technical civilization" (Schelsky 1961), Daniel Bell's "post-industrial society" (Bell 1973), Rolf Kreibich's "scientific society" (Kreibich 1986), or Nico Stehr's "knowledge society" (Böhme/ Stehr 1986; Ericson/ Stehr 1992).

which closely ties societal progress to scientific and technological progress, thereby stimulating an escalating use of scientific knowledge in society (Schimank 1990; 1992). Thus, Francis Bacon's Renaissance vision of scientific research receiving generous financing from the state because of its beneficial effects on the general welfare of mankind (Krohn 1987) has finally come true to a remarkable extent.

This relationship between scientific research in search of truth and extra-scientific actors demanding useful truths does not necessarily have to be encumbered by trouble. But it always implies the possibility of trouble. Indeed, for researchers, impatient and often immodest demands to find solutions to societal problems are a disruption. Societal groups who see themselves as potential beneficiaries of the knowledge produced in a particular research area often try to instrumentalize the research process strictly for their own immediate interests. The more successful such interventions are, the more narrow-minded and short-sighted research efforts are likely to become. This poses trouble for researchers and research institutes because they are committed to the goal of attaining the best possible scientific reputation, which tends to be garnered by publishing broadly-based contributions furthering the long-term advancement of the respective research area. Within academic science in the universities as well as in many state-financed research institutes outside of the universities, this curiosity-based orientation is very strong. Consequently, the relation to extrascientific interests which demand "value for money" often becomes a source of trouble.

Moreover, in the Baconian vision, there was no presentiment that scientific knowledge might become not only useful, but harmful to society as well. This innocent, optimistic point of view can no longer be upheld. Of course there were many eruptions of distrust in science before the twentieth century (Cohen 1980). But this distrust was temporary and often articulated by relatively small and ineffectual societal groups. In recent decades, however, scientific progress has increasingly exhibited its gloomy side to society, as warfare with poison gas or the atomic bomb, fatal accidents in chemical plants and atomic reactors, or the recent damage to the ozone layer exemplify. A science-based society is necessarily also a "risk society" (Beck 1986; Schimank 1990; 1992). Moreover, the risks of scientification have given rise to social movements which attack not only the ways scientific knowledge is used by certain social groups, but also scientific research itself as irresponsible insofar as it practically uses society as its extended laboratory for often dangerous experiments

(Krohn/ Weyer 1990). The Chernobyl catastrophe as well as the Gulf war were large-scale scientific experiments, indeed (Krohn/ Weingart 1987; Weyer 1991). Thus, protests against lax restrictions of scientific research also begin to cause trouble for researchers and research institutes. Again, scientific research following its own logic of action oriented toward the acquisition of scientific reputation cannot help but perceive extrascientific restrictions of its choice of themes or methods as being troublesome to the extent that these restrictions prohibit promising work. This is especially true if researchers in a given research area are not all restricted to the same degree – for instance, if the laws regulating genetic engineering are much stricter in one country than in another.

Thus, scientific research is ambivalent to society: It is useful, but it also harbors risks. This ambivalence results from the researchers' and research institutes' dependence on external financial resources combined with their dominant interest to improve their scientific reputation. Three interrelated trends – increased costs of research, increased demands for its societal use value, and increased societal risks of the application of scientific knowledge – constitute the basic sources of trouble for those researchers and research institutes primarily devoted to the internal logic of research. Having thus sketched the practical relevance of our topic, we can now consider its theoretical relevance.

Investigating how researchers and research institutes cope with political trouble could be an important extension of an *institutionalist perspective on science* shared by many sociologists and historians of science as well as by political scientists investigating science policy. This perspective, theoretically developed especially within the sociology of science during the 1950s and 1960s,[3] focusses on scientific research as a subsystem of modern society with its own roles and norms, its own communication and reward structure, its own formal organizations, and its own relations to the other subsystems of society. The institutionalist perspective stresses the distinctiveness of research as a specific type of social action – a distinctiveness produced by these components of the institutional order of the research system.[4] What is especially

3 Initiated by Robert K. Merton, this perspective was best represented by Joseph Ben-David (Ben-David 1971; 1972; 1977: 29-193; 1991).

4 In contrast to this, the sociology of scientific knowledge which has superseded the institutionalist perspective within the sociology of science since the 1970s neglects, and some-

interesting for us are the studies using this approach which deal with structures and processes of mutual social influence between the research system and the political system (Price 1965; Weingart 1970; Greenberg 1971). It is just this interface, as seen "from below" by individual researchers or research organizations, which interests us when we ask how they cope with politically induced troubles.

But we need more than a theoretical perspective which emphasizes the distinctiveness of scientific research as a societal subsystem and deals with the manifold relationships between the research system and the political system. Both conditions are fulfilled also by the perspective of sociological systems theory as developed by Talcott Parsons and, later, by Niklas Luhmann (Storer 1966; Parsons/ Platt 1973; Luhmann 1968; 1981; 1990; Stichweh 1984; 1987; 1988). This perspective, however, denies the analytical importance of distinguishing between social entities which are able to act and social entities which can only shape action – it overlooks the distinction between actors on the one hand and institutional structures or social systems on the other (Schimank 1985: 426-432). From a systems-theoretical point of view, the research system or the political system acts, while we conceptualize each of these societal subsystems as an institutional order which shapes the actions of the actors embedded within it – the researchers or research institutes within the research system, for example, and politicians, bureaucrats, parliaments and ministries within the political system. Such an *actor-theoretical foundation* of the institutionalist perspective allows us to describe and explain the selection and the outcomes of social action with regard to the interests, interdependencies, resources, and strategies of the relevant individual and corporate actors within societal subsystems.

An interesting exception within the systems-theoretical perspective is Wolfgang Krohn and Günther Küppers' understanding of scientific research

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times even denies, the difference between scientific research and other kinds of social action (Latour/ Woolgar 1979; Latour 1987; Knorr-Cetina 1981; Knorr-Cetina/ Mulkay 1983; Lynch 1985). This approach, inspired by relativist philosophies of science, an epistemological social constructivism, and sociological ethnomethodology, does point out important similarities between social action within laboratories or scientific controversies, on the one hand, and political, economic or religious action on the other. From the institutionalist perspective, however, the sociology of scientific knowledge is not a theoretical competitor but, rather, a source of possibly useful, complementary approaches to analysis (Ben-David 1983; Freudenthal 1984).

as a "self-organizing" social system (Krohn/ Küppers 1987; 1990; Küppers/ Krohn 1992). They explicitly deviate from the usual systems-theoretical premise and state that actors – individual researchers and the research group – are the proper basic elements for an analysis of scientific research. These actors' principal aim is to ensure the continuation of their research activities. To achieve this, researchers and research groups engage in several kinds of activities, some of which are directed toward the political system – such as, for instance, political lobbying to attain the promotion of research programs from the state. This analytical approach stressing the vital interests of actors within the research system to come to terms with actors in this system's environment – especially political actors – is obviously a good starting point for our topic.

We would like to introduce three additional aspects which Krohn and Küppers have not addressed, but which we have found necessary for a thorough analysis of the research system's coping reactions to political disturbances of research conditions. Firstly, Krohn and Küppers analytically disregard the fact that not only individual researchers and groups of researchers, but also research institutes and groups of research institutes – such as, for instance, the Max Planck Society – are actors within the research system. One of the reasons such corporate actors – which are often quite powerful – have emerged is to facilitate strategic action against potentially troublesome political interventions into scientific research. Secondly, Krohn and Küppers offer no analytical tools for the analysis of interdependencies of actors and the aggregate effects of the interplay of many actors' actions. Such tools for understanding complex constellations of actors have been developed within different branches of the actor-theoretical perspective – in social-exchange theory, network analysis, principal-agent theory and game theory, to name just a few. Integrating such general analytical tools within the toolbox of an institutionalist perspective on scientific research seems indispensable to us. Thirdly, being very abstract, Krohn and Küppers' outline disregards specific institutional factors within the research system and within its societal environment. For instance, whether a research institute is primarily financed by institutional grants or by project grants obviously has a strong influence on both the type and the intensity of resource trouble the respective political actors can cause the institute.

There are many empirical studies of the different facets of the relationship between scientific research and politics which implicitly share Krohn and Küppers' general analytical orientation and also take the aspects neglected by them into account to a certain extent. Some work at the Max-Planck-Institut für Gesellschaftsforschung in Cologne in the field of the sociology of science has been oriented toward formulating a new institutionalist perspective based on the latest concepts of actor theory. This approach has been applied to such topics as the institutional dynamics of state-financed research institutes outside of the universities in West Germany (Hohn/ Schimank 1990), the political promotion and guidance of medical research in different Western countries (Braun 1991; 1992), the reaction of German research institutes and research politics to promising research opportunities in superconductivity (Jansen 1990), the differentiation and political role of the Federal Ministry for Research and Technology in West Germany (Stucke 1993), or the promotion of cooperation between research institutes and industry as a new instrument of research policy (Lütz 1993). These studies found numerous examples illustrating the ability and willingness of researchers and research institutes to take advantage of good opportunities to further their vital interests: to promote institutional growth, monopolize research domains, or increase their institute's research autonomy. Occasionally, the investigations also came across situations of politically induced trouble for researchers or research institutes, especially in the studies about medical research and about state-financed research institutes outside of the universities. It is this other side of the coin we want to investigate more systematically now. We hope to complement the institute's theoretical perspective with regard to an aspect which has gained political relevance and also promises additional theoretical insights into the complex relationship between actors within the research system and within the political system. With this, we would like to make a contribution to a political sociology of science.

2 Main Variables and Analytical Framework

Our topic is circumscribed by two main variables: trouble and coping. Having used these terms in their everyday sense in the previous section, we would now like to define more precisely what they mean within the framework of our theoretical considerations.

The basic idea is familiar from psychological studies of the reactions of individuals to so-called "critical life events" such as the death of a spouse, a chronic illness, or becoming unemployed.[5] These stressful life events cause trouble with which the individuals somehow have to cope. Transposing this to our topic, we can start by stating that *trouble* means more than the everyday problems which researchers or research institutes face in their interactions with political actors. Examples of political actors' attempts to instrumentalize scientific research for their own interests, for example, are legion, as are those of researchers in relentless pursuit of adequate financial support for their institutes. But in order for these difficulties to be classified as trouble, they must become critical. The researchers or research institutes involved must perceive the problems as drastic violations of their vital interests. Thus, whether an event is categorized as a source of trouble for an actor depends in the final analysis upon his aspiration level with regard to his relevant interests. For example, if a research institute has no ambition to select its research topics autonomously, perhaps because the institute's corporate identity emphasizes a research mission of supporting public policy-making, even strong political interventions into the setting of the research agenda will not be experienced as trouble, but as "business as usual."

However, although the criterion for classifying something as being trouble for a researcher or a research institute can only be taken from this actor's self-defined identity, an actor may still misperceive relevant events. The factual magnitude of a problem and its magnitude as perceived by the actor concerned can differ significantly, so that an actor may be in trouble without knowing about it, or may at least be in bigger trouble than he thinks, or, conversely, may exaggerate his trouble. Whenever an analytical observer can plausibly argue that a research actor has misperceived his trouble, we have to take this into account. In such a case, one of the interesting questions is why an actor has misperceived his trouble, and for how long.

Finally, an actor's trouble may be very idiosyncratic – for example, if an individual scientist does not succeed in mobilizing a particular project grant he desperately needs to realize some research goal. Such fates will not concern us here. We shall concentrate on trouble that affects at least a considerable number of individual researchers, even though it may not affect all of them with the same intensity.

Coping refers to each reaction of researchers or research institutes aimed at reducing existing trouble. Thus understood, coping is distinguished, on the

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5 Compare, for instance, Haan (1977) or Lazarus/ Folkman (1984).

one hand, from *prevention*. Prevention is only possible when the trouble has not happened yet, but has been anticipated by the actor; if he is actually able to avert the trouble, there is no need to cope. Often, however, the trouble is not foreseeable for the actors concerned, so that they can hardly intervene in advance. On the other hand, coping is distinguished from leaving oneself to one's fate, a response often accompanied by despair. This happens when an actor perceives that his scope of action is so limited that he can do nothing about his trouble. Such fatalistic *suffering*, which can be equated with letting the trouble happen, does not mean that the actor concerned stops acting altogether. But it does mean that he makes no move to change his way of acting intentionally with the aim of reducing his trouble. Although he experiences trouble, he acts as if there was no trouble. An example might be a researcher who writes one application for a project grant after another, is repeatedly rejected by the funding agency, but never tries to improve his chances by switching to another funding agency, modifying the form of his applications, or choosing a new research topic. Of course, an actor's attempts to overcome trouble may be unsuccessful, yielding, in the end, the same result as inactivity would have. But the intentions are clearly different in these two cases.

As long as an actor who endures his trouble has a definite hope that it may be eliminated or at least reduced in the future by someone else's action, his suffering is, in effect, waiting for better times. For example, a powerless actor who knows that some powerful actors are affected by the same trouble as he is, and who expects that they will cope with it successfully and, as a side-effect, will also free him from it, may assume that his suffering will not last long.

If trouble is understood as a growing discrepancy between an actor's actual situation and his aspiration level, there are two possible directions coping can take. An actor may either try to adapt his aspiration level to his changed situation, or he may try to change his situation so that it fits again with his unchanged aspiration level. An example for the first alternative of *defensive coping* might be a professor who comes to terms with his growing teaching load, which has forced him to neglect his research interests, by altering his professional self-identity. Rather than thinking of himself primarily as a researcher, he would now think of himself primarily as a teacher. On the surface, defensive coping is sometimes difficult to distinguish from a fatalistic suffering of trouble. Fortunately, this is not our problem here because we are interested in the second alternative: *active coping*, i.e. an actor's attempt to adapt the situation to his aspiration level. Examples of this would be a research institute trying to get very involved in contract research in order to compensate for a shrinking financial resource base from institutional grants, or an individual researcher from this institute who is strongly devoted to basic research leaving the institute when it shifts over to applied contract research.

It is not only the kind of trouble an actor faces that determines whether he will choose an active or a passive coping strategy and which specific steps he will take: Two additional factors are important. First of all, the troublesome situation itself consists of opportunity structures which shape the actor's room to maneuver – for example, rights to participate in relevant decision-making bodies, the availability of alternative sources for financial resources, or competitive relations with other actors. Secondly, the respective individual or corporate actor's identity, made up of his resources of social influence (e.g. power, money, prestige) and his abilities (e.g. his inventiveness) determines his capacity for strategic action. With regard to corporate actors, the degree to which they are capable of making collective decisions that are binding for their individual members is especially relevant.

Concerning active coping, two subtypes can be distinguished according to the goal of the coping activities. Active coping may, on the one hand, be an attempt to eliminate trouble. If this is successful, active coping will have had the same result as prevention would have had – with a time lag. For instance, research institutes may protest against resource cutbacks, mobilize allies, and thereby pressure the political actors causing the trouble to change their minds. On the other hand, active coping may merely be an attempt to *adapt* to trouble: the trouble itself is taken for granted, and the actors facing it only try to make the best out of a bad situation. The research institute described above, which decides to compensate for the loss of institutional grants by turning to contract research, exemplifies this strategy.

This clarification of our two main variables corresponds closely with psychological or social-psychological theories of personal coping. As we turn to our major analytical focus, the differences in our approach will become evident. Psychological or social-psychological theories of personal coping focus on a *single actor* struggling with his trouble. They try to work out a systematic and comprehensive classification of the different kinds of coping and to analyze which kind of coping an actor chooses in response to the kind of trouble he is faced with, his opportunity structure, and his capacity for strategic action.[6] Sometimes these theories examine the consequences the specific coping reaction chosen by an actor has for him. Psychological and social-psychological studies, however, analyze individual coping reactions *isolated from each other*. For instance, a person with a chronic illness somehow manages his life – but usually in a social context of relevant others who do not share this kind of trouble. Often this analytical perspective is adequate. But there are other situations where a plurality of actors interacting with each other share the same trouble. Then, a new phenomenon arises which tends to be neglected by psychological and social-psychological studies because their point of reference is an individual's psychological condition: the *social interference of different actors' coping reactions*.

To illustrate this type of interference, we can take the example of a small company town in which many people have lost their jobs. Here, it would be worthwhile to look not only at how each affected worker deals with this "critical life event" individually, but also at the aggregate effects of the sum of the individuals' coping efforts, which are directed not only at solving the same problem, but at overcoming *common* trouble.[7] One of the most interesting research questions might then be how the individual coping efforts of the plurality of actors mutually reinforce or weaken each other. It would also be important to find out whether the individual actors perceive these interferences and, if they do, whether this provokes them to coordinate their coping in order to increase its effectiveness. If many of the unemployed persons react by accepting very low wages from all kinds of employers in the region, the aggregate outcome of this might be a ruinous competition among those seeking employment. But if the unemployed become aware of this hazard and are able to organize themselves in order to prevent such competition, they might, in the end, attain a collective bargaining power which would be advantageous for each one of them.

This very simplified example demonstrates what we are primarily interested in: *the aggregate effects of the interconnected coping efforts of a plurality*

6 An excellent example is Erving Goffman's study of how stigmatized persons try to manage their "spoiled identity" (Goffman 1963).

7 In their classical empirical study of the unemployed workers of Marienthal (a small town in Austria) conducted during the Great Depression in the late 1920s, Marie Jahoda, Paul Lazarsfeld and Hans Zeisel combined both analytical concerns (Jahoda/ Lazarsfeld/ Zeisel 1933).

of researchers and research institutes affected by common trouble. From a growing number of studies investigating the reciprocal causal connections between individual actions and their combined structural effects, we are aware that the nature of aggregate effects is often very *complex.*[8] Assuming a simple additive cumulation of single actors' coping efforts is, in most cases, clearly inadequate. To give just one example, consider the situation of researchers competing for project grants that are becoming increasingly scarce. One sensible way the researcher can cope with this kind of trouble is to try to gain a competitive advantage by investing more effort into carefully reasoned grant applications. But if everybody does this, the aggregate effect is definitely not an increase of everybody's chances, but a collectively self-defeating increase in the standards for grant applications. Consequently, for a proper understanding of many empirical phenomena we have to search for theoretically more complex patterns of aggregation.

From this point of view of the respective *constellation of actors* as a whole, we are also able to evaluate more thoroughly a single actor's chances of succeeding with his coping efforts. His relative success or failure, moreover, is theoretically not attributed to his respective actions, but to how these particular actions *match*, within the given pattern of aggregation, with the actions of the other actors involved.

This declaration of our research interest shall now be specified into a set of interrelated theoretical concepts. These theoretical concepts are deliberately not designed to apply only to situations of trouble. They can also be used for the analysis of trouble-free situations, be they situations offering good opportunities to researchers or research institutes, or be they situations classified as "business as usual." In our view, it seems to be advantageous to have one single framework for the analysis of all kinds of relationships between scientific research and politics, instead of designing specific frameworks for specific kinds of relationships. This does not exclude the possibility that the general framework can be enriched by certain specific concepts which apply only to one kind of relationship – for instance, to a troublesome relationship. We certainly aspire to do this, but we will not go very far in this direction right here because we are convinced – as will become clear from our research

8 The stimulating studies by Raymond Boudon or Thomas Schelling (Boudon 1978; Schelling 1978) illustrate this point.

approach sketched below – that such concepts have to emerge primarily from carefully studied empirical cases like the ones compiled in this book.

We begin constructing our layout of an analytical framework by distinguishing two potential *sources of political trouble*: firstly, political actors pursuing a given kind of research policy with troublesome consequences for particular research actors and, secondly, political actors in other policy areas whose actions have troublesome side effects on a given research actor's research conditions. Research policies are not only formulated and executed by the ministry responsible for research, but also by other ministries responsible for economic affairs, defense, or the health care system. The trouble caused by such policies may be intended or unintended. Policies with side effects on research conditions may be educational policies, especially with respect to research conditions at universities, which are often strongly influenced by the teaching load of professors and their assistants, or budgetary policies which may restrict the financial resources available for the promotion of research.

Different levels of actors within the research system may be *affected by political trouble*. The first level is that of the individual researchers. The second is that of the informal or formal groups made up of individual researchers. Informal groups of researchers may become quite large, as exemplified by national or international scientific communities in well-circumscribed fields of research, sometimes referred to as "invisible colleges." At some point, such originally informal groups usually organize themselves formally as scientific associations or sections of them. The most common case of a formal group, on the other hand, is a project team within a research organization. Not all kinds of informal or formal groups can be properly characterized as actors. A group can only be called an actor if it is able, either by a majority rule or by compliance with its leader, to make group decisions each member is bound to comply with. A third level of actors consists of subunits of research institutes, such as departments of a university or divisions of a national laboratory. Again, these organizational units are only actors with respect to the issues they attack with a common will. A functioning formal hierarchy within these organizational research units will ensure that their categorization as actors is valid because their formal leader is entitled to determine the common will. The same holds true with respect to the fourth level of action: research institutes as formal organizations. Finally, there may be a fifth level: groups of research institutes. In Germany, an example of such a group is the

Max Planck Society, which consists of about sixty institutes. These groups of formal organizations work essentially like groups of individuals: They can be – but are not necessarily – actors.

The different levels of actors potentially affected by political trouble are often nested. Individual researchers are often members of informal or formal groups of researchers; these, in turn, are usually parts of organizational subunits of research institutes. These subunits are parts of research institutes which may, in turn, belong to a group of research institutes. But although the different levels of actors frequently fit nicely into one another like Russian dolls, there are not always common interests among them. Neither are the interests on a higher level necessarily determined by the interests at the lower level, nor vice versa. Accordingly, a situation that means trouble for actors on one level may not mean trouble for actors on another level, although the first level is contained within the other. For instance, the closing of a research institute as a formal organization can mean big trouble for its researchers, too, because they lose their research opportunities, not to mention their jobs. But it may be that there are plenty of other excellent institutes where they can continue their work. In this case, the trouble exists only on the higher level. Conversely, if the state agency financing a research institute refuses to allow the institute to give permanent positions to researchers, this may certainly mean trouble for the researchers, especially if job opportunities in the their research field are scarce. But the institute may find this policy beneficial because it allows for some flexibility in dealing with personnel.

Sometimes, even when actors on different levels are affected in the same way by particular political interventions, the reactions on the different levels nevertheless run counter to each other. For instance, a research institute may be faced with political actors threatening to close it down if it does not step up its technology transfer to industry very soon. This certainly may mean trouble for the research fellows of this institute who are interested in basic research. But when the institute as an organization reacts by putting increasing demands on the researchers to engage in transfer activities, the best of them (who will have the best chances of receiving interesting job offers) might leave. This individual coping effort could impair the institute's coping effort, which may vitally depend on the capabilities of these very researchers.

In addition, there can be mild or extreme differences of interests – including different intensities of the same interest – between actors on the same level. For example, an institute's researchers oriented toward basic research will be affected quite differently from those oriented toward applied research when political actors call for an increase, say, in the institute's share of contract research for industry. While this could be major trouble for the first type of researcher, it may well be a good opportunity for the second type to improve his standing within the institute. Another example could be the different consequences which increasing scarcity of federal funding might have on the different kinds of member institutes of the Max Planck Society. If, for instance, the spending cuts apply mainly to the purchase of expensive research equipment, the natural-science institutes would have much more trouble than the institutes in the humanities.

All in all, a single political intervention may mean very different things to different actors within the research system, be they on the same level or on different levels of action. Some may see trouble looming, while others are unconcerned, and still others may see a good opportunity opening up. Those faced with trouble may be affected in the same way, or in different ways. This is the context within which coping occurs as a *complex interplay between political actors causing trouble and research actors affected by that trouble.* Actions causing trouble may produce coping efforts as reactions; in turn, those who caused the trouble may react to the coping, which may bring about new or intensified trouble, thus causing further coping, and so on. For instance, politicians demanding a new orientation of certain research areas according to political priorities not shared by the researchers may provoke evasive reactions: the researchers will pretend to comply with the political demands while secretly continuing to do their own thing as they see fit. When the politicians detect this, they may implement new devices for monitoring research, so that they cannot be deceived again. This may put an end to the evasive tactics the researchers had been using, but it will probably cause, depending on the individual circumstances of researchers, a new series of adaptive reactions. Some researchers may leave the respective institutes and look for new positions where they can better realize their own research ambitions; others may, from that moment, perform their research without any enthusiasm or creativity. Again, the last kind of reaction, which is perceived as work-to-rule by the politicians, may motivate the latter to install additional mechanisms to enforce an adequate level of research output – which may elicit yet another round of reactions by the researchers, and so on.

Such sequences of trouble and coping efforts, which can sometimes go on for quite a long time, have certain *effects on the research conditions.* As stated above, we consider these effects to be complex aggregations of action which is embedded within an institutional context. Because the research conditions are the result of an interplay of many individual, collective and corporate actors on several levels of action, they cannot be traced back to any single actor and his intentions and capabilities. Although this holds true for almost all results of human action when it is triggered and shaped by interdependencies between actors,[9] there are many constellations of actors which are structured so simply that their aggregate effects are evident to any interested observer. Two features of an actor constellation – the degree of compatibility between the intentions of the actors, and variation in the amount of social influence they possess – largely determine its complexity and, hence, the extent to which its aggregate effects are obscured.[10]

The higher the compatibility of intentions among different interdependent actors, the more all these intentions can be realized simultaneously without friction. The scale of degrees of compatibility can, for reasons of simplicity, be divided into two opposites. On the one hand, there are several possible relations of compatibility between intentions: Different actors' intentions can be identical without being competitive, their intentions can be complementary, or their intentions can be indifferent to each other so that none interferes with the other. In these cases, the aggregate effect of the actors' combined actions is comparatively simple, because the actors are headed, more or less, in the same direction. On the other hand, however, there are at least two possible relationships of incompatibility between intentions: The intentions of different actors can be competitive, or they can be antagonistic. In these cases, the aggregate effects often become much more puzzling because the actors are headed in opposite directions and there is no easily conceivable point where their intentions might meet – especially if there are three or more actors involved.

9 As James Coleman puts it, social interdependencies result from a "simple structural fact": "Actors are not fully in control of the activities that can satisfy their interests, but find some of those activities partially or wholly under the control of other actors" (Coleman 1990: 29).

10 The following builds upon general ideas developed in Norbert Elias' studies of "social figurations," which were applied to the topic of unintended results of action by Reinhard Wippler (Wippler 1978: 158-161, 174-175).

But even a high incompatibility of intentions can be overcome quite simply if there is a strong social dominance of one actor or a subgroup of actors with compatible intentions. The greater the difference in social influence is between the actors within the respective constellation, the less relevant the intentions of the other actors become for the aggregate effect. The intentions of the actors without significant social influence are socially neutralized. But the smaller the differences of social influence among the involved actors are, the more puzzling the aggregate effect of their combined actions becomes if the incompatibility of intentions is high.

Applying these general considerations to the constellations of political actors causing trouble and actors within the research system coping with this trouble, we can assume, first of all, that there is a high incompatibility of interests between the political actors and the researchers and research institutes. The extent of the compatibility of interests among the actors affected by the political interventions interests us more, however; as shown above, there are several combinations possible. There may be a high compatibility of interests among these actors, so that they stand united against the political actors. Or their interests may be highly incompatible: Some actors are faced with trouble, while others perceive this very "trouble" (especially if it affects their competitors) as presenting good opportunities for themselves. Or there may be an incompatibility of interests, with all actors seeing trouble, but each in different ways.

Turning to the differences of social influence, we find that political actors have the capacity to influence the actors within the research system significantly, not just by incentives, but also by directives. There may be a clear social dominance of the political actors – in this case, they will have their way. Or, the political actors may need the cooperation of at least some actors within the research system in order to effectively implement their interventions. If this is the case, political actors might make use of the incompatibility of interests among the actors within the research system by playing off those who see good opportunities for themselves against those who see trouble. If all the relevant actors within the research system are faced with trouble, but each is faced with a different kind, the political actors can also make concessions to some, thereby winning them for an alliance against the others.[11]

11 These concessions are a kind of "side-payment" (Scharpf 1991: 20-23).

Such a policy of "divide and conquer" can even work if all relevant actors within the research system are faced with the same kind of trouble.

The extent to which the political actors predominate will determine how foreseeable the structural effects of their interventions will be, no matter how great or small the compatibility of interests among the relevant actors within the research system is. The effects will be those the political actors desire – or, at least, accept – and, consequently, those that are not desired by the actors within the research system who are facing trouble from the political interventions. The latter's efforts at active coping will be futile. There is nothing left for them to do but to bite the bullet and come to terms with the politically induced circumstances. But the more dependent the political actors are on the cooperation of actors within the research system, the more ambiguous this tableau becomes. Now, the structural effects will depend on several factors: whose cooperation the political actors will try to win, who will offer cooperation for what price, and what kinds of social influence can be accumulated in this way. These factors – and the choices of action shaped by them – may all be contingent to some extent; consequently, there may be no clear, stable, predictable outcomes; the outcomes will always be partially accidental. The questions raised by these considerations can only be answered by turning to specific cases and analyzing them carefully.

3 Research Approach

Perhaps the best brief characterization of the research approach we are trying to realize with this book is a combination of two well-known sociological slogans. What we are searching for is a "grounded theory of the middle range."

Robert K. Merton distinguished "theories of the middle range" from grand theories providing "... a complete vade mecum to the solution of sociological problems" (Merton 1949: 165-166). This first element of our approach formulates the goal we want to reach, signalling, on the one hand, theoretical modesty. We want to emphasize explicitly that we are definitely not trying to work out an entirely new, all-encompassing theoretical perspective for all kinds of social studies of science, but a set of theoretical propositions about some specified aspects of a limited area of social phenomena. On the other hand, stating that we are searching for elements of a theory of our particular subject indicates that we are not satisfied with descriptions and explanations of singular historical episodes. We want to go beyond a mere compilation of cases, however well analyzed they may be. By providing a more abstract reflection about the cases and then comparing them, we wish to find general patterns of analytical relationships between trouble, coping strategies, constellations of actors, and effects of coping with trouble on the research conditions.

The formulation of our research goal connotes the course we will take to achieve it: the "grounded theory" approach, as developed by Barney Glaser and Anselm Strauss (Glaser/ Strauss 1968; Strauss/ Corbin 1990), who made a clear distinction between their approach and others tending toward purely deductive theorizing or purely inductive empiricism. Purely deductive theorizing supposes that a complete and sufficiently detailed theory exists that can be used to examine the class of phenomena at hand, so that the subset of phenomena to be empirically analyzed just has to be subsumed under this theory. In this case, there is essentially nothing to be learned from social reality because everything is already included in the existing theory. Such an approach would undoubtedly fail to answer our research questions because there is no comprehensive theory which applies to our phenomena. Purely inductive empiricism, conversely, supposes that there is a tabula rasa regarding the phenomena at hand, waiting to be filled with theoretical concepts and propositions. Such an approach is often as unrealistic as the purely deductive approach: This is certainly true in the case of our research questions. As we have documented here, we have some theoretical ideas about what to look for. These ideas are often still vague, and sometimes there are contradictory suppositions – but not only would it be impossible to pretend we could forget about the already existing knowledge, it would be foolish indeed not to use it as a starting point for our investigation. This is the major message of the "grounded theory" approach: In such a situation of incomplete and insecure theoretical knowledge, one should go back and forth between theory construction and empirical investigation again and again, until the theory consolidates. How often these two steps have to be taken cannot be stated a priori – the moment to stop has come if further empirical work does not reveal any new surprises.[12]

12 Bühler-Niederberger (1985) elaborates this point very clearly.

Our selection of cases to be discussed at the conference was guided by this approach. Each of the contributions dealt with a particular empirical case illustrating a typical pattern of politically induced trouble and coping strategies within the research system. Of course, each case we have selected exhibits only a fraction of the aspects we have sketched. Moreover, we could not hope to offset this deficit fully with our particular selection of cases. With such a small number of cases it is impossible to portray the whole variety of possible constellations of trouble and coping efforts. While trying to reflect the diversity of trouble in research to a certain extent, we had to bear in mind that too much diversity would make it difficult to compare the cases. The diversity results from different national contexts (France, Germany, Great Britain, and the United States), different kinds of research institutions (universities, Big Science centers, other state-financed research institutes), different kinds of political trouble (financial cutbacks, redirection of research programs, institutional change, political regulation), different degrees of success and different effects of the coping efforts.

Let us briefly introduce the case studies. The first two, by Schimank and Braun, deal with resource trouble in the university sector. While Schimank can show that the high degree of autonomy professors enjoy at German universities makes collective coping efforts rather improbable, and that individual researchers must thus resort to adaptive strategies, Braun explains in his comparison of biomedical research in Great Britain and the United States how political trouble is filtered by funding agencies and medical schools before it reaches the individual researcher. Both cases deal explicitly with a multilevel actor constellation.

That prevention of trouble is not only a theoretical idea but also, under certain circumstances, a real possibility is stressed by the two French case studies presented by Krauss and by Musselin and Vilkas. Each case shows that in the extrauniversity research sector in France there is a high potential for successful conflict avoidance and for bargaining between the elites of the research system and the political system. Krauss points, in addition, to the possibility of "mock trouble" staged sometimes when political actors perceive a need to demonstrate activism.

An example of extreme political trouble is examined at two levels in the cases presented by Mayntz and Wolf, who analyze the dissolution of the East German extrauniversity research system as a consequence of the unification of Germany. Looking at trouble at the highest level, Mayntz shows that the

East German Academy of Sciences failed to survive as a corporate actor because of misperceptions, a lack of social influence and allies, internal conflicts, and permanent pressure from a changing political environment. Wolf describes how particularistic coping methods enabled certain institutes, research groups and individual researchers within the Academy of Sciences to be partly successful in finding a new role in the unified German research landscape when the Academy disappeared as a corporate actor. Stucke's case study also involves the effects of German unification on the research system – in the West. Analyzing how the German National Research Centers have dealt with the most severe cutbacks in their history, he concentrates especially on how the interplay of coping at four levels of actors affects the respective coping strategies.

The next two case studies concern trouble as a consequence of political regulation. Hasse and Gill argue that in the case of genetic engineering in Germany, regulative trouble was not only triggered but also continually reinforced by public distrust toward the biotechnological research community and by an erosion of support from industrial users of research results. The coping activities divided the scientific community – and often even individual institutes – into opposing factions of "hardliners" and "moderates," who mutually weakened each other's efforts. Analyzing the decisions to build new research reactors in Berlin and Munich, Gläser et al. also illustrate the relevance of intrascientific dissent, which, in their case, made it relatively easy for political actors to drag out the licensing procedure or even refuse to license the reactors. Moreover, Gläser et al. stress the extremely limited coping repertoire available to research actors faced with regulative trouble.

Weyer's case study of strategic action and actor network dynamics in space policy concludes the empirical section of this book. He shows that even successful coping may engender new trouble in the future, and that coping and trouble must be analyzed not only with respect to specific focal actors, but also by considering the whole social network in which the actors are embedded.

We take a final, comparative look at the empirical material in our concluding theoretical examination of the cases. Here, we further clarify each case analytically and group the cases according to similar patterns in order to derive theoretical generalizations which can be divided into two categories. Firstly, we expect to find conceptual generalizations which allow us to classify the analytical dimensions "trouble" and "coping." While these conceptual generalizations will be descriptive, we also hope to find, secondly, causal generalizations: propositions about general patterns of coping with trouble and its effects on research conditions. These generalizations will be explanatory.

Most of the contributions of this reader were originally presented at a conference entitled "Coping with Trouble" which we organized at the Max-Planck-Institut für Gesellschaftsforschung in Cologne in November 1992. We gratefully acknowledge that the conference was financed and hosted by our institute. Most of the chapters based on a conference paper profited greatly from the lively, inspiring discussions at the conference. Thus, we editors and most of the authors are heavily indebted to the discussants: Erhard Friedberg (Centre nationale de la recherche scientifique, Paris), Dorothea Jansen (Universität Bochum), Wilhelm Krull (Wissenschaftsrat, Cologne), Werner Meske (Wissenschaftszentrum für Sozialforschung, Berlin), Arie Rip (Universiteit Twente), Peter Weingart (Universität Bielefeld), Tom Whiston (Science Policy Research Unit, Brighton), David Wilsford (Georgia Institute of Technology, Atlanta), Hans-Willy Hohn and Fritz Scharpf (Max-Planck-Institut für Gesellschaftsforschung, Cologne).

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How German Professors Handled Increasing Scarcity of Resources for Their Research: A Three-Level Actor Constellation

Uwe Schimank

In Germany, as in many other countries, research coexists with teaching at universities.[1] Both tasks are, moreover, only situationally differentiated. There are only few roles or organizational subunits specializing in just one of these tasks. Professors in particular have to devote their attention to both tasks, which often leads to conflicting demands on their time budgets. In addition, most of the financial and personnel resources of German universities are shared by research and teaching as a common pool. Less than one fifth of the universities' resources are separately budgeted funds for research projects, while more than four fifths are general university funds from government which do not specify separate budgetary categories for each of the two tasks.[2] This *common pool of resources for teaching and research* establishes a zero-sum relationship between teaching and research. Since to the general public,

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This contribution sketches some main results from my extensive study of the resource problems of university research in West Germany since the mid-1970s; see Schimank (1993) for an extensive presentation. In addition to consulting other data sources, I conducted a survey of a representative sample of professors, asking them about some important aspects of their research conditions. The results, parts of which are used here, are described in Schimank (1992). I also carried out extensive unstructured interviews with about 30 professors from different academic fields who conducted more than the average amount of research, and with officials from the relevant government agencies and the organizations which politically represented the interests of universities. Findings from these interviews are also used here. To protect the anonymity of my interviewees, they are cited only by the number of the respective interview.

1 See Braun/ Schimank (1992) for a theoretical conceptualization of the following.

2 This rough calculation is based on data for the mid-1980s in Wissenschaftsrat (1988: 234). No significant changes have occurred since then.

especially students and their parents, to important interest groups like business associations and labor unions, and to politicians alike teaching usually is the much more important task of the universities, this zero-sum relationship implies structurally built-in resource trouble for research. When the amount of general university funds required for teaching increases faster than the funds themselves, the share allocated to research diminishes correspondingly.

Since the beginning of the last century when German universities adopted research as their second task besides teaching, this kind of trouble arose repeatedly in German university research. The last time it started was in the mid-1970s. I will focus here on the period from the mid-1970s until the end of the 1980s, although the trouble is still going on. From 1975 to 1989, the number of students at German universities increased by two thirds. Even if one allows for the fact that the demand for teaching did not grow to quite the same extent, the universities had to bear a huge increase in this demand, while the established posts for personnel and the institutional funding stagnated.[3] As a common pool of resources for teaching and research, these general university funds for personnel and finances were consumed more extensively by teaching, with shrinking leftovers for research.[4] This is corroborated by the professors' appraisal of their resource situation. About 40% of the professors estimated in 1990/91 that their general funds had worsened with regard to research during recent years (Schimank 1992: 23-26).

Compensating these losses by falling back on separately budgeted funds became more difficult, too. The total amount of separately budgeted funds to the universities grew from 1975 to 1986 by about one third.[5] In one of the German states, North Rhine-Westphalia, separately budgeted funds increased by nearly three quarters between 1982 and 1990.[6] Nevertheless, separately budgeted funds did not suffice. Thus, on the one hand professors depended increasingly upon separately budgeted funds. Actually, between 1988

3 See the data in Wissenschaftsrat (1988: 234), BMBW (1990: 139, 260; 1991: 218). Financial increases during these years were only effects of inflation.

4 This was corroborated by my unstructured interviews with professors. Some of them even had to admit that they had to use practically all of the general funds allocated to them – the financial assignments for themselves and their assistants as well as their assistants' work capacity – for teaching (interviews 20, 21, 40).

5 My own calculations on the basis of deflated data from DFG (1975: 271); Wissenschaftsrat (1988: 234-239); BMFT (1990: 340-341).

6 My own calculations, based on deflated data from MWF (1992: 20).

and 1990, nine out of ten professors had to rely on some amount of separately budgeted funds (Schimank 1992: 26-27). On the other hand, these funds expanded much more slowly than the demand for them. This applies especially to the *Deutsche Forschungsgemeinschaft* (DFG), the most important funding agency for universities, which had been able to grant about two thirds of the total sum applied for in 1975 but could grant only half in 1988 (DFG 1975: 64, 195; 1989: 21).

On first sight, it seems plausible that an increasing demand for teaching not only consumes resources but also working time formerly available for research; if this reduces the amount of resources needed for research, the resource trouble for research might be significantly reduced. Indeed, most professors had to come to terms with a considerably higher teaching load. But, at first sight surprisingly, the common notion in political debates about German universities that professors could devote less and less time to research was not true. On the contrary, while research on average made up 23% of their working time budget in 1976/77, it increased to 28% in 1990/91 (Schimank 1992: 16-17). Evidently, the professors succeeded in neutralizing the time pressure of an increasing teaching load by reducing the quality of teaching, standardizing teaching, standardizing examinations and making them easier, informally delegating teaching duties to assistants, and sometimes coupling teaching and research activities more tightly.[7] Actually, on the average professors even gained a little more time for their research activities. If many professors had been prevented from doing as much research as before by their increased teaching load, the resource troubles for those who still had been able to do research would have been less because there would not have been that much demand for separately budgeted funds. Things being as they were, however, almost everybody continued to need resources for research.

This small selection of indications of the resource troubles of German university research must suffice here. Each point could be described in more detail and documented with more empirical proof, especially with regard to differences between various academic fields; further empirical indicators could be added. But, essentially, the overall picture would not change. Thus, for more than fifteen years German professors had to cope with gradually increasing resource trouble affecting their research conditions. I will describe and

7 Findings from my unstructured interviews with professors illustrate all of these practices abundantly.

explain the unsatisfactory, but unchanging pattern of coping activities exhibited during that time as a *three-level actor constellation.* The three levels of action are: uncoordinated reactions of individual professors who each tried to make the best for themselves out of their own particular troublesome situation; collective decisions about the distribution of general university funds within universities, especially on the faculty level; and the interplay between government actors and certain corporate actors who represented the interests of the universities in the political arena. As I will show, action on the level of collective decisions within universities was decisive for what was possible on the political level; and both levels of action determined what happened on the level of individual reactions to trouble.

1 Nonredistributive Intrauniversity Allocation of General University Funds

It is certainly not entirely absurd to expect that, as resources for research from the general university funds declined, the yearly allocation of these resources within the university, especially on the faculty level, might have been affected. Several criteria for a redistribution of resources might have been considered. The professors with a high intrascientific reputation for outstanding research could have been especially favored, exempting them as much as possible from unavoidable sacrifices of resources. These sacrifices, instead, could have been demanded from those professors with a low research productivity. Or professors who did research in fields of high importance to extrascientific users of research results could have been spared from inordinate resource losses. Or older professors who had accumulated a rather large resource base over the years and, perhaps, were not doing that much research anymore, could have been forced to relinquish some of their resources. Younger professors, on the other hand, whose productivity was at its peak level but whose resource base was comparatively small could, for example, have been excluded from resource losses. These and other imaginable criteria for the preferential treatment of certain professors, of course, could have been combined if the need for a more complex appraisal became evident. The criteria also could have been softened so that the degree of redistribution could be determined according to what seemed to be reasonable.

As these brief remarks indicate, there are various potential good reasons to react to an increasing scarcity of resources for research with a redistribution of general university funds within the university. Moreover, redistribution very well could have been designed in a way which would not have brought about unbearable individual losses of resources and an intolerable increase of resource inequality among professors. But, in fact, almost nothing of this kind happened in German universities. The intrauniversity allocation of the general university funds did not redistribute resources according to any of the possible criteria mentioned. Instead, significant losses of resources as well as small intermediate or local gains were most often distributed proportionally according to the share of resources each professor had at the time of the respective decisions.

To understand this *status quo-oriented distributive logic*, we must look back for a moment at how general university funds were allocated to the German universities by the federal states before the university reforms began in the late 1960s. Until then, a bilateral funding relationship between each professor and the state's ministry of education existed. When a professor got an appointment for a chair at a particular university, and afterwards whenever he was offered a chair from some other university and had to decide whether to stay or to move, he could bargain with the ministry about the general university funds that were to be dedicated to him personally. The bargaining agreements were binding for the future. The ministry was unable to reduce what it had once granted to a professor. Thus, allocation of general university funds consisted in a multitude of bilateral binding commitments. It was, therefore, highly inflexible in the social as well as in the temporal dimension. A redistribution of the general university funds given to a university was only possible by allocating increases selectively.

In this traditional funding regime, within a university no collective decisions about the distribution of general university funds were taken. The university's organizational potential for self-governance remained untapped. For several reasons not to be dealt with here, government tried to increase the universities' self-governance by instituting the university reforms. Since then, general university funds have been given in a lump sum to the university, which has to allocate them internally in two stages. General university funds must first be divided among the departments, and then, within each department, among the professors. Thus, on the level of the university as a whole as well as on the department level, collective decisions regarding distribution of funds are required. Universities have thus acquired the authority and the responsibility for the internal allocation of their most important resources. But, as explained above, this radical institutional change has had almost no redistributive effects. There were two reasons for this: the lack of resolve on government's part, and the widespread attitude among the professors that it was best to cooperate with one another.

Under the old funding regime of bilateral bargaining, the state's ministries of education had got into a ruinous cycle of outbidding each other in their competition for professors. Professors were able to exploit this competition and acquire very generous supplies of general university funds for themselves. At the end of the 1960s, the joint commission of the states' ministries of education (*Kultusministerkonferenz*, KMK) made a collective decision to restrict this ruinous competition by prohibiting the dedication of general university funds to individual professors. This evidently was integrated into the new funding regime established shortly thereafter according to which the universities themselves had to allocate general university funds internally. But the formal renunciation of personal resource dedications was not in tune with the ongoing competition for professors between the states. In this competition, such dedications have remained the most widely used weapons. Under the pressure of competition, a lack of resolve prevailed among the states' ministries of education (interview 52). As a consequence, government itself has acted contrary to its own intention, which had been to give universities a wide area of discretion for redistributive decisions about resources. At most universities, a significant amount of the general university funds – sometimes well over half – are still dedicated to individual professors and, hence, cannot be handled any more flexibly than under the old funding regime (interviews 16, 20, 39, 56).

But even the segment of the general university funds which was at the disposal of intrauniversity collective decision was only rarely redistributed. Instead, a mutual attitude of cooperativeness among the professors prevailed, causing them to refrain from challenging the status quo of resource distribution as it had emerged from the past. Losses of general university funds as well as rare increases were distributed proportionally. In effect, this resulted in an implicit mutual non-aggression pact among the professors (interviews 1, 2, 4, 6, 11, 12, 19, 23, 42, 43, 45). Undoubtedly, most of them suffered significantly from the scarcity of general university funds. Moreover, in any university everybody knows about certain departments or professors whose research productivity is low but who nevertheless have considerable resources for research at their disposal. Still, these resources are not taken away from them by their colleagues who have a much more legitimate resource demand and who would be able to decide collectively on a redistribution of these inefficiently allocated resources.

There are several good reasons for professors to act in this way, as odd as it may seem at first sight:

1. The conflicts associated with challenging the established distribution of resources produce emotional stress, especially on the faculty level where one literally meets one's opponents every day.
2. Most professors are not experienced in organizational micropolitics. Socialization as a scientist does not include acquiring such skills, and often even breeds a certain contempt for the "dirty tricks" associated with them.
3. Conflicts arising from redistributive efforts destroy the collective influence of the department or university needed against enemies outside. In difficult times when the state ministry of education permanently threatens to reduce the general university funds and the universities have to fight for additional funds, internal conflicts are clearly out of place.
4. As risk-aversive actors, professors are well-advised to refrain from redistributive initiatives which might trigger future revenge. And even if no revenge is taken, establishing redistribution as a possibility of action always implies that one might be a victim of it oneself some time in the future.
5. All these reasons why a professor is better off not pressing for redistributive resource decisions even if they are to his present advantage have a strong basis as long as he sees good chances for himself to acquire the resources he needs as separately budgeted funds. By this, he substitutes a comparatively comfortable anonymous competition according to scientific criteria for the politicized face-to-face conflicts within the faculty or university.

These five reasons strongly overdetermine a professor's attitude of cooperativeness toward his colleagues. As long as most professors are motivated by at least one of these reasons, this is sufficient to bring about this implicit mutual nonaggression pact.

Government's lack of resolve, produced by the competition for professors among the state education ministries, together with the professors' mutual attitude of cooperativeness toward each other arising from one or more of the five reasons mentioned above, jointly brought about the nonredistributive nature of intrauniversity allocation of general university funds. Thus, scarcity of the resources for research from these funds was usually divided evenly among professors. Collective decisions about resource allocation made everybody worse off. It was only under exceptional circumstances that a professor had the opportunity to cope with his resource troubles on this level of action. Coping, therefore, had to happen on other levels. I will now turn to the level of research policy, where government actors and advocates of the universities interacted.

2 Mutual Blockade between Government and Advocates of the Universities

From the beginning of this troublesome situation, there was a clash between government and the corporate actors representing the interests of the universities at the political level. The goals of both sides were mutually exclusive.

The main corporate actors functioning as advocates of the universities were the West German Rectors' Conference (*Westdeutsche Rektorenkonferenz*, WRK),[8] representing the universities, the DFG mentioned above, and the Association of University Professors (*Deutscher Hochschulverband*, DHV), a professional association of professors. In the early 1970s, these advocates began to criticize political indifference toward worsening research conditions at universities that had resulted from the increased teaching load. The resource trouble with all its implications was pointed out again and again. This criticism evolved into demands for far-reaching *compensation*. Essentially, the manifold and varied expressions of the advocates' opinions boiled down to a quite simple recipe. Increases were called for, primarily in the general university funds, secondarily in the separately budgeted funds (especially from the DFG), so that autonomous research would be possible on a satisfactory level for each professor. Obviously, this would have meant huge increases,

8 Its new name since 1990 is the University Rectors' Conference (*Hochschulrektorenkonferenz*, HRK).

as the advocates were well aware. To give just one recent example: In 1992 the HRK estimated that about 30,000 additional established posts for scientific personnel were needed to restore approximately the situation of the mid-1970s (HRK 1992: 14). While conceding readily that this could only be accomplished in a medium- and long-term perspective, the HRK left no room for doubts about how necessary such an increase was.

Such demands were the coping efforts on the corporate-actor level. The advocates of the universities had neither power nor resources of their own that could compensate for the worsening of the research conditions. They could only articulate the trouble and try to persuade the political actors to do something about it. For this purpose, they brought normative arguments to bear, pointing out that professors have not only the legal right to perform research, but, indeed, an obligation to do so. They also employed utilitarian arguments, emphasizing that a highly industrialized, export-dependent high-tech society like Germany cannot afford to do without university research on a large scale.

The Federal Ministry for Research and Technology (*Bundesministerium für Forschung und Technologie*, BMFT) – which is an important funding agency – and the state education ministries had plans for the universities which differed considerably. As early as 1972, it was stated in the *Bundesbericht Forschung IV*, a research policy report issued every four years by the federal government, that there were "... still very unclear ideas ... within the universities ..." (BMBW 1972: 15, 63-64, translation by the author) about their role within a research system planned and guided by government according to its new emphasis on "demand-oriented research."[9] One of the two important goals of government was to eliminate this lack of clarity. University research programs were expected to be oriented more closely than in the past toward the types of new knowledge required by firms and institutions in government and the private sector. The other important goal was to raise the quality of university research. This goal was first expressed by the Science Council (*Wissenschaftsrat*), an advisory board made up of representatives from the states and the federal government, and from the universities and the research institutes outside the universities. Especially in its proposals made in 1979 concerning the research conditions in the universities the Science

9 An expression coined at that time by the federal minister of research and technology, Horst Ehmke (translation by the author).

Council denied that the university funding was inadequate, stating instead that there was a significant lack of quality in university research (Wissenschaftsrat 1979: 17-19). This view was adopted by government.

Both goals, in conjunction with the fiscal restrictions, implied that government was not only unwilling, but also unable to fulfill the demands of the advocates of universities. Instead of providing a comprehensive compensation for the loss of resources which university research had incurred, government favored a *redistribution* of the reduced resources to those professors whose work was either of high interest to the government or private sector, or of high quality or, preferably, both. Thus, government wanted to make a virtue out of necessity: By increasing the competition for scarcer research resources among professors, research that was useful for the government or the private sector or was of high quality would benefit, while useless and mediocre research would be eliminated. In time, this would bring about an overall transformation of university research, the highly controversial consequence of which would be that many professors would cease conducting research altogether for want of resources. This would amount to the factual elimination of the traditional German "unity of teaching and research." One of the reasons government tacitly accepted such consequences was because, as a useful side-effect, they would make available the additional personnel required for teaching.[10] The great majority of professors had to resist these prospects, of course, because most of them were highly interested in maintaining their research opportunities and only a few could be sure that they might not lose them if such measures were taken.

This incompatibility of viewpoints was defused somewhat during the second half of the 1980s, but not enough on both sides to allow for effective compromises. Government actors came to accept the universities' claim that the resources for research were far too inadequate altogether. As a result, some special programs were initiated to alleviate this lack of resources, in-

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10 In 1983, Eberhard Böning of the Federal Ministry of Education and Science (*Bundesministerium für Bildung und Wissenschaft*, BMBW) openly criticized: "... [W]e are trying to translate the idea of the unity of research and teaching too perfectionistically from the Humboldtian university to today's university." He concluded that it would be best to "... say farewell to the idea that each professor" can be expected to conduct research with consistent intensity throughout his career (Böning 1983: 55-56, translation by the author).

cluding several programs to enable the universities to offer young talented researchers at least temporary positions, and 5% annual increases of the DFG's budget for five years. When the states' ministries of education together with their ministries of finance did finally admit in the early 1990s that the universities' demands for strongly increased general funds were basically justified, they did not hesitate at the same time to make it quite clear that the states' financial capacities were overtaxed and that the federal government would have to step in and take action. But the federal government's finances were also very limited because it had to pay the biggest share of the huge costs of German reunification. Only Baden-Württemberg, a relatively prosperous state, was able to implement a special program for an improvement of its universities' general funds (*Stuttgarter Zeitung*, 21 November 1991). Financial scarcity, thus, severely restricted the government's ability to compensate.

Turning to the other side, we find that some advocates of the universities also cautiously began to adopt government's concerns about the performance deficits of university research. As early as 1977, for instance, Werner Knopp, the president of the WRK, had proclaimed that in future only qualified university research could and should be preserved. He came to the conclusion that traditional claims for resources had "... to be reflected critically – even self-critically ...": "What is necessary here is the courage to differentiate according to the criterion of quality" (Knopp 1978: 39-40, translation by the author). In the debates about the necessity to intensify competition for resources between universities and between professors which started in the early 1980s, there were also some voices from the universities signalling partial approval of government's point of view. For example, the new president of the WRK, Theodor Berchem, declared in 1983: "We will have to ask how to distinguish good research from bad, and how to react adequately to this distinction" (WRK 1983: 56). But these had to remain lip-services paid to government by the advocates of the universities or by individual professors. The universities were unable to live up to such promises because, as shown above, collective decisions within the universities about the allocation of general university funds refrained from the redistributions which would have been necessary to promote research of high quality and of high economical or political usefulness.

Therefore, the constellation of government actors and advocates of the universities resulted in a mutual blockade. For fulfillment of their demands, the advocates of the universities depended on government actors. But these actors were unwilling and unable to provide comprehensive compensation for the resource losses to university research caused by the increasing consumption of general university funds by teaching. In order to realize their goals of improving the quality and societal usefulness of university research, however, government actors also depended on the universities. In order to serve as implementation agents for these goals of research policy, and to redistribute resources internally, the universities would have had to have a capacity for self-governance they in fact lacked: they therefore blocked government's efforts. Thus, on the political level of action, both sides increasingly frustrated each other.

The outcome of these coping efforts on the political level shaped the situations of individual professors. Since their advocates failed to obtain a far-reaching compensation for their worsening research conditions, the professors had to rely on individualistic coping efforts. Each one of them was forced to try to take care of himself.

3 Competition among Individual Professors for Separately Budgeted Funds

For a large majority of professors, general university funds were not sufficient as resources for their research activities. 83% of all professors declared in 1990/91 that they needed separately budgeted funds for their research. Only 17% stated that the availability of separately budgeted funds was not an important prerequisite for their research work (Schimank 1992: 28-29). This small group consisted of three subgroups of professors: those whose general funds were sufficient for their research activities, those whose inability to cope with an increasing teaching load forced them to give up research altogether, and those who were indifferent to research.

The first of these subgroups deserves a closer look here. It was composed mainly of professors in academic fields where research requires relatively small financial resources. Typical fields for such "armchair research" which usually needed nothing but a good library were mathematics (interview 1), philosophy (interview 15), the legal sciences (interview 47), and even some parts of the engineering sciences (interview 2). These professors sometimes asked private foundations or firms for donations to compensate for the declining general university funds for their library or for travel costs. This was all they needed in addition to their general funds. Even in these academic fields, however, changes in the way research was conducted often made research more resource-demanding. In mathematics, for example, additional resources became necessary, though still on a comparatively low level, for computer facilities which opened up new ways to solve theoretical problems (interview 1). In many academic fields, as the expenditures for equipment and personnel required for empirical research grew hand in hand with progress made in developing new theories, the niches for inexpensive research became increasingly rare.

In some academic fields, however, professors could at least alternate between doing resource-intensive research or less expensive research. In archeology, for example (interview 4), one branch of research is philologically oriented, while another involves costly excavations. Professors with such an alternative had an escape route when resources became scarce. But all in all, only very few professors were in such lucky circumstances. This reflects the path-dependency of individual research careers which, as scientific specialization increases, sharply narrows down the options remaining open for researchers. A researcher who is on a certain track of research has usually invested so much time and effort in mastering this track's difficulties that he will think twice before switching tracks and starting anew.

The need for separately budgeted funds in addition to the general university funds varied also with a professor's bargaining position when he was appointed to his professorship or, later, when he got an offer from another university (interviews 1, 46, 47). Professors endowed with plentiful resources, either by good luck or because they were excellent researchers who could demand such a resource base, were less pressed by resource trouble than others. On the other hand, in many academic fields a professor's proven ability to acquire separately budgeted funds had gradually become an important criterion for the respective faculty's recommendation to appoint him to the professorship in the first place (interview 24). This points to the fact described above that for most of the professors separately budgeted funds had become absolutely necessary for their research. The general university funds were especially scarce with regard to capital expenditures so that professors had to pay the costs for new research facilities or for necessary repairs from separately budgeted funds. This sometimes even resulted in alibi projects whose only covert purpose was to get certain types of equipment (interviews 20, 23, 40).

The sources of separately budgeted funds had diversified since the mid-1970s (Schimank 1992: 27-28). Private foundations, newly established pools for separately budgeted funds on the state level, and the European Community (EC) became increasingly important. Still, the DFG remained the most important distributor of these funds in all academic fields. There is no other funding agency without any restrictions for the subjects of proposals. Thus, the DFG is the only source of separately budgeted funds for those professors whose research topics do not fit into the programs of any of the other funding agencies. Quantitatively, second to the DFG was the BMFT, whose research promotion is concentrated in the natural sciences, engineering, and medicine. Unfortunately, these two "big spenders" experienced the highest scarcity of funds. The DFG not only had to suffer, as already mentioned, a dramatic decline of the proportion of grants applications it was able to fund. Additionally, in certain years even money already granted to professors had to be cut back. The BMFT, which was often criticized for concentrating its research promotion too much on a few technological fields like nuclear energy and space technology while neglecting many other promising fields (interview 20), had to cut back spending, especially in these other fields, because it had to fulfill huge long-term commitments in nuclear and space research.

The professors who needed separately budgeted funds individually tried to cope with this situation in four ways. Firstly, professors still applied for funds at the DFG and BMFT, but increased their efforts, either by writing more than one research proposal at a time instead of just one, hoping that at least one would be granted (interviews 18, 19, 20, 22, 40, 43), or by writing more carefully argued and extensive proposals, hoping that these would be more persuasive (interview 16). Both practices were time-consuming, and became even more so because the periods for which funds were granted were reduced by DFG and BMFT as a reaction to their scarcity of resources. This meant that, as one professor put it, he now wrote four grants proposals a year while some years ago he had written one every two years (interview 21).

To diversify one's sources of separately budgeted funds beyond the DFG and the BMFT was a second way of coping with scarcer resources from these two most important funding agencies. The private foundations, which were especially important for the social sciences and humanities and for the medical sciences, were one source which could partially make up for the decrease in separately budgeted funds from DFG and BMFT. Another source were the pools for separately budgeted funds at the ministries for research which were founded in several states during the 1980s. Since the end of the 1980s, the BMFT in particular pointed to the EC as a source of separately budgeted funds to which German universities had paid too little attention, compared to universities from other EC member countries (Wissenschaftsrat 1988: 46-52). The main reason for this, however, was that German professors did not need the EC as an additional funding agency as long as the funds from other funding agencies sufficed. When this was no longer the case, they were referred to the EC, but without adequate information about programs of research promotion and application procedures; standing outside the informal circles of EC clients, German professors found this to be a very hard road (interviews 17, 20, 25, 39, 43). Moreover, the realization that the EC would only be able to grant less than one fifth of the total money applied for along with the very complicated application procedures discouraged German professors from submitting applications. The frequent partitioning of projects into many short segments, each requiring a new application, was also a deterrent, as was the belief that scientific quality as a criterion in grants decisions was often superseded by regional considerations in favor of South European countries.

Another attractive source of additional separately budgeted funds was contract research for firms or government agencies. Ranging from small studies to large-scale projects, contract research had a long tradition in many fields of science. The fields of engineering, agricultural sciences and medicine were most likely to be engaged in this type of research. Due to the scarcity of resources available for research from the general university funds and the increasing difficulties encountered in trying to acquire separately budgeted funds from other sources, many professors in these fields were forced to intensify contract research; others who had shunned this type of research altogether up to this point had no choice but to begin conducting it (interviews 1, 3, 19, 20, 22, 23, 25, 40).

A third way to partially cope with the decline of separately budgeted funds from DFG and BMFT was to intensify research cooperation with certain government-financed research institutes outside of the universities, especially with institutes of the Max Planck Society (*Max-Planck-Gesellschaft,* MPG) and big science centers. This cooperation was called for frequently by the WRK. As early as 1977, a resolution was formulated by WRK and MPG concerning the desirability of a further increase of research cooperation. Similar joint proposals were made by the WRK and the association of big science centers. Quite recently, in 1991, the Science Council documented the present state of the cooperation between universities and big science centers and recommended a number of improvements (Wissenschaftsrat 1991). Such cooperation was especially interesting for professors because these research institutes outside of the universities usually had much better research facilities. Thus, research cooperation frequently meant nothing more than being allowed to participate in the use of these facilities (interviews 20, 23). The genuinely cooperative research activities that did occur tended to date back to a time when professors were not being nudged toward cooperation by resource trouble (interview 12).

A fourth way of coping with resource trouble was to increase the use of students as an extension of a professor's research staff. In many fields of the natural, engineering, and agricultural sciences, it had become quite common for students writing their final thesis to select their topic from lists compiled by the professors, reflecting the latter's research interests (interviews 1, 12, 16, 38).[11] Often research consists mainly of extensive experimental work within the framework of a theory which is finished in general and has to be worked out in detail; advanced students are competent to do this routinized research and to deliver useful contributions to a professor's research program. These are frequently very time-consuming research activities requiring neither the theoretical creativity nor the extensive knowledge of the field which only an experienced researcher possesses. The students' tasks consist mainly of developing experimental designs and, later, observing and measuring the processes. This is perhaps best exemplified by chemistry, where a particular research problem is often attacked simultaneously from different approaches, each assigned to one researcher within a professor's group; some of these researchers will be advanced students working on their thesis. In contrast, many research problems in theoretical physics are too difficult or too complex to be distributed among advanced students, so that in this field such a coupling of research and teaching was not possible (interview 43).

Of these four ways of coping with the resource trouble, the third was available only to very few professors, and the fourth could only yield marginal improvements. Thus, the first two ways were by far the most important.

11 See the extensive empirical study analyzing this situation at the beginning of the 1970s by Wilhelm (1978).

Both of them resulted in an increased competition among professors which forced them to intensify their efforts to acquire separately budgeted funds. These efforts consisted not only of writing grant proposals, but also of manifold activities of social networking with influential colleagues or officials from the funding agencies. The success of these efforts, however, became less likely because the demand for separately budgeted funds increased stronger than the supply; and even if a professor was successful, he was usually granted a smaller amount of resources for a shorter period of time. In other words, an increase of invested efforts corresponded with a decreasing return on the investment. In addition, no professor knew how much effort others were investing. This mutual ignorance between competitors motivated each one to redouble his efforts. For a risk-aversive actor, this is a rational way of acting under such circumstances. Applying for separately budgeted funds was like bidding at an auction where one does not know how much one's competitors are bidding. If one is desperately in need of separately budgeted funds, investing as much effort as possible maximizes one's chances for success. But such individually rational action results in collectively undesirable, ruinous competition. The mutual pressure between professors brought about increased standards for successful applications, which was against everybody's interest because it required even more effort. Thus, if the professors could have committed themselves to not to try to beat each other by perfecting grant applications and cultivating good connections, this would have been helpful to everybody. Everybody then would have had more time to actually conduct research, instead of having to spend time acquiring resources for research.

The situation was a *Prisoner's Dilemma* (Colman 1982: 101-104, 113-136) where no professor could be sure that all others would refrain from trying to get a competitive advantage. But if one must anticipate that some, at least, will defect from a collective self-restraint, it is better to be one of the defectors. With everyone taking this into account, the escalation of competition which harms everyone begins to take its inevitable course. This dynamic was accelerated by two differences between the professors:[12] Those in greatest need of separately budgeted funds were more likely to start the race than others, as were those who – rightly or wrongly – felt they had good chances of winning such a race. As a consequence, professors became "professional

12 These differences are "threshold levels" in Mark Granovetter's sense (Granovetter 1978).

application writers," as one of them concisely expressed it (interview 18), finding correspondingly less time for genuine research work.

Thus, individual coping efforts resulted in increasing collective frustration. A growing number of professors were unable to keep pace with the intellectual and social efforts necessary to apply successfully for separately budgeted funds, and those who did keep pace found themselves devoting more and more energy to an intrinsically unsatisfactory activity.

4 An Incremental Way Out of a Self-Replicating – and Unsatisfactory – Coping Pattern

As my analysis up to this point shows, the pattern of coping activities on the three levels of action was a stable equilibrium. It reproduced itself again and again because none of the actors involved was able to improve his situation unilaterally. More precisely, the equilibrium of the constellations of actors on the level of intrauniversity allocation of general university funds strongly determined the equilibria on the other two levels. Because an implicit nonaggression pact between the professors on the middle level prevented redistributions of general university funds, a mutual blockade between state ministries and advocates of the universities ensued on the political level. This, in turn, increased the demand for separately budgeted funds on the level of individual professors, which in turn brought about an intensified competition between professors.

This three-level equilibrium was unsatisfactory for almost all of the actors involved. Government actors, on the one hand, did not succeed at increasing the universities' research performance either with respect to intrascientific quality or with respect to extrascientific utility. A majority of professors, on the other hand, suffered from resource trouble because their individual coping efforts could at best reduce, but not totally compensate for, their losses. In addition, professors had to make the efforts necessary for coping. Thus, professors were worse off than before the trouble started; and government actors could not achieve the strongly desired improvements. But whereas professors had no chance to get out of this unsatisfactory equilibrium, government actors did have an option for at least a small, slow way out. They began to take advantage of this opportunity in the mid-1980s.

Government's option amounted in effect to bypassing the obstruction of redistribution that prevailed in the intrauniversity allocation of general university funds. Whenever resources became available, government actors were able to distribute them according to their own discretion. Although the total amount of resources did not grow during the period under consideration and no additional resources therefore became available for redistribution, resources which had been dedicated to a professor did become available on a small scale whenever that professor left his post, either to retire or to take on a new job. While taking away a vacant professorship from a department or a university, including the finances and the established posts attached to that professorship, does cause conflicts, it is relatively easy. First of all, there is no one occupying the chair who can claim any specific rights to it. Secondly, although a department or a university usually tries to preserve its resource base, if it is forced to give up some of it, it will tend to choose resources which do not belong to anyone at the moment.

Since professorships became vacant from time to time, the states' ministries of education were able to collect up these unclaimed resources and redistribute them. This occurred sporadically until the mid-1980s, when ministries of education in several states, seeing an opportunity to further their research-policy goals, started systematically redistributing resources by building up special discretionary pools of general funds (Wissenschaftsrat 1988: 43-46).[13] Baden-Württemberg was the forerunner (*Stuttgarter Zeitung*, 22 July 1987), with others like North Rhine-Westphalia soon following suit (interviews 52, 54). Resources from these pools were mostly used to reward high-quality research and to make new high-quality research possible. For example, when a group of professors acquired a special research area (*Sonderforschungsbereich*) from the DFG, it meant they had proven the quality of their research activities. Hence, they could get additional general funds from the special discretionary pool of the state's ministry of education. An additional criterion for the assignment of these resources was the promotion of research considered to be economically or politically useful. Informatics profited especially from this redistribution (interviews 43, 45, 52) as did fields such as

13 A little earlier, in 1979, the Science Council had advised the states to instruct the universities to build up central pools of general funds *within* each university (Wissenschaftsrat 1979: 20-29). The states' ministries of education seldom followed this advice, assuming – correctly – that the universities would be unable to significantly redistribute these resources (Wissenschaftsrat 1988: 46).

biotechnology or material sciences, while the humanities and many fields of the social sciences suffered.

Obviously, this very slow, piecemeal approach to gathering resources for redistribution could seldom keep pace with the amount of resources required for accomplishing far-reaching political goals at any given moment.[14] At the beginning of the 1990s, the total amount of separately budgeted funds acquired by the universities in North Rhine-Westphalia, for example, was about twenty times higher than these discretionary funds from the ministry; using the special pools for established posts, the ministry could redistribute no more than 40 to 60 posts annually, and only one third of these could actually be redistributed according to priorities of research policy (interview 52). Thus, these resources from the special pools could at best reinforce certain developments whose impetus came from somewhere else, or sometimes catalyze such developments. But this was the only escape route government could take from the blockade described above. At least government was able to redistribute about 15% of the universities' established posts for personnel between 1975 and 1990 – an average of 1% annually. While this may seem to be a very small amount, it is three times the amount the universities themselves were able to redistribute.[15]

It seems safe to predict that other states will try to take this route on an extended scope during the coming years. As of the mid-1990s, the political room to maneuver in this respect will expand considerably for several years because many professors will retire. Some observers, like the HRK, even expect that government will use this opportunity to eliminate permanent per-

14 A lucky coincidence could be exploited politically in Baden-Württemberg (*Stuttgarter Zeitung*, 23 November 1991). According to general orders from the state's ministry of finance, all ministries were called upon to reduce their budgets for several years as of the end of the 1980s, in 1990 and 1991 by 5% each year. For the universities this meant a proportional reduction of the general university funds from the ministry of education. It so happened that this ministry initiated special programs for the universities during this same period. The funds it mobilized to do this were part of the very money it had been forced to cut. Now, the education ministry was able to distribute these resources according to its own priorities. Thus, the ministry of finance unintentionally did for the ministry of education what the latter would hardly have been able to accomplish on its own.

15 These data are from an internal survey of the HRK (see also *Deutsche Universitätszeitung* 1991(4): 7).

sonal dedications of general university funds and, instead, establish dedications which last for a specified number of years and can be renewed only after a successful passing of a personal evaluation of one's research performance (interview 55). If this really happens, government will have considerably more discretionary power in distributing general university funds, and its political guidance capacity with respect to university research will increase significantly. Thus, a slow, incremental shift of the still predominant equilibrium into a direction more favorable to government's research-policy goals has already begun and will probably pick up speed in the near future.

This change will surely be accompanied by growing conflicts with faculties and universities. But not all professors were and will be against this government policy. Professors with a high intra- or extrascientific reputation for their research performance will be at an advantage. Over time, government might establish a tacit coalition of interests with them. The redistributive measures of government will probably exacerbate the resource trouble faced by the rest of the professors while providing good opportunities for the professors with outstanding performance records.

5 Effects on University Research

Turning back again from speculations about the future to the existing resource trouble and the pattern of coping reactions, I will now briefly sketch some of the effects these developments had on university research.

To begin with the most general finding, 32% of all professors claimed in 1990/91 that their chances of acquiring separately budgeted funds had decreased during the preceding years; 35% said their chances of acquiring such funds had remained the same, while 16% said their chances had even improved.[16] This is, clearly, a mixed picture. About one third of all professors suffered from resource troubles without being able to cope with them successfully. The other two thirds either coped successfully with this kind of trouble or had no need to cope because they were undisturbed by it. This indicates that the damage done to university research by the shrinking of general uni-

16 For the remaining 17%, separately budgeted funds were not an important determinant of their research activities.

versity funds remained limited – partly because individual coping activities were sometimes reinforced by research policy.

Lacking appropriate data, one cannot say anything definitive about whether the overall volume of university research has declined. But it may very well be that in this respect nothing significant has happened yet. And even if the universities' quantitative research capacity has been reduced to some extent, this may in effect turn out to be a blessing in disguise. As long as mediocre or even bad research is being weeded out, the resource trouble can be said to have spawned a prudent consolidation of the research system.

Effects of the resource trouble which can more clearly be categorized as harmful relate to certain qualitative dimensions of university research. At least four possibly harmful effects on research quality can be distinguished. The first concerns the implications arising from the fact that the increasing efforts necessary to acquire separately budgeted funds kept professors from doing research work themselves and left them with less time to supervise their assistants' research work. As mentioned above, professors were forced to spend more and more time writing grant applications and had to pass the research work on to their assistants,[17] who were often comparatively inexperienced. As a consequence, the assistants' work may not have been as efficient as the professors' would have been, and may sometimes have lacked the ingenuity which could have been contributed by extremely innovative professors.

A second harmful effect resulted from the shorter terms of the research projects and the diminishing chances of getting follow-up projects, which meant that many professors had significant problems maintaining their research staff (interviews 20, 22, 23). Often, research assistants had to be dismissed just when they had finally gathered some research experience. Thus, professors repeatedly had to make fresh starts with new, inexperienced staff. The increased turnover of research assistants was not only detrimental to the continuity of research work, but also demotivated the assistants themselves and lowered the quality of their performance. Moreover, as the time allotted to the research projects was reduced, many investigations were left incomplete because the scientists had to start completely new projects when they applied for new grants (interviews 19, 20, 23).

Thirdly, the professors who coped with their resource trouble by doing more contract research often suffered harsh consequences in the form of a

17 See also Kaddatz (1987), who interprets this as a general tendency.

significant loss of their research autonomy. Doing contract research was necessary for many professors not only to be able to support their research staff, but also to be able to buy and repair very expensive research equipment (interviews 1, 3, 20, 23, 40). Contract research was often noninnovative, routine work, and the professors were sometimes exploited by the firms as cheap R&D consultants (interview 19). Professors had to suffer with changes of priorities of research topics coming from the firms or government agencies that could be sudden and erratic (interviews 19, 23); they also had to bear with restrictions on their rights to publish their research results and with very short deadlines for projects, which meant that they were repeatedly forced to go on to the next subject before a thorough analysis of the research results could be completed (interview 19). By informal agreement, the routine work entailed in a research contract was occasionally done by an advanced student who could use it as his graduate thesis, while the theoretically interesting aspects were sometimes dealt with by one of the doctoral candidates (interview 23). Nevertheless, even under comparatively favorable circumstances, the necessity to accept one research contract after another, for fear of otherwise being driven out of this market and missing out on potentially important future options, remained, pushing self-determined research alternatives aside. Professors saw clearly that, in time, this might result in their losing the ability to keep pace with scientific progress in their academic field (interviews 3, 19, 22, 23). Some of them therefore classified contract research as being their last resort, as the high-risk step of "prostituting oneself" – as one professor drastically put it (interview 39) – if there was no other way to survive as a researcher (interview 12, 16). Government policies aimed at the promotion of transfer-oriented research sometimes reinforced such deleterious tendencies.

Fourthly, it might be suspected that the resource trouble drove out unconventional, risky research approaches in favor of middle-of-the-road research.[18] This seems probable because the research orthodoxy is usually well represented in the peer-review committees which determine who gets separately budgeted funds for which kind of research. The research establishment serves itself first. As long as comparatively plentiful resources are available, there still remains a significant amount for outsiders and newcomers. But if resources become more scarce, even adherents to the established ortho-

18 A tendency Dietmar Braun also identifies in his case study of biomedical research in Great Britain and the United States (in this volume).

doxy have more difficulties acquiring the resources they need: They become more determined than ever to protect their claims against invaders. Being able to legitimize this by the supposed cognitive superiority of their established theoretical positions reinforces this tendency even more. In the long run, however, this eliminates important opportunities for radical innovations in science and, in fact, increases the danger that the orthodoxy will run into a blind alley sooner or later. Because "... progress can occur along unlikely and deviant paths ...," it is essential that "... space ... be left for the individual variants of knowledge to grow and mature ..." (Nowotny 1989: 342).

The problem with all of these harmful tendencies is not so much that they have already grown to dangerous proportions. We do not know for sure if this is the case. The real problem, rather, is that there are no alarm signals to warn us about these tendencies, and no emergency brakes to bring them to a halt. These tendencies can continue for a long time without the damage becoming visible. What does it mean for research and for society at large if the most outstanding researchers are worn down by resource acquisition and research is conducted by largely unsupervised, relatively inexperienced staff? How harmful is it if many research projects have to be stopped half way? What happens if university research becomes streamlined according to narrow and short-term extrascientific priorities? Where are we headed if research orthodoxy is allowed to reign virtually undisputed? The answers will only become evident in the long run – and this may very well be too late.

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Political Disturbances of Biomedical Research in Great Britain and the United States: How Political Choice Is Translated into Scientific Behavior

Dietmar Braun

1 Introduction

After the Second World War, biomedical research was regarded as one of the most promising research fields in the United States and Great Britain. In all Western countries, but most notably in the US, governmental support to biomedical research institutions and scientists was increased considerably. The situation changed when, after two oil crises, the neoliberal governments of Ronald Reagan and Margaret Thatcher took office and launched a general attack on the "oversized" public sector. As the economic crisis of the 1980s grew more severe, biomedical research institutions and scientists were confronted with a "period of scarcity" which challenged their belief that the government might spare biomedical research from austerity measures.

Starting with the governmental strategies Reagan and Thatcher introduced during the 1980s, this chapter describes how organizations and scientists dedicated to biomedical research have been affected by these strategies and how they have reacted to the disturbance of their research conditions. I intend, however, to go beyond a mere description of actions and reactions in a specified empirical case. I would like to combine the empirical description with analytical insights into the implementation process of governmental strategies in research policies.

My points of departure are the following:

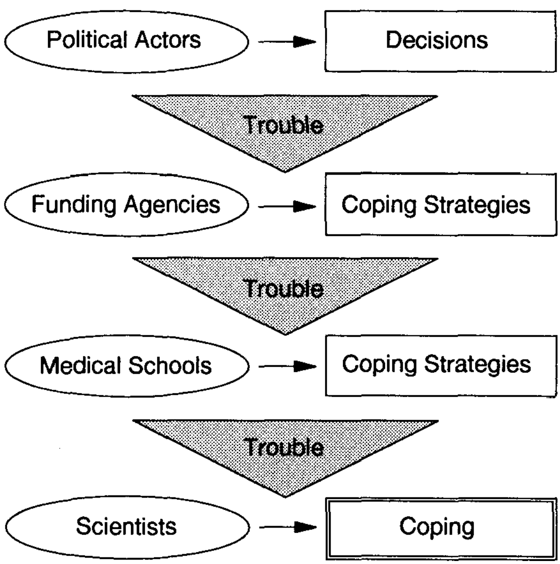
- There is no straight and linear relationship between actions taken by the government and reactions of scientists responding to the trouble induced by governmental action.

- Instead, policy makers must be aware that the strategies they favor will be "translated" within several intermediary institutions which transfer policy decisions according to their own perceptions, interests and possibilities to the micro level of the scientist.
- For the policy maker and for the scientist, these institutions – in our case funding agencies and medical schools – act as *filters* over which they have no control and which may amplify or dampen the effects of governmental strategies.
- The filtering effect of funding agencies and medical schools is due to the organizational self-interests of these institutions and to the role they fulfill in the production of biomedical knowledge by way of research.
- A two-country comparison is used to show, in addition, that the *kind of institutionalization* of biomedical research (i.e. the organization of the production of research and of its funding) explains variances in the trouble for biomedical scientists caused by political disturbances and, hence, differences in reactions of scientists.

The description follows a "top-down model" of decision making in research policies: It is assumed that the government develops strategies which may cause trouble for biomedical scientists. As the actual measures taken concern the distribution of money to research, funding agencies are the first institutions which have to react to political disturbances. They translate the political message in terms of funding instruments and goals. The decisions taken within funding agencies have direct implications on both scientists and their organizational environment, the medical schools. In many respects, however, the reactions of medical schools predetermine the scientists' room for maneuver. It is, therefore, reasonable to assume that medical schools are the next filter in the "top-down model." Political strategies and coping behavior of funding agencies and medical schools have a significant effect on the choices left to the scientist (see Figure 1).

Just because the "top-down model" is used to describe trouble in biomedical research does not imply that there are no "bottom-up" moves by intermediary institutions or scientists to prevent trouble on the level of government, in funding agencies or in medical schools. Decisions taken on each level are the outcome of manifold interactions. Nevertheless, it seems useful to proceed with the "top-down model": We are able to differentiate between the "logic of action" of each corporate actor and point to the disturbing "effects" of egoistic organizational behavior on political action. It is not *the biomedical research system* which reacts as a unitary actor to political disturbances, but many individual and corporate actors with some common interests and with interests of their own. I am less interested in how decisions come about at each level than in the fact that, once decisions have been taken at one level, they function as constraints on the choices left to actors on the next level of action.

Figure 1: Steps of Coping with "Trouble" in Biomedical Research



2 What Is the Trouble? The Tale of High Expectations, Increasing Demands and Shrinking Resources in the Biomedical Research System

The trouble in the biomedical[1] research systems[2] of Great Britain and the United States is primarily caused by a gap between financial needs and the supply of public resources for biomedical research. Three factors have contributed to an *expansionary dynamic of demands* of biomedical scientists for more public resources: rising expectations, a rising supply of research workers and cost increases.

First, since the discovery of gene-sequencing in 1974, biomedical research has entered upon a period of immensely increased opportunities for new understanding. Molecular biological techniques have opened up numerous ways for scientists to gain new knowledge which promises to enhance their scientific reputations and to yield valuable applications in industry and in health care. Given the high international competitiveness in this field of discovery, the biomedical research community urged national policy makers to provide more funds and improvements in the infrastructure for a healthy and competitive biomedical research enterprise.

1 The biomedical sciences are a broad field comprising basic and clinical research which is directed, first, to the improvement of our knowledge concerning disease-related processes and, second, to the application of this knowledge for the benefit of the patient. Hence, investigators may come from the biological, pre-clinical and clinical disciplines. By using the term ***biomedical scientist***, I refer to those scientists who have a close affinity to medical schools, i.e. pre-clinical scientists (e.g. pathologists, biochemists, immunologists) and clinical scientists (e.g. surgeons, pediatricians).

2 The constituency of the biomedical research system consists of all individual and corporate actors who contribute in one way or other (i.e. by financing, administrating and executing) to the production and application of biomedical knowledge (i.e. investigators, funding agencies, medical schools, hospitals or industrial enterprises etc.). Actors are part of the system as long as they contribute to the general function of the biomedical research system. It is quite likely that they will have additional functions and interests. In fact, most individual actors (the clinician at the medical school) and corporate actors (the medical school, the hospital) do have concurrent or even competing interests (patient care, teaching). The "coexistence" of functions is one of the factors explaining the particular dynamics in the biomedical research system (Braun 1992).

Second, new opportunities and promising roads to success in the biomedical sciences have, of course, led to an expansion in the number of scientists competing for resources in research. Lederman reports for the US that in 1990 twice as many researchers were competing for a sum of money which had not grown much since 1968 (Lederman 1991: 8). During the 1980s, the number of biological scientists multiplied from 46,000 to 67,250 while the number of medical scientists rose by 50% (Holden 1991: 1113). During the same period, the amount of money applied for by biomedical scientists from funding agencies rose by about 150% (NIH 1990: 52). Although there are no comparable and valid figures on Great Britain, it can be said that the number of full-time professors, readers and senior lecturers at universities increased from 1972 to 1980 by about 30%. In the 1980s, this expanding trend reversed due to the policies of the government. The number of students in medicine and the biological sciences stagnated, for example.

Third, in all countries the growth rates of the costs for biomedical research have increased at a much quicker pace than the gross domestic product or inflation rates.[3] This is partly due to the introduction of new technologies needed in molecular biological research (see Lederman 1991: 10) and partly due to the increase in cost-intensive regulations of biomedical research (see Smith 1990: 119; Holden 1991).

It was bad luck that the resource demands of biomedical scientists resulting from these developments coincided with a general disenchantment with science on the part of the public, an economic crisis in the Western world, and a general shift to austerity policies. Not only did both the Reagan administration and the Thatcher government place emphasis on austerity measures, they each launched major attacks on the "overburdened" public sector, which they considered much too large. Though, as we will see, there have been significant differences in research policy strategies between the two countries, one may discern three strategies the governments in the US and Great Britain had at their disposal:

- The cutbacks in financial resources induced the governments to consider a *reduction of financial support* to research in general.

3 The average annual percent increase in biomedical R&D prices in the US, for example, was more than 8% from 1980 to 1989 (NIH 1990), while inflation increased on average by about 5.5% and the gross domestic product by about 2.8% annually.

- Thought was given to a *shifting of distributional patterns* in favor of directed governmental funds at the cost of a supply of general university funds. In addition, resources were to be concentrated on the most promising centers of research. It was hoped that such measures would serve not only to reduce expenditures but also to raise the productivity in research.
- It was considered necessary to *augment the exploitable output of scientific research*. The "value-for-money" principle began to gain ground in discussions on research policies.

For biomedical scientists this promised trouble: a reduction in financial resources would wipe out most scientific aspirations in biomedical research. The prospects for a scientific career would become dim. The concentration of resources on a few centers of excellence or other government-favored research institutions would increase competition among scientists, thereby intensifying emotional stress and reducing professional solidarity. The "value-for-money" principle is traditionally suspected to be a means of government to intrude into the self-determination of scientific choice and action.

Which one of these governmental strategies were implemented in the two countries? In *Great Britain*, Thatcher did not attack the scientific community in particular, nor did she follow clearly formulated science policies when she started her crusade against the "oversized" public sector. Scientific research was just one of the many government-supported areas which suffered from these policies. In contrast to the US, biomedical research is traditionally not regarded as a high priority area by the government.[4] Thatcher radically favored the implementation of all three strategies mentioned above: The overall public investment into science declined. The share of government expenditure in gross domestic expenditures on R&D (GERD) decreased in constant terms

4 In 1984, budget appropriations for health had a share in total government outlays for R&D of only 3.7% in the UK (OECD 1991: 332) but a share of 11.3% in the US (ibid.: 347). According to another publication of the OECD (1992: 30), Great Britain supported life sciences in academic and related research with 30.9% of total research expenditures in 1987, while the US invested 48.9% of its expenditures into this area of research. Health research is second to expenditures on defense research in the US, but only number eight on the priority scale of the British government. The US is the world leader in terms of the share of health-related research in government direct appropriations to civilian R&D. In 1987, this share was 41%. The British government only spent 16.9% and the German government only 11.8% (OECD 1989: 114, Table 36).

from 49% in 1981 to 36% in 1988 (OECD 1991: 323, computations by author; in the US the share remained 49% throughout the 1980s, OECD 1991: 337).[5] There are no comparable figures concerning the government support for biomedical research. In current terms, government outlays for health research seem to have been expanding moderately, though at a somewhat quicker pace than total government outlays for research (OECD 1991: 332, Table 18; see also Irvine/ Martin/ Isard 1990). In constant terms, this meant a stagnation of financial support.

Gradually, general budget cuts were accompanied by government demands for a more transparent and efficient resource management in the universities. It was expected, moreover, that investigators contribute to social and, above all, economic (industrial) needs. Both scientists and research institutions should reduce their efforts in pure curiosity-oriented research and concentrate on "missions" formulated by industry and other users. The distributional measures which have been taken to implement these strategies have led to an emphasis on directed funding expenditures of the government at the cost of general university funding.[6] The austerity policy would be relaxed, the government announced, if the academic community demonstrated its willingness and ability to comply with these demands. It partly fulfilled its promise by a cautious relaxation of budget cuts in 1989.

The research system in general thus suffered from a considerable levelling of governmental support and political attempts to reorganize the very foundations of the research system at the cost of pure curiosity-oriented research. Biomedical research was among the victims of these governmental policies. Since the funding to biomedical research was traditionally low, both in comparison to other areas of research and compared to the high funding levels in the US, the stagnation of resources had serious implications for the activities of biomedical researchers. The international competitiveness of British biomedical research declined.

5 Government-financed GERD as a percentage of total government expenditure decreased in Great Britain from 4.5% in 1981 to 3.9% in 1985. It increased from 6.2% to 6.8% in the US (OECD 1989: 109, Table 26).

6 While expenditures on general university funds had a share of 66% of all governmental expenditures for research in 1981, this share was reduced to 51% in 1989. At the same time the share of direct governmental funds rose from 15% to 25% (OECD 1991: 323).

In the *United States* a similar break with the past in research policies cannot be found. Though Reagan favored research related to defense[7] (Smith 1990: 125), he substantially supported basic research both in defense- and nondefense-related R&D.[8] The President had, however, clear priorities in mind: The research fields contributing to industrial competitiveness should receive more governmental support than other fields. In particular, he attempted to cut down biomedical research funding which was the traditional favorite of the Congress. For a better part of the 1980s, Congress exercised its influence in the appropriation process to stop the President from introducing austerity measures it considered too harsh. As a result, federal funding of biomedical research kept growing in constant terms from 1981 to 1987 by about 2.8% each year, fluctuating between a decrease of 7% in 1981 and an increase of 9% in 1984 (Ginzberg/ Dutka 1989: 10). Overall annual support for universities by federal grants rose by 4% each year during the 1980s. Given the high initial levels of funding for biomedical research and the continued increases in financial support, what kind of trouble did the biomedical research system in the US face? The answer is simple: The governmental support was, given the needs of biomedical research, not sufficient. Because of the generous funding in the past, total expenditures for biomedical research rose, for example, from 300 million dollars (constant) in 1950 to 1,654 million in 1965. Biomedical research had, thus, become the largest academic activity in the US, thereby multiplying demands on governmental support. The strong dependence of biomedical research on government spending is indicated by the fact that public support accounted for 51% of funding in 1989 (Institute of Medicine 1990: 33). Research universities and medical schools are often dependent on federal funds for two-thirds of their income. Since 1968, however, a constant levelling of federal funding has taken place. In real terms, the support decreased until 1975 and since then has fluctuated around an annual increase of 2% (ibid.: 38), which does not meet the needs for capital investments and salary rises in universities (Congress of the United States 1990: 22). Due to

7 In 1985, 59% of total government-financed R&D was devoted to defense (OECD 1989: 111, Table 30). The increase in constant dollars from 1981 to 1988 was 83%, while nondefense R&D decreased 24% (Smith 1990: 133).

8 Defense-related basic research expenditures rose, in constant terms, by 11% from 1980 to 1988; nondefense-related basic research expenditures increased even more, by almost 40% (Smith 1990: 133).

the shrinking resources for capital investment supplied by federal funds, medical schools have failed to invest in buildings and equipment since the end of the 1960s. The result is an old-fashioned, worn-out infrastructure which does not meet the rising expectations and technical needs in biomedical research.

The situation was aggravated at the end of the 1980s when the Gramm-Rudman-Holling Act (Smith 1990: 156), which was intended to limit the budget deficit, failed to render the fiscal constraint the President and Congress had hoped for. Tight limits were set in 1990 for the expansion of all "discretionary programs"; governmental support for biomedical research was included in this category. Since then, governmental agencies supporting research have to compete for an increase of their funds with other discretionary programs such as housing or Medicare. The increase of public support in one program requires, by rule, a decrease in funds available to others (see Norman 1991).

Thus, though trouble for biomedical research is found in both the UK and the US, it differs in two respects. First, the starting positions of biomedical research actors were different when political disturbances began early in the 1980s. Most notably, biomedical research had been among the most valued governmental research activities in the US since the end of the Second World War, leading to generous support unequalled in any other country. With the Congress being traditionally very supportive of the biomedical research community, expectations for constant gains in the appropriation process were high. When expectations are high, disappointment begins earlier and is felt more profoundly than in the case of low expectations. This is one of the reasons – next to the problem of outmoded research facilities and the gap between cost increases and federal funds – why, despite steady increases in funding in the US, political strategies were perceived as being just as troublesome there as they were in Great Britain, where biomedical scientists had to deal with austerity based on a much lower level of governmental support. The frustration of British biomedical scientists was not the result of unfulfilled high-level expectations, but of a profound deterioration of research conditions.

Second, British biomedical scientists were subject to governmental strategies which comprised three components of trouble: reduction of resources, concentration of resources and an emphasis on the "value-for-money" principle. The last two points in particular were considered an attack on the traditional framework of scientific activity in Great Britain. In contrast to the US, British scientists were accustomed to receiving a substantial part of their resources for research from general university funds, which meant that they did not have to compete with other scientists for funds. Instead, each department at a university was supplied with money calculated according to the number of students and teaching capacities. There were no incentives for competition or for a concentration of resources. Doubting the effectiveness of these distributional patterns, Thatcher called for a more performance-oriented approach to allocating funds for research. It was not only austerity which threatened research activities in Britain; the new measures meant a major break with the past requiring far-reaching organizational and individual reorientations. A similarly pervasive political disturbance cannot be found in the US. Reagan's strategies were limited to austerity measures. The support of curiosity-oriented research remained a high priority. The trouble in the US resulted, at least until the end of the 1980s, from the very high expectations on the part of the biomedical research community, inadequate investment policies of medical schools in the past and cautious budget restraints. Therefore, though the ingredients of trouble differed significantly, frustration levels in both countries were high.

3 The First Filter: Funding Agencies

Both in the US and Great Britain funding agencies play an important intermediary role in the distribution of public money to the research system. Once the government has decided upon the amount of money to be appropriated to the broad research areas (defense, health, agriculture etc.), funding agencies in Great Britain and the US have considerable autonomy in determining by what means and to what specific research areas the appropriated funds will be distributed. The task of funding agencies is to allocate the resources for research in such a way that international competitiveness is achieved, the most promising scientists and institutions are supported, underdeveloped scientific fields are developed and the transfer of basic knowledge into areas of application is accomplished. Funding agencies translate political disturbances according to their own needs, perceptions and functions. They are the first filter through which political decisions have to pass on their way to the biomedical research scientist.

The *structure of the funding system* is an important factor in explaining the strategic opportunities and the chosen strategies of funding agencies in a country. In the *United States,* for a number of reasons we cannot discuss here (see Ben-David 1968, 1971; Wittrock/ Elzinga 1985; Braun 1990), biomedical scientists in the major medical schools of research universities[9] are highly dependent on the support of the *National Institutes of Health* (NIH), a relatively independent government agency. In 1987 medical schools reported that 75% of their research income was paid by federal agencies (AAMC 1989: 25), NIH being the most important grant supplier (NIH is responsible for 75% of federal grants to biomedical research; Institute of Medicine 1990: 210). This outstanding position is accentuated by the federal funding agencies' practice of paying not only for the direct costs of research (i.e. technical and administrative assistance, salaries of investigators), but also for the indirect costs (i.e. overhead: infrastructure supplied by the universities for the conduct of research, the maintenance of buildings, heating systems etc.). Both private and public medical schools, therefore, finance most of their research activities (salaries, buildings, facilities) by project grants. Both private and public medical schools are, therefore, bound into a funding system in which one agency – the National Institutes of Health – dominates the distribution of governmental funds to researchers and research facilities alike.

The funding system for biomedical research in *Great Britain* differs from the American system. Research is supported by a *dual funding system.*[10] The *University Funding Council*[11] (UFC) is responsible for the distribution of the general university funds (so-called "block grants") to all universities. Block grants are divided into a teaching and a research component, but the universities are free to distribute the money as they see fit among their vari-

9 I will limit myself to the discussion of those medical schools in the US which have dedicated themselves to the promotion of biomedical research. If we regard medical schools receiving more than 60 million NIH extramural dollars each year as "research medical schools," about one fourth of the 127 medical schools belong to this group (see NIH 1989: 99-103).

10 The dual funding system is under discussion at the moment. Major reforms are expected in 1993.

11 Before 1988, the *University Funding Council* was called the *University Grants Committee*; since 1992 it has been called the *Higher Education Funding Council.* I will use *University Funding Council* throughout the paper even when referring to the period before 1988.

ous activities. The funds distributed by the UFC to research are supposed to provide sufficient means for the universities to finance the "well-found laboratory": Universities are required to supply the research facilities for their scientists who have been awarded project grant money by funding agencies or for the exploration of research areas not yet taken into account by the funding agencies.

The *Medical Research Council* (MRC) is the most important funding agency dealing with biomedical research.[12] The task of the MRC is – in addition to conducting intramural research, i.e. research in its own research establishments – to support research activities in selected fields of research at universities by providing temporary grants to research groups and individual scientists. The role of the MRC is less important than that of NIH because the dual funding system allows biomedical scientists in principle to conduct research without applying to the MRC. Furthermore, there is a plurality of sources for health research available in Great Britain; philanthropic foundations and the pharmaceutical industry are actively engaged in clinical research. More than 60% of funding for basic biomedical research projects comes from the MRC.

In the unitary funding system of the US, the entire responsibility for the distribution of federal funding resources rests on the shoulders of one funding agency. The institutional support to medical schools is directly linked to the temporary grants awarded to their scientists. In a dual funding system, institutional support and grants are administered by two agencies with different functions and perceptions.

In order for the dual funding system to function well, the agencies must be able to distribute the money to the research system in tune with each other. In order for the unitary funding system to function well, one funding agency must be able to avoid displacement effects of direct and indirect cost reimbursements. How did British and American funding agencies cope with trouble?

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12 If the sources of funds for health research are compared, it is shown that the MRC provided for about 12% and the UFC about 15% of all health research expenditures in Great Britain in 1985. Omitting expenditures by industry and charities, the share increases to 31% and 39% respectively. The MRC spends about half of the total expenditures of all the agencies which award only research grants (House of Lords 1988: 415).

3.1 Coping with Crisis: British Funding Agencies

The *UFC* faced stagnation of its funds in real terms. The Thatcher government declared that unless the UFC could demonstrate that the block grants were being distributed more transparently, selectively, efficiently and in accordance with social and economic needs than before, it would not receive an increase in appropriations. The UFC's organizational survival thus depended on its being able to develop a more efficient and performance-oriented distribution procedure.

Though the *MRC* was not as seriously affected by austerity measures as the UFC, it suffered a serious setback in funding opportunities during the 1980s. The government was determined to increase its support for research councils only proportionately to the rates of growth of the GDP, which at that time were exceeded considerably by the inflation rates. Meanwhile, the demands of university researchers on the MRC became more pressing because the appropriations from the UFC were stagnating. This initiated a downward spiral in the dual funding system: though the MRC increased its support to biomedical research at universities, it could not compensate for the financial losses induced by stagnating resources from the UFC. Furthermore, the MRC had neither the authority nor the funds to cover the increase in indirect costs caused by the rise in the number of research projects at universities. Medical schools were thus confronted by an increase in the costs for the maintenance of research facilities. Insufficient research facilities led, in turn, to a reduction of approval rates by the MRC. Grants could not be awarded if the applicants could not provide the preconditions for conducting research.

On top of all this, the MRC's traditional pattern of distributing funding resources – in which intramural research dominated and pure curiosity-oriented research was a priority – was challenged both by the biomedical research community and by the government. They demanded jointly – though for different reasons – a reduction in the support to intramural facilities of research councils and, hence, an increase in the support of university research. In addition, the government, but not the university researchers, demanded a much more application-oriented and selective attitude toward support to universities. The MRC thus had no choice but to make basic changes in its funding patterns.

The British funding agencies' room for maneuver in coping with trouble was seriously hampered by its lack of influence on the appropriation decisions taken by the Department of Education and Science (DES).[13] The UFC and the MRC were, moreover, confronted by a government which was determined to implement its policy goals despite the resistance of scientific and funding organizations; more importantly, it had the executive power to do so. British funding agencies could, therefore, only attempt to *minimize the damage* imposed on biomedical research by political disturbances. It was very unlikely that they would be successful if they attempted to bargain for change in these government policies.

If the UFC was to minimize damage, it was well-advised to concentrate its resources on the research projects and scientists which offered the best prospects of success (UGC 1984; ABRC 1987: 3). The medical subcommittee of the UFC realized the harm which would be done by a general and abrupt decrease in block grants. It also realized "that this relatively small country, with its present not very high gross national product can [not] compete across the board and in every institution" (House of Lords 1988: 409, see also 391). The best strategy was, therefore – given the seriousness of the threats of the government –, to *select* the most promising research groups and to *concentrate* the available resources on these groups or universities. This policy was reinforced by several authoritative assessments of the situation, such as the Merrison Report. The MRC policies were guided by very similar considerations.

Though the reduction of resources compelled the British funding agencies to submit to selection and concentration, there was a convergence of interests between funding agencies and the government in this respect. Both the UFC and the MRC had a strong interest in overcoming some of the institutional rigidities of medical schools. They were eager to improve the mediocre research performance of biomedical scientists in Great Britain. Selective funding strategies seemed the adequate way to challenge the traditional behavior and distribution patterns at medical schools which frequently led to suboptimal research results. The funding agencies sustained, therefore, the selection exercise for reasons of their own. They did not, however, support the governmental view on the "value-for-money" principle. Instead, they aimed at the strengthening of curiosity-oriented research.

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13 Now, the authority over the research councils has been passed on to the Office of Science and Technology.

Both the UFC and the MRC worked, therefore, on elaborating their own policies of selection and concentration. The UFC began to distribute the research component of the general university funds in accordance with a number of criteria of performance (ABRC 1987: 37). One of the criteria required a thorough evaluation of the research performance of all university departments. For this purpose, they were divided into subject areas such as pharmacology or clinical medicine (so-called "cost-centers"). Though the medical schools maintained their authority to distribute the general university funds according to their perception of their own needs, the UFC urged universities to improve their management of financial resources for research and establish a performance-oriented procedure for the distribution of research money (UGC 1984).

The MRC introduced a number of coping strategies. Pushed by the government and the academic community as well as by a recommendation of the Advisory Board of Research Councils, the MRC began to shift its resources for intramural research to extramural activities at medical schools.[14] It was accepted that the new opportunities arising in biomedical research should be responded to with a more vigorous financial effort. The shift from intramural to extramural activities was supported by another consideration: Intramural institutes and research units were increasingly regarded as obstacles to the more flexible and selective funding strategy the MRC considered indispensable. A number of research units as well as the Clinical Research Centre were closed.

In accordance with its selective policy, the MRC created a strategic committee to prepare plans for the medium-term and define priority areas of research. From then on, these plans were published in a "Corporate Plan" every four years.

The funding of priority areas demanded a reorientation in funding procedures. The MRC chose to reduce its support to pure curiosity-oriented research initiated by university scientists. New funding initiatives were announced to improve the performance of research in selected research areas. A number of interdisciplinary research centers were set up to establish links between the scientific community and industry.[15]

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14 56.3% of appropriations went to the intramural divisions in 1981. Ten years later, the share had fallen to 46.9% (Braun 1992: 363).

15 Added together, the share of the measures directed to priority and to deficit areas increased from 12.9% in 1981 to 25.1% in 1991, while the grants for basic research initiat-

The shift to a more selective and longer-term support corresponded to the need to minimize the damage to the biomedical research community. It was, in addition, regarded as an adequate response to immanent changes in scientific research. Interdisciplinary research areas as well as new ones needed longer periods of assured support and larger amounts of financial resources than were usually regarded as appropriate.

Given the strong executive power and the determined stance of the British government, the funding agencies were not able to prevent the government from introducing its encompassing policy of reforms. Being unable to forestall a period of scarcity, they had to resort to selection and concentration. Funding agencies submitted to the selection imperative, but the distribution criteria and funding instruments which were introduced were not as rigorously guided by the "value-for-money" principle as the government might have wanted. The first filter allowed, therefore, a shift toward more selective and concentrated research but only a minor shift toward more application-oriented research. Governmental demands for more competition within the academic community continued.

3.2 Coping with Crisis: The National Institutes of Health in the US

In the United States, the National Institutes of Health was challenged by the pressure of biomedical scientists and medical schools for more resources while the government attempted to curb down the size of its expenditures. This situation was aggravated by several other factors. The demands of the biomedical research community caused the NIH to extend its money for investigator-initiated projects from an average of 3.3 years in 1983 to 4 years in 1989 (NIH 1990: 49). The average size of research projects was increased from 97,800 dollars (1980) to 113,900 dollars (1989; constant dollars) (NIH 1990: 56). When appropriations decreased relative to the financial needs of biomedical research, the commitments to projects already receiving grants caused the "success rates" – the number of new and competing project grants actually awarded each year – to decline to 24% of all applications in 1990

ed by university scientists (the so-called project grants) decreased during the same period from 17.5% to 10.3% (Braun 1992: 363).

(Institute of Medicine 1990: 218; see also Palca 1989). Young investigators were increasingly deterred by this development.

A second problem was caused by federal policies of infrastructural support to medical schools. After the Second World War, federal agencies not only supported universities by direct and indirect cost reimbursements, but also provided facilitation grants to medical schools. Up to 50% of the costs for constructing, remodeling, and equipping new or existing buildings for the health sciences were provided under this authority (Institute of Medicine 1990: 151). When the facilitation grants program ended in the early 1970s, medical schools had no funding sources to compensate for the losses. As a result, they used grant money not only to finance overhead for specific projects, but also to maintain and improve their buildings and equipment in general. In this way the share of indirect cost payments of the NIH rose relative to direct costs,[16] eroding the capacity to supply sufficient means for new grant proposals.[17] In 1970, indirect costs added up to 111 million dollars (21% of total costs). In 1988, they had increased to 1.4 billion dollars (31% of total costs).

The NIH finds itself, therefore, in an uncomfortable position: Its project grant money is the most important source to pay salaries for investigators and to support young investigators and innovative research. At the same time, the financial needs of medical schools with regard to research facilities force the NIH to redistribute its resources in favor of indirect cost reimbursements.

Finally, though Congress protected NIH from the most severe effects of governmental austerity measures until the end of the 1980s, it gradually began to insist that the NIH demonstrate more visibly its contribution to public concerns in the health sector. The "earmarking" of appropriations to NIH became a preferred strategy of the Congress. In the most recent budget, the only institutes at the NIH which prospered were those which demonstrated that they were actively engaged in research areas of concern to the Congress such as AIDS and women's health (*Chronicle of Higher Education*, 2 December 1992, A28). In contrast, basic research without clear applications in mind as

16 Though the average share of indirect costs was only 31.6% in 1989 (NIH 1990: 47), the research medical schools were often reimbursed for 50%-100% of direct costs.

17 The discovery of recent abuses of the indirect cost system led the Congress to prepare a bill which will cap reimbursements for administrative expenses at about 26% of direct cost payments (Barrinaga 1991; Hamilton 1991; Palca 1991a).

it is conducted by the National Institute of General Medical Sciences is finding it difficult to avoid losses in funding resources.

How did NIH cope with these problems? It reacted in two ways: it endeavored to protect investigator-initiated, curiosity-oriented research using a bundle of strategies; when this strategy seemed no longer feasible, it switched over to a more selective style of funding.

The responsiveness of the NIH to the demands of the biomedical research community for support for basic research has been demonstrated on several occasions in the past. The NIH has more room for maneuver to do this than the British funding agencies do: The American political system provides the legislative branch with more power over decisions about annual appropriations. The directors of the individual institutes of the NIH have a substantial voice at the appropriation hearings of the congressional committees. This voice was often used successfully in obtaining substantial increases in funding (Strickland 1972; Braun 1990). British funding agencies are not given a similar chance to contribute to the government's decisions on appropriations.

The NIH took advantage of its ability to directly influence Congress as early as 1981: It made a deal. It was agreed that a specified minimum number of new and competing project grants should be appropriated by the Congress and awarded by the NIH, which was supposed to prevent the erosion of the investigator-initiated grant (Institute of Medicine 1990: 2). Although the agreement provided some relief for the NIH, enabling it to keep apace with the rising number of applications during the 1980s, the situation has deteriorated considerably when the Congress abrogated the deal in 1989. Since then, the Congress has become more and more reluctant to provide the NIH with the usual surplus in funding resources. In 1992, for the first time in history, Congress appropriated a smaller budget for the NIH than even the President had asked for (*Chronicle of Higher Education,* 2 December 1992, A28).

To sustain basic research in the period of scarce resources, NIH began in the early 1980s to redistribute its resources in favor of the investigator-initiated grant. The number of training awards was, for example, kept at a constant level and intramural support was decreased. When the increasing financial strain caused reviewers of grant applications to begin to favor scientists with a high reputation conducting "safe research," new funding instruments were created. These instruments were designed to support the groups most likely to be the victims of the period of scarcity, e.g. young investigators conducting innovative and risky research (Smith 1990: 182-183).

After 1989, it became evident that the policies aiming at the protection of pure curiosity-oriented research had not been very successful. NIH had to contrive new coping strategies to obtain additional appropriations from Congress. "Big science projects," such as research on AIDS or the Human Genome, could demonstrate to the Congress NIH's willingness to address the most urgent problems in health while continuing to explore the frontiers of biomedical science. They could, moreover, bind resources for a long period of time and allow scientists of the NIH to use the appropriated money as windfall profits: AIDS-related research projects, for example, have multiplied during recent years, indicating the effort of intramural investigators to attach a label to their research which would meet the approval of Congressional representatives. In order to forestall this tendency, Congress has proposed legislation to centralize and coordinate the distribution of earmarked funds for research on AIDS, in the belief that this is the only way to ensure that the earmarked money will actually get to the investigators exclusively devoted to studying the disease and finding a cure (*Chronicle of Higher Education,* 24 February 1993, A24).

The problem for NIH has been that big science projects, although receiving more money, have absorbed resources usually distributed to the investigator-initiated research projects (Holden 1991).

Another policy announced by NIH's director was the plan to increase the competitive strength of the NIH in the annual bidding for appropriations by developing a common strategy for selecting future research fields and objects valid for all Institutes of Health. It was hoped that the integration of the fragmented activities of the different institutions would give NIH a more distinct corporate identity useful for the lobbying activities in Congress (Palca 1991b). Again, this indicates the adoption of a revised policy which shifts – though still on a minor scale – from support for individual investigator-initiated research to support for strategic projects which are more likely to address the interests of the Congress and the public.

NIH was thus able to cushion the negative effects which governmental austerity considerations would have had on biomedical research by bargaining with the Congress for increases in its resources. At the end of the 1980s, the NIH had to acquiesce to the more short-term and application-oriented stance of the Congress and devote a larger proportion of its funding resources to strategic areas of research about certain diseases.

In sum, the relatively successful attempts of the NIH during the 1980s to protect biomedical research from the harsher consequences of reduced financial resources, and its aim of protecting pure curiosity-oriented research stands in contrast with what happened in a similar situation in Great Britain. There, the UFC and the MRC were unable to protect biomedical scientists and medical schools from the austerity management of the government. Research stimulated by scientific curiosity was a prominent victim of the research councils' efforts to adapt to the decrease in available financial resources. The MRC decided to shift its resources from investigator-initiated grants to grants designed within the council and targeted to priority areas. The UFC was unable to deliver sufficient resources for the "well-found laboratory." The British biomedical scientist therefore began to lose the opportunity to develop his own research designs and independently conduct curiosity-oriented research.

Several factors explain this difference between the modes of adaptation devised by the NIH in the US and those used by the MRC and the UFC in Great Britain: One, the NIH had the advantage of being able to lobby directly with the legislative branch for additional resources. This reduced the pressure to concentrate funding resources on strategic areas for quite some time. Two, in the US most of the salaries of biomedical scientists and most of the expenses of institutional maintenance at medical schools have been dependent on the continuing support of the NIH. A selection of research fields as limiting as in Great Britain would have threatened a number of disciplines and subdisciplines with annihilation. Income from other sources, most notably from patient care, industry and foundations, could not compensate for the declining resources of the NIH. The "unitary funding system" entrusts the NIH with a responsibility extending far beyond support just for the most promising research areas. NIH provides, as it is said, the "oil that keeps the whole biomedical research machine running." In Great Britain, the dual funding system guaranteed, at least for the faculty at medical schools, a regular income and a (albeit deteriorating) research base for those scientists who could not obtain grants from research councils. The MRC could, therefore, disavow responsibility for scientists working in less promising fields of research.

Three, there is a basic difference between the flexible and adaptive structure of the American biomedical research system and the more rigid structure of the organization of research in Great Britain (Ben-David 1971; Braun 1992). NIH was not compelled to design funding instruments which would more selectively remove institutional rigidities in the research system, while the MRC and the UFC were convinced that the selection and concentration of funding resources would contribute to a more productive and internationally competitive research enterprise. For this reason, the British funding agencies passed on the policy of the government to concentrate resources in research without cushioning the biomedical research community from its effects.

4 The Second Filter: Medical Schools

Medical schools are the switchboards distributing a large part of the financial resources from funding agencies to biomedical scientists. The administrators of medical schools must decide how the trouble caused by policy makers and funding agencies will affect the time resources and career perspectives of the faculty and how financial means and research facilities are to be distributed among the departments and among teaching, patient care and research. The services the clinical faculty of medical schools deliver in hospitals provide additional financial resources not available to other departments and contribute to the distinguished position of medical schools in universities. They are, as Geiger noted (1985: 81), often like "autonomous fiefdoms"[18] and may be regarded as relatively autonomous corporate actors at universities. It makes sense, therefore, to discuss the coping strategies of these institutions with regard to the trouble which arrived in the 1980s.

4.1 The Trouble for Medical Schools

Medical schools in *Great Britain* suffered loss in income due to insufficient funding of the UFC and the selection procedure of both the UFC and the MRC. Given the far-reaching intentions of the government and the converging interests of the funding agencies, the British medical schools had to face attempts to transform them from primarily "state-subsidized" institutions into more "market-competitive" ones. Organizational routines and procedural rules

18 In fact, 27 of the 127 medical schools in the States are independent of universities.

of medical schools were scrutinized. Uncertainty about the future characterized the policies of medical school administrators.

American medical schools were not challenged by a similarly profound transformation. They were simply faced with shrinking competitive markets. The effects on the rules and routines of medical schools were, nevertheless, as far-reaching as in the British case, for a number of stabilizing organizational mechanisms could not be maintained. In fact, one can say that the whole system of financing biomedical research began to falter. For example, biomedical scientists teaching at a medical school are supposed to acquire project grants in order to finance both a part or all of their salary and most of the indirect costs of research. Medical schools often pay salaries to their staff only for their teaching duties. This reduces the fixed costs of medical schools considerably and contributes to a higher flexibility and more intensive organizational support to research. In addition, the income the scientists obtain from research grants is used as a comparative base for the evaluation of the research performance of each scientist. This has been an efficient mechanism for distributing financial resources within the medical schools. If, however, only 20 to 30% of the applications for grants are successful, this system can no longer be upheld.

As it cannot be assumed that all biomedical scientists who are now not awarded grant money are insufficiently competitive on the academic market, medical schools are forced to compensate, at least to a certain extent, for the scientists' losses in income (Froomkin 1983: 43). This led to a vicious circle: If the medical schools pay more for their faculties' salaries, their resources for the institutional maintenance of research decrease and demands for indirect cost reimbursements at the NIH rise. Rising indirect costs threaten the amount of money NIH has earmarked for investigator-initiated grants. In turn, success rates decline, and the scientists' demands for more support from medical schools grow. In this way, the American biomedical research system finds itself captured in a maelstrom which pulls down all participants. Coping strategies of medical schools become mere strategies of organizational survival.

Setbacks in the *health care system* had additional negative consequences on biomedical research conducted within the confines of medical schools in both countries. Though the *British* National Health Service suffered less than the medical schools from Thatcher's austerity policies, the cutbacks in resources and manpower still affected the medical schools. This became evident, for example, when financial aid and infrastructural support to clinical research in NHS hospitals were withdrawn. The teaching duties of NHS consultants in medical schools were reduced considerably, thereby increasing the faculties' work loads. Health care systems in general raise, moreover, some of the costs of medical schools. This is due to the greater opportunities available to the clinical faculty of medical schools to leave for an appointment in the health care system which is often better paid and promises better career perspectives. In order to prevent clinical faculty from leaving, the medical schools have to offer a salary comparable to that of a medical doctor in the health care system. In the period of scarcity, the ability of British medical schools to equalize doctors' salaries was substantially reduced as the government failed to raise the salaries of the clinical staff at the same pace as the salaries of NHS consultants. The growing difference in salaries contributed to the exit of many promising young scientists.

Affiliated hospitals in the *United States*, confronted with the increasing financial contraction in the health care system, started to emphasize a more professional and competitive patient-care management and to concentrate their resources on patient care (Braun 1990). Medicare – the federal sickness insurance program for elderly people –, for example, reduced its indirect teaching payments to hospitals; Health Maintenance Organizations and other sickness insurance companies refused to pay for the costs involved with patient-research.

The linkage to the health care system increased the trouble of medical schools in both countries. How did medical schools deal with this situation?

4.2 Coping Strategies of Medical Schools

The general university funds provided by the UFC are the major income of *British* medical schools. As these funds stagnated in constant terms (OECD 1991: 323) and compensation from other sources – in particular for basic research – did not seem to be sufficiently available, the administrators at medical schools have been forced to cope by concentrating their efforts on the most important tasks (1) and reorganizing the administration of research (2).

1. The most striking difference between the coping strategies of American and British medical schools is that the American schools have tried to protect research from the negative effects of austerity strategies, while the British schools have redistributed their resources to the disadvantage

of research. This difference may be traced back to the American medical schools' tradition of regarding themselves as "research schools"[19] and the British medical schools' tradition of maintaining a balance between teaching, patient care and research, with a somewhat greater emphasis on teaching (Ben-David 1977). In the US, being a clinician has primarily meant pursuing a career as a scientist; in Great Britain clinicians have been physicians, investigators and teachers at one time.

It is therefore no surprise that when British medical schools exercised their authority to distribute the general university funds, their first priority was preserving their teaching mission and their second was meeting their obligations to patient care in the hospitals of the NHS. The financial support for the "well-found laboratory" was only third on their list.[20] Teaching and patient care have been regarded as the most urgent tasks of the medical schools which cannot be postponed. Research can wait. In addition, teaching and patient care have a powerful clientele (students and their parents; patients) pushing the medical schools to meet their commitments. Biomedical research is not backed in any similar way by specific social groups or elites.

The clinical staff of British medical schools was expected to compensate for the loss of NHS consultants by spending more of their time in teaching (Smith 1988). Almost all of the medical schools have temporarily stopped making new appointments with permanent tenure in order to keep down their expenditures on salaries.[21] Both factors have added to the work load of the clinical professors and decreased the time available for research.

Without doubt, the redistribution of resources in favor of teaching and patient care has added to the deterioration of the medical school support

19 See for the significance of research in American graduate schools in general Geiger (1986) and Ben-David (1972: 87), and for medical schools in particular Ben-David (1972: 91-94).

20 There are, of course, differences among the British medical schools in this regard. Leading universities such as Oxford, Cambridge or London University may have tried very hard to maintain their level of research. Nevertheless, on average the statements above seem to hold.

21 This led to a reduction of full-time university-paid clinical professorships by about 500 from 1980 to 1986. At the same time, full-time research personnel increased from 324 to 656 (Walsh 1991: 32).

to academic research and has, above all, jeopardized the position of young, innovative or "unorthodox" investigators who could not yet obtain project grants from the MRC.

2. Many British medical schools have not been able to resist the demands of the government and the funding agencies for a more accountable, transparent and selective distribution of resources to their departments. It has been, in fact, in their own interest to be more selective and concentrated in their uses of the financial resources available to them. Given the general decrease in resources, it was impossible to continue the support to all research areas and all scientists at each medical school. The concentration of resources to a few centers of excellence diminished the total sum of money needed for research. It also had the advantage that the research groups or departments receiving support had a better chance of obtaining grants from the MRC or charities. The acquisition of more resources from funding agencies led in turn to a higher amount of funding distributed by the UFC. The number of grants awarded by research councils is one of the distributive criteria in the research component of general university funds.

   A number of British medical schools have started, therefore, to establish evaluation and research committees to draw up corporate plans entailing the medium-term research priorities; they have also improved the management of financial resources in research (Walsh 1991; Irvine/ Martin/ Isard 1990: 43).

The coping strategies of British medical schools were, one can conclude, not designed to protect the "economic capital" of biomedical scientists. The time available for research was reduced, the career perspectives of young and promising clinicians began to look dim, and medical schools were not able to protect the "well-found laboratory." The most important location of risky curiosity-oriented research in the British system, the medical school, began to lose its vitality. Moreover, the establishment of corporate plans and the announcement of research priorities introduced a more selective style of distributing the institutional resources of medical schools to research. Meanwhile, the competition for resources in research inside and outside the medical schools has intensified. This has been done by establishing evaluation committees and by encouraging scientists to apply for project grants outside the medical schools and not to expect institutional resources from the medical school.

The room to redistribute financial resources among teaching, patient care and research has been much smaller for *American medical schools* than for British ones because their corporate identity has been associated with the promotion of qualified biomedical research. The organizational fate of the medical schools themselves often depends on the maintenance of a reputation as a center of excellence in research because federal grants for research are their most important source of income. The organizational survival of medical schools is, therefore, intimately entangled with the productivity of their biomedical scientists. This is why the decrease in federal resources put enormous pressure on medical schools to search for adequate coping strategies to protect research.

The exploitation of existing and new sources seemed the most likely candidate as a coping strategy. Medical schools attached to private universities could try, for example, to acquire additional endowments and private gifts; those attached to state universities could apply for additional appropriations from state governments. There was a certain reluctance, however, to apply for grants in both cases, since the grant-giving bodies often attached conditions to the money which were detrimental to the goals of basic research (Froomkin 1983: 41-42).

The easiest and most obvious strategy open to American medical schools, then, was to expand their income from hospital services. There had been a general shift in the composition of revenue sources of medical schools since the 1970s when federal agencies began to gradually withdraw their facility grants. Incomes from hospital services increased considerably while federal grant income decreased (AAMC 1989). Medical schools attempted, however, to avoid a displacement of research by patient care. The additional time which clinical professors had to devote to patients was equally distributed among members of the clinical staff in order to reserve as much time as possible for research. A considerable amount of the money medical doctors earned for their patient-care services was put into a general fund the dean of the medical school could tap to support the nonclinical and research-oriented departments.

Even though these strategies of exploiting existing and new sources were partially successful, they could only mitigate the financial curtailments American medical schools suffered from. Though the NIH has softened the impact of governmental austerity policies, the medical schools have not been able to profit from this support, but have reinforced the financial strain caused by mistakes in the past. The long-standing failure to renew buildings and facilities for research was one of the major strategic mistakes medical schools had made. The cutbacks in federal support in the 1980s would have been easier to tolerate and cope with if they had not been accompanied by the urgent need to invest in the modernization of research facilities.

At an increasing pace, therefore, American medical schools lost the capability to prevent fierce competition among their scientists and had to acquiesce to a concentration of their resources on too few scientists and research groups (Froomkin 1983: 38).

In both Great Britain and the US, the systems are unbalanced compared to the 1970s, though they started from different positions: The British system is being transformed from a "state-subsidized" into a "market-competitive" academic system *and,* at the same time, must learn how to live with less. In the market-competitive system of the US, declining resources have led to a downward spiral in biomedical research activities neither funding agencies nor medical schools have been able to terminate.

5 Coping with Crisis: The Biomedical Scientists

The coping strategies American and British biomedical scientists could choose varied because of different governmental strategies and different coping behavior of intermediary institutions in the two countries. Let us see how coping strategies of scientists are affected by decisions taken on the other levels in the biomedical research system.

The main interest of biomedical scientists is to acquire a scientific reputation by doing research. Without "economic capital," provided by funding agencies and medical schools, this interest cannot be materialized. The "economic capital" consists of time resources, money and infrastructure for research and career opportunities. The biomedical research systems of the two countries did not provide sufficient "economic capital" during the 1980s. The time available for research decreased in both countries, but was better protected in the US than in Great Britain. Career perspectives have also worsened in both countries, but only in Great Britain have this contributed to a real decrease of the clinical staff at medical schools. Difficulties providing institutional means for research have been evident in both countries; they have been more visible, though, in the US because of the prolonged failure to invest into buildings and other facilities. Finally, the money for research was seriously reduced in Great Britain and the US, though to different degrees. The NIH in the US could prevent a strong decline of its funds during the 1980s. In Great Britain the stagnation of UFC funds, the redistribution of general university funds by medical schools in favor of teaching and the more strategic funding of the MRC reduced the funds for investigator-initiated curiosity-oriented research considerably.

It seems, therefore, that the "economic capital" of biomedical scientists needed for the continuation of their scientific career was more seriously reduced in Great Britain than in the US.

Scientists can respond to these situations with one of three strategies. The first way open to the clinical scientist is to *leave the scientific profession* and embark on a new career as a medical doctor in the health care system. This alternative is open especially to the young graduates of medical schools who have not yet decided whether to become a researcher or a physician. Given the economic constraints in the opportunities for careers in research, this alternative has become more attractive in recent years. In fact, in both countries the number of physicians in research has declined.

It is easier for the British clinician to choose this alternative than for his American colleague because of the traditional lack of differentiation in Britain between the tasks of teaching, patient care, and research. In the US, the biomedical scientist is often specialized in the profession of an investigator, though he does, of course, teach and deliver services to patients. Teaching and patient care are regarded as unavoidable means to conduct research. In Great Britain research has not become a profession in its own right to the same extent as it has in the US. It is an auxiliary function attached to teaching and patient care. This makes it easier for the British clinical scientist to decide in favor of a career as a physician than for his American colleague.

Scientists and young physicians may also accept appointments in industrial firms. This has become a favorite strategy for young graduates in the US because of the biotechnological industry's great interest in hiring biomedical scientists.[22]

22 Holden (1991) reports that in 1969 25% of graduates went to industry, and that the share had increased to 40% in 1987.

A second strategy open to the scientist is to remain in a medical school and *be content with conducting less research* than before. Again, this option seems to be more feasible for the British biomedical scientists than for their American counterparts. In Great Britain, medical schools have been pressing their clinical faculty to accept a reduction in their time for research while the research-oriented medical school in the US has looked with disfavor on those scientists who are losing their competitive strength in research.

The third strategy is to *remain a reputation-seeking biomedical scientist.* This option is open to those scientists who – in Great Britain – think they can stand the competition or who – in the US – are forced to stand the competition. Those who have chosen this way may leave the medical school and look for an appointment at an extra-university institution or for an appointment abroad, or they may fight for more "economic capital," both within the internal distribution process of medical schools and within the distribution process of the MRC and charities.

The *British* biomedical scientist who has chosen to continue his research career inside a medical school has been confronted with a rapidly changing institutional environment. Given the decrease in time available for research, he can cope by attracting a number of research fellows whose efforts can make up for the time he sacrificed to his other obligations in the medical school. Or he can attempt to become a member of a high-quality research group or center within a medical school. Being a member of a center of excellence promises additional support of funding agencies and more support of the department. In both cases he has to submit to the review process of research councils and charities. Many biomedical scientists have to get accustomed to writing research proposals and to the uncertainty involved in a distribution process based on reviews of grant applications.

Two problems remain, however: First, even though funding agencies have increased their support for university research, they cannot meet the rising expectations and demands for funds for biomedical research. Therefore, the contraction of financial resources has not ceased. Second, scientists have felt limited in their opportunities to follow the bent of their scientific curiosity by, for example, the MRC's switch to a policy of stipulating which fields, subjects or problems are to be investigated, or by having to turn to charities, government departments or industry for money.

In sum, the British biomedical scientist has had to live with less, to compete harder for his research resources, to submit to constant evaluation by review committees inside and outside the medical school (Anderson 1991), to do research in fields designated by others, and to learn how to cooperate in research groups and interdisciplinary research.

The British scientist has had to adapt to a competitive funding system. His *American* colleague has been confronted with a destabilization of the very foundations of such a competitive system.

The foundations of a competitive system are its legitimacy and its productivity. The selection based on competing applications for research money from the funding agencies was for a long time considered a justified means to keep up the productivity of the system. As long as it was believed that each scientist had a fair chance of being awarded research grants in the review process at the NIH, the system was highly esteemed. The discouraging effects of decreasing success rates started to raise profound doubts about the capacity of the competitive system to deal with the trouble generated by political disturbances. Dissatisfaction has been increasing among the biomedical research community ever since young scientists and physicians became discouraged by the frequent refusal of their grant applications. Young scientists and physicians began to apply for jobs in hospitals and industrial laboratories. Unrest intensified when the scientists who did not work in the most approved subjects were unable to obtain financial support. The situation has become untenable ever since even scientists with a very high reputation and outstanding research proposals had to face rejection. This situation, which has gradually destroyed the image of a competitive but fair selection in the distribution of federal money in biomedical research, has led to three coping strategies of biomedical scientists:

1. Both young and promising scientists and those with high reputations have begun to demand from their medical schools what was refused by the NIH, leading to the downward spiral in the research system described above.
2. In consequence of the increased probability of refusal, biomedical scientists have begun to develop a more cautious and defensive way of putting forward their research proposals.[23] Risky and innovative research is be-

23 In fact, one can observe an overall conservatism in research triggered off by the period of scarcity: NIH Institutes and reviewers were inclined to award grants only to proposals which had a very good chance of demonstrating visible success within a reasonable time-

ginning to lose ground.

3. The contracting research system has made for rivalry and jealousy among scientists in medical schools. This rivalry seems to be more pronounced than in the British medical schools which are still buffered by the dual funding system. The American scientist often depends on NIH grants for the major part of his salary. It is, therefore, not only his "economic capital" which is jeopardized, but also his very existence as a scientist. It is no wonder that social relationships in medical schools have deteriorated with negative effects on the productivity of the research system as a whole. There are abundant complaints about the loss of congeniality and the tendency to keep scientific information to oneself. Deans who have to redistribute resources within the medical schools encounter much resentment on the part of scientists and departments who refuse to share their income with others. Scientists who are lucky enough to acquire substantial grants obtain a powerful position in medical schools. They supply the medical schools with urgently needed resources through the indirect cost reimbursements. They are appreciated because they pay their own salaries from their grants. Deans are forced to reward these scientists with extra resources. More and more fiefdoms have developed, possessing an unprecedented amount of power in the daily affairs of medical schools which is sometimes abused and often envied.

On top of all this, even the scientists who are selected to review grant proposals at the NIH seem to abuse their position by using the information they obtain during the review procedure for their own purposes. The establishment of a "science police" at the NIH is an indication of the merciless and rude competitive struggle in the biomedical research system.

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span. Well-known scientists and research areas which traditionally seemed to be promising were favored in this process. The biomedical scientists' defensive way of putting forward research proposals anticipated this attitude of reviewers. In this way, a self-reinforcing circle developed.

6 Concluding Remarks

The description of political strategies and their consequences for biomedical research in the United States and Great Britain during the 1980s has demonstrated that the tension between rising expectations and demands of scientists on the one hand and insufficient supply of financial resources on the other could be relieved neither by funding agencies nor by medical schools. Though these agencies cushioned biomedical scientists to some extent from even more serious consequences for research, major problems remained and seem to be aggravated in the 1990s. If the decline in financial governmental support continues, the impact on the productivity of biomedical research and on its international competitiveness may be quite harmful in both countries.

Let us turn to the initial remarks concerning the analytical insights into the implementation process of governmental strategies in research policies: I think we can fairly state that there is indeed no straight and linear relationship between governmental policies and the coping behavior of scientists. But it is evident that no actor in the biomedical research system of the two countries could avoid being affected by the impact of declining resources, and no actor in Britain could oppose Mrs. Thatcher's policy of introducing the "value-for-money" principle and selecting and concentrating resources. Governmental policies have had a profound impact on biomedical research.

They have, however, been filtered – to the positive and to the negative – by intermediary agencies:

- In the US, the NIH could conclude an agreement with the Congress which has helped, at least for some time, to maintain investments into pure curiosity-oriented research; there was some room for a redistribution of resources in favor of curiosity-oriented research within the NIH.
- American medical schools amplified the trouble for biomedical scientists by demanding more indirect cost reimbursements, which displaced the resources reimbursed for the direct costs of research projects conducted by investigators.
- The UFC and the MRC sustained the selection and concentration exercise of the government but worked on its mitigation concerning the impact on pure curiosity-oriented research.
- British medical schools chose to distribute their resources in favor of teaching and patient care, thereby aggravating the financial strain of biomedical researchers.

These examples demonstrate that intermediary agencies translate political disturbances according to their own needs and according to their institutional situation:

- The NIH could bargain with the legislative branch for a mitigation of austerity policies. British agencies could not. This explains why the NIH could dampen the trouble, whereas the British agencies were forced to comply to governmental demands.
- American medical schools had to demand more indirect cost reimbursements because of their outmoded research facilities and strategic mistakes in the past. British medical schools had more room for maneuver because the dual funding system had provided sufficient resources for research for quite some time. In Britain the harsher austerity management of the government caused the deterioration of research conditions.
- The UFC and the MRC were interested in a reduction of institutional rigidities in medical schools. This led them to support governmental demands for selection and concentration. The NIH acted within a biomedical research system more flexible and adaptive than the British one. There was no cause for the NIH to support Reagan's demands in this respect.
- Finally, the British medical schools had a long tradition of organizationally juxtaposing teaching, patient care and research which disfavored research in the period of scarcity, while American medical schools were obliged to support biomedical research at all costs given their corporate identity.

It is the combination of political strategies, interests of intermediary agencies and national-specific institutional environments which determines the kind of trouble the biomedical scientist has to deal with in each country.

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Conflict Avoidance within a System of Centralized State–Science Relations: The Agricultural Research Sector in France

Gerhard Krauss

Introduction

The agricultural research sector represents a part of the French research system which, in comparison with other research sectors (such as atomic research) or research institutions (such as the CNRS), has rarely been exposed to harsh external criticism (from political actors, the agricultural sector etc.). Public discussions about it do not tend to be encumbered with political and ideological connotations. There seems to be a large political consensus about the necessity and social utility of agricultural research. Big science organizations like the *Institut National de la Recherche Agronomique* (INRA) are not only relatively undisputed – they are even respected by politicians and the general public. Thus, the agricultural research sector offers the opportunity to examine the ordinary, unspectacular interactions between actors of the political system and the science system. These relations may nevertheless be sources of conflicts and trouble which affect research conditions in different ways. Often, actors seem to try to avoid "real" conflicts. This is an important element of the relation between the French state and science. But the mechanism of conflict avoidance which definitely exists in the French system does not preclude the creation of mock trouble which may lead to very strong struggles on a symbolic level.

In the following analysis,[1] attention will be paid particularly to the interplay between the institutional structures of the political system and the research system in the domain of agricultural science, and to the effects of

1 This article takes up certain aspects of my doctoral thesis (for more details see Krauss 1993).

interdependencies between actors on situations of trouble. The major questions will be: What kinds of state interventions and what kinds of trouble are likely or possible under the given institutional structures? And what are adequate coping strategies for actors under these conditions?

1 Institutional Background

The institutional structures of the political system in France, which in the past have often been described as highly centralized (for an overview, see for example Suleiman 1974; 1987) and which are related to the idea of a strong and interventionist state, may suggest that there are only few stable or socially protected spheres of autonomy for state-financed research organizations. This view could refer to the interventionist tradition of the French state in other domains, especially in the field of economic and industrial policy (see Zysman 1977; Dannebom et al. 1984; Fach/ Simonis 1987; Cohen 1992). Some authors therefore recently characterized French science as "state-led science" (Baumgartner/ Wilsford 1993).

The traditional image of the unitary French state can lead us to forget the limits to strong state power (Wilsford 1989). This prevalent picture suggests a big trouble potential for research institutes. However, especially for the research sector, recent studies have qualified the common idea of the strong, interventionist state in France and pointed out that political centralism can easily coexist with numerous internal conflicts of interests in the state administration (see Callon et al. 1986). The comparatively influential and centralized state consists itself of powerful units and subunits with particular interests which are sources of administrative conflict. In the past, in the field of science policy different ministries were responsible for different research institutes. Conflicts appeared not only across but also within ministries. Moreover, the research system was structured around several big research institutions which represented important power centers, too. One of the major topics in the history of French science policy therefore was the problem of coordination of the different power and decision centers (Papon 1988: 496; see also OECD 1986: 60-84; Lesage 1992).

Perhaps the most important peculiarity of the French research system concerns the form of its institutional differentiation and especially the predominant model of research organization. For the delimitation of domains between research institutions, for example, the *type* of research is not the principal criterion, though it may play an important role in several cases. Instead, the institutional differentiation seems to be more oriented toward different problem areas or sectors of public policy. This structure has not only and not principally emerged from actions of scientific actors, but is above all a product of specific state intervention patterns and internal conflicts within the state.

At an early stage, the centralized social and political structures in France favored the centralization of research structures. For example, an attempt was made to institutionalize an immense research organization for all disciplines and theoretically all types of research. Actually, this all-embracing research center from which today's *Centre national de la recherche scientifique* (CNRS) eventually emerged was not able to fulfill the politically assigned mission, namely to coordinate all research activities, i.e. all disciplines and both fundamental and applied research. One important reason was that the CNRS had no authority over the different political administrations, which financed their own specialized research centers and were not very receptive to coordination. Apparently, there was no central political agency capable of neutralizing such particularistic interests. The CNRS's difficulties with coordinating the whole research system explain its reorientation toward fundamental research after the Second World War (Picard/ Pradoura 1989: 38). But, although the CNRS henceforth had an image of being an organization devoted primarily to basic research, it in fact remained a rather polyvalent institution. Compared to the German Max Planck Society, for example, many CNRS laboratories are dependent to a comparatively high degree upon external resources which they can acquire only by making concessions to the social demand.

Consequently, the institutionalization of new specialized research centers besides the CNRS after the war was mainly the result of a certain political constellation and only secondarily of boundaries between different groups of scientists. Most of these research institutes emerged according to this logic (Rouban 1988: 92-93). This is true for the *Commissariat à l'Energie Atomique* (CEA), where the political component is particularly evident, as well as for other research centers like the INRA, the institution for agricultural research which is the subject of our case study. It was typical for the history of the French research system that for almost every important research field (atomic research, medical research, agricultural research, space research etc.) a big research organization[2] was founded which served as reference point and guidance agency for the respective research sector, and whose research activities were not limited only to applied research but included fundamental research activities as well.

2 The Agricultural Research Sector in the French Research System

According to the logic of development of the extrauniversity research sector described above, the French agricultural research sector of today is structured around the big *Institut national de la recherche agronomique* (INRA).[3] This institute was created after the Second World War on the basis of the former *Institut de recherches agronomiques* (IRA). As part of the postwar reorganization of the French research system, this new organization copied in part the model of the CNRS (see INRA 1986: 17), which meant that it incorporated a certain degree of autonomy from political actors, the inclusion of all relevant fields of agricultural science, certain guidance capacities across the sector, and a philosophy implying that applied agricultural research must not only be open to social demands but closely connected to basic research, too (INRA 1986: 23-24). The economic hardships and problems with the food supply during the postwar years, as well as the discussion about the backwardness of French agriculture, facilitated the realization of this project greatly (INRA 1986: 15; Muller 1984: 22).

2 This could mean a real big science center like the CEA, which implies a certain kind of research and research organization, as well as the mere concentration of a great number of research units like INRA. In the case of space research, on the other hand, the *Centre national d'études spatiales* (CNES) acts primarily as a funding agency and as an institution of political guidance, the main part of its funds being allocated to applied projects of private industry. A small part of the funds is earmarked for the support of basic research laboratories, which are very costly to equip.

3 For an overview of the French agricultural research sector, see also Kellermann (1988: 83-88).

INRA, which has a staff of about 8,500 employees (INRA 1991: 44), has a dominating position in agricultural research today. At the national level there are only a few smaller institutions besides INRA,[4] such as the *Centre de coopération internationale en recherche agronomique pour le développement* (CIRAD), with a staff of about 2,100 employees (CIRAD 1991a: 98), the *Centre national du machinisme agricole, du génie rural, des eaux et des forêts* (CEMAGREF), with a staff of about 1,000 (CEMAGREF 1991: 4), or the *Centre national d'études vétérinaires et alimentaires* (CNEVA), with a staff of about 500 (CNEVA 1991). Another actor in the field of agricultural research is the *Institut français de recherche scientifique pour le développement en coopération* (ORSTOM),[5] with a staff of about 1,600 (ORSTOM 1990: 58), but the pure agricultural research represents only about a third of its activities and is, moreover, oriented to the problems of developing countries. Finally, institutions which also play an important role are the *Instituts et centres techniques agricoles* (ICTA). These institutes are differentiated by different lines of agricultural production and financed by a parafiscal tax raised on agricultural products. Due to their mode of financing and their specific relations to both the agricultural sector and the state-financed research institutes, they play an important interface role between these two sectors. In fact, they represent a semi-public sector which is internationally unique. The ICTA, which are federated in the *Association de coordination technique agricole* (ACTA), have some 1,500 employees altogether (ACTA 1990: 9). A similar federation is the smaller *Association de coopération technique des industries agro-alimentaires* (ACTIA) for the sector of the food and agricultural industry. Besides these institutions, the agricultural research capacities are sparse. In contrast to Germany, for example, the French higher educational sector in agricultural sciences (universities, colleges) is much less research-oriented.

The INRA is the institution of the sector with the widest spectrum of basic and applied research. However, the applied character of its research has been preserved to a higher degree than, for example, in the case of the comparable *Institut national de la santé et de la recherche médicale* (INSERM), a medical institute whose orientation has tended more and more toward basic research.

4 A description of their functions will follow in lieu of a literal translation here.

5 Until 1984: *Office de la recherche scientifique et technique d'outre-mer*. The old abbreviation was retained after the name was changed.

One can suppose that the continuing strength of applied research at INRA results from the higher degree of embeddedness in regional and local structures, which favors more intensive relations with the agricultural sector. One indicator of this is that in 1990, only 30% of the INRA personnel were concentrated in the Paris area (INRA 1990: 15). This is relatively low compared with the whole public research sector, which showed an average concentration of more than 52% around Paris in 1989 (Ministère de la recherche et de la technologie 1991: 117). On the other hand, compared to other agricultural research institutions, INRA is subject to relatively little economic pressure: In 1990, INRA's earnings[6] represented just 12% of its entire budget (INRA 1991: 45). At present, it can be highly selective in the choice of projects when it comes to acquiring external resources by means of contract research. Therefore it is relatively free to carry out also projects oriented toward basic research. Thus, in 1990 public funds represented about 88% of the INRA budget (INRA 1991: 45), some 70% of which were to cover personnel costs. Scientists expressed the opinion that laboratories were often forced to solicit external funds matching the funding they received from INRA just to maintain an adequate scientific infrastructure in the labs.

Whereas INRA is a research organization which combines different types of research (from basic research to applied research and even certain service functions outside research), the other agricultural research centers emphasize more the applied part of their activities and the complementary missions outside research. The CIRAD, for example, whose research activities are limited to certain geographic regions (tropical regions and developing countries), tries to keep its basic research at a necessary minimum. Often, CIRAD research programs are organized along certain lines of tropical agricultural production. In addition, its own resources are higher than INRA's, representing 37% of its budget (CIRAD 1991a: 97; 1991b: 40). The voluntary limitation of basic research is also true for the CEMAGREF, which works in the fields of agricultural mechanization, environmental planning and farming equipment, originally the principal agricultural problem areas not covered by INRA (Ministère de l'agriculture 1981: 78-79). In the case of the CEMA-

6 Technically called its "own resources," i.e. resources not allocated by the state, these include income from research contracts registered by the central administration of INRA, livestock sales, sales of agricultural products, provision of services (e.g. analysis), licenses and patents, real estate rentals and sales and, finally, publication sales.

GREF, the spectrum of research activities is very broad and can lead to very specific applications, sometimes even to the development of prototypes. The CEMAGREF's own resources amounted to nearly 30% of the budget in 1990. Moreover, it received about 15% of its budget from the Ministry of Agriculture for activities outside research, especially for technical support (CEMAGREF 1991: 4). Finally, there is the special research center CNEVA, whose main tasks are technical support in the areas of animal hygiene and diseases, food safety of animal products, participation in programs of health protection, and the development of norms (see Décret no. 88-478, 1988). Because it is legally charged with many important licensing and control functions, which are estimated by CNEVA scientists to amount to 50% of all activities, the CNEVA is the institution which is most dependent on external constraints imposed by the state.

Besides all these research facilities, the sector of the technical centers and institutes is functionally differentiated as the part of the research system which is mainly oriented to development. While scientists at INRA tend to have a basic research profile, scientists at the CIRAD, CEMAGREF or the CNEVA tend to have an engineering research profile, as do the scientists in the technical centers and institutes belonging to the ACTA. The activities of technical centers and institutes, being directed toward agricultural development, must, however, be principally distinguished from those of the preceding institutions. Among other things, engineers in technical centers have a very broad knowledge of a certain line of production and are less detached from practical problems of agriculture than perhaps an engineering researcher at, say, CEMAGREF who is specialized in one narrowly defined field.

In many countries, agricultural research institutes are often dependent to a high degree on political authorities: The pattern of ministerial research institutions, which are part of the central state administration and therefore subject to directives of the ministries (in Germany, for example, the *Bundesforschungseinrichtungen*; see Hohn/ Schimank 1990: 297-341), is quite typical. Paradoxically, despite state centralism and interventionism, French agricultural research diverges from this organizational model, allowing the individual institutions a surprising amount of freedom. Compared to their German counterparts, French agricultural research institutes are more independent from the different political administrations and generally have a certain autonomy conceded by the state. Nevertheless, they all have a public status (a fact which theoretically opens several possibilities for state control), and they are all closer to the political power centers than, for example, research organizations like the German Max Planck Society. This constellation offers specific opportunities for attempts to exercise control by state actors which induce both specific kinds of trouble and special coping strategies – the subject of our empirical analysis in the case of INRA.

In general, the political control of these organizations seems to be more complicated than in a system of direct hierarchical control relationships. Also, political actors may find it more difficult to get direct access to the researchers' results. However, the fact that the directors of public research institutions are appointed by the political actors and are accountable to their ministers creates a certain relationship of political dependence which may have important effects on the internal management of these organizations. In addition, the participation of the responsible ministries in the boards and supervisory committees of research institutions offers, of course, certain possibilities for political control. But these bodies do not necessarily exercise their prerogatives down to the level of detailed project management.

Although the leaders of research institutions are responsible to the political administration, i.e. to different ministries, this does not mean that the political actors intervene in their research programs. The real external influence in many cases comes rather from the cognitive orientation in reaction to the social demand, which is more or less a product of the intense relations with social and economic actors. The role of the responsible ministries is often confined to overseeing the proper functioning of the research institutions in two ways: ensuring that they remain true to their institutional mission of fulfilling social and economic needs of the society, and supervising their compliance with formal administrative regulations.

The variations among research institutions regarding their formal status is only a minor determinant of their political dependence. INRA is rather free from short-term external demands. Because it fulfills a multitude of functions – as the central research actor of the agricultural sector covering almost all agricultural research fields, it compensates for the lack of research capacity among professors in the sector –, it is much more resistant to short-term instrumentalization by political actors. INRA itself represents an important power center, due, for one thing, to its sheer institutional size, and also to the fact that it is not only, or even primarily, limited to strictly applied research and technical support services, but also is responsible for the production of basic knowledge in agricultural research on a large scale (in all cases where the CNRS does not already fulfill a part of this function). The most important reason for the power of the institution thus is its *monopolistic* position in agricultural research. Because of this research monopoly, political actors cannot play the research actors off against each other. Finally, the present financing structure of INRA does not open many possibilities for direct political guidance. Today, INRA receives the majority of its resources from the Ministry of Research, which does not seem to consider agriculture to be a priority;[7] the Ministry of Agriculture provides only 0.7% of INRA's public funding (see INRA 1991: 45). This small share has always been earmarked for the supplementary missions outside research, originally in the form of the financing of a certain number of positions. Over the years, the agreements concerning these non-research activities have never been updated by the ministry, so that these positions have become just a budgetary category hardly associated with specific employees. Recently, the new tendency of INRA is to create branches for those activities. Because the state seems to find it more difficult to instrumentalize INRA's research activities directly than it does those of other agricultural research organizations, this instrumentalization plays a less important role for INRA than it does for the others.

However, after establishing a new powerful Ministry of Research in the 1980s, the state also wielded a stronger instrument in the domain of project funding and therefore could more easily guide research activities of institutions like INRA than before. Certainly, the funding agency of the Ministry of Research, the *fonds de la recherche et de la technologie* (FRT), appeared to be more intertwined with the political power structures than the former *délégation générale à la recherche scientifique et technique* (DGRST). This largely explains the frequent change in the administration of certain research programs like the biotechnology program. In fact, due to its strategic position, INRA is an important actor for the realization of the agricultural research programs of the ministry[8]. But the way in which the state really guides the

7 The department responsible for agricultural research in the Ministry of Research is not very important. Thus, in general, INRA does not have to fear much trouble coming from this ministry. However, the Ministry of Research plays an active role in guiding INRA research indirectly, namely through project funding. This is the case when the ministry finances interdisciplinary research programs which include agricultural research institutes.

8 For the different research programs of the FRT, see *Projet de loi* (1991: 105-121).

research of INRA is less direct than in the case of technical support services and may be perceived by scientists as less disturbing.

The fact that several agricultural research institutions act in specific subareas which have not been integrated into INRA for historical or political reasons – and are therefore exposed in different ways to the political influence of different ministries – does not stop INRA from playing an important role for the scientific orientations and the development of the sector as a whole.[9] First, INRA's great asset is its scientific knowledge and competencies on which the other smaller institutes are rather dependent. It monopolizes most of the public funds spent on agricultural research in France. Secondly, it has several ways of realizing its own policy for almost the entire sector and of influencing the research policy of other actors. INRA representatives participate in the boards and scientific councils of the other organizations. The mutual participation and institutional links between these corporate actors are more or less asymmetric in favor of INRA. The president of INRA is a member of the board of the CEMAGREF and the CIRAD, and often the president of INRA and of the CIRAD has been the same person. For many years, the president of INRA also served as INRA's director general. He had a particularly strong position because of his participation in numerous committees inside and outside the research sector, even in those of private enterprises (for example, the board of Rhône-Poulenc etc.), and because of his wealth of connections to influential people. Since the functions of the president and the director general were separated, however, the former receded somewhat into the background in favor of the latter.

In the case of INRA and the CIRAD, in addition, the scientific councils have the same president. The first director general of INRA after the reform had previously been the director general of the CIRAD. Moreover, INRA representatives have been granted an important role in the scientific councils of other research institutions. For example, the scientific director of INRA is member of the scientific council of the CEMAGREF and chairman of the scientific council of the ACTA, the federation of the technical centers and institutes. In fact, INRA is also deeply involved in the affairs of technical centers and institutes, a strategically important sector for the diffusion and

9 However, this does not go as far as a coordination of the different agricultural research institutions; each of them, in fact, represents its own domain with its own interests. This "fragmentation" of the sector has been sharply criticized (see Poly 1988: 3).

popularization of new knowledge because of its access to the agricultural producers through the different organizations of agricultural development. In general, the scientific and technical council of each institute is directed by a scientific director of INRA who is the INRA specialist for the respective area. This may favor the exchange of information between INRA and the agricultural sector, but, above all, it shows the numerous institutional links between INRA and the other research actors and the breadth of INRA's social influence.

Apart from these institutional links, INRA can act as a funding agency and invite tenders for projects. These are often financed by its head office,[10] but INRA's various departments can also provide incentive grants,[11] not only to INRA laboratories, but also to external researchers and research groups. Examples of bigger research programs set up recently and realized by means of incentive grants are the intersectorial programs AGROBIO (research on food) and AGROTECH (research on environment and the management of natural resources), endowed with a total of 18 million francs each for three years, beginning in 1990 (*Projet de loi* 1991: 188; INRA 1991: 44). In addition, INRA is also able to pursue a research policy toward the higher-education sector through the association of laboratories. The system resembles the CNRS system of the associated research units and laboratories somewhat, but is much less structured. The decision about an association is taken by the head office of INRA according to its scientific priorities. In sum, INRA has achieved an important strategic position for the guidance of French agricultural research on a general level. For the state, it therefore represents a decisive actor within the framework of an agricultural research policy. On the other hand, its strategic position is the basis of its capacity to counterbalance external interferences from the state. This rather ambivalent relationship between the state and INRA leads to specific kinds of politically induced trouble. In the following, we will concentrate our analysis on this relationship and study the kinds of trouble which can emerge under these specific institutional conditions, as well as the possible coping strategies.

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10 Between 1986 and 1989, the expenditures for project funding by the head office of INRA represented between 4% and 5% of all operating expenditures and on average about 4% of all expenditures for materials (see INRA 1990: 7 and 9).

11 However, in the past, only a minority of INRA departments (for example, the department Rural Economy and Sociology) used this possibility on a large scale.

3 The Division of Power in Agricultural Research Policy Between State Actors and INRA: Disruptive Effects on Research Activities, Possibilities for Politically Induced Trouble and Coping Strategies of Research Actors

The division of power in research policy between one or several political actors (ministries) and one important research actor for each of the important problem areas (or for the research system as a whole like the CNRS) is very typical for France. In this respect, the agricultural research sector is no exception. However, it differs from other sectors with regard to its specific relationship with the corresponding economic sector. In the past, the intensive contact of INRA to the professional organizations of French agriculture, and occasionally even to farmers, represented an important additional factor which enhanced its strong political position. In fact, INRA was almost more powerful than the Ministry of Agriculture and, consequently, the decisive actor in the field of agricultural research policy in France. Since the recent establishment of the powerful Ministry of Research, the constellations of power have changed, however, because its portfolio includes much of the political responsibility for INRA. But the basic character of the relationship between the central state and INRA has remained almost the same, except that on the state side, the important actor is no longer the Ministry of Agriculture but the Ministry of Research, and the political responsibility over INRA is divided up between both. In the following we will first turn to the period before the institutional reforms and then pass on to the present constellation. During the first period, the Ministry of Agriculture was solely responsible for the bargaining and the allocation of the INRA budget as well as for the choice of candidates for the appointments at the top of INRA by the Council of Ministers. Today, in fact, it is essentially the Ministry of Research which is responsible for the allocation of public funds for the INRA budget and which negotiates the respective budgetary demands in a bilateral bargaining relationship with INRA. Comparatively, the part of the budget coming from the Ministry of Agriculture is very low. The two ministries share the responsibility for appointing the chairman of the board and the director general of INRA as well as the control of certain budgetary operations, whereby each ministry has a right of veto.[12]

In the context of this specific state–science relationship, trouble for research actors (for INRA as a corporate actor as well as for its different groups of researchers and for individual scientists) emerges essentially from the contradiction between the state's interventionist self-image and its actual lack of authority to intervene competently in areas about which it has no knowledge. Therefore, scientists aware of this have to guard themselves against the harmful effects of political interventions all the time. It is true that in France political actors frequently tended toward an interventionist policy style. According to this general pattern, until the 1980s different ministries claimed to have a certain amount of control over research institutions, as for example the Ministry of Agriculture in the case of INRA. Nevertheless, the domain of research did not have a high priority for this ministry, and the department responsible for INRA was composed of only a few officials. Despite the obvious difficulties involved in guiding an institution like INRA, there was every reason to believe that, due to their rank and to the specific French vision of the role of the state, these officials may have continued to cultivate the illusion of being capable and authorized to control French agricultural research. This was a more or less latent element of the relationship between the state and agricultural research, even if the corporate actors of the agricultural research sector actually often seemed to manage their affairs in a rather autonomous manner. Above all, it was the mere possibility that the ministry could try to conduct its own agricultural research policy, based on relatively little information and specialized knowledge compared with INRA, which could have led to disturbances in the agricultural research sector. To prevent such disturbing effects, it seems that INRA often pursued a rather offensive strategy, proposing its own conceptions for the future development and guidance

12 Both ministries must agree not only on the appointment of the president and the director general of INRA, but also on appointment of members of the board other than the representatives of the state or of the personnel (Décret no. 84-1120, art. R. 831-4, 1984). Apart from that, each has a right of veto for certain budgetary decisions of the board, particularly for modifications of the budget and the budget account, loans, acquisitions, exchanges and sales of buildings. The participation in *groupements d'intérêt public* (GIP), changes in the financial support of affiliated institutions and the creation of new branches also require the approval of the Minister of Finance (see Décret no. 84-1120, art. R. 831-7, 1984).

of agricultural research, which could easily contradict the ministry's approach. Chiefly the capacity to act (or react) in early stages of the elaboration of new agricultural research policies and of the definition of new objectives and priorities shielded INRA in the long term from more important disturbances. In this respect, the closer connection to the power centers of the state did not ultimately favor so much political guidance by the state but rather the influence of INRA in the French agricultural research policy. Nevertheless, INRA always had to reckon with occasional attempts at intervention from different state actors. The relatively offensive strategy of INRA can easily be explained by its leading position in a certain network of scientific actors. As mentioned above, INRA had a monopoly on a vast wealth of scientific knowledge and competences, it represented a huge institutional structure capable of guiding the whole agricultural research sector, and it had intensive contacts with the private sector of the French society. The combination of all these factors was a prerequisite for a successful strategy of INRA toward state actors under the given structures.

The bilateral relationship in a state-oriented system between one principal state actor which was politically responsible and one central research organization could also be a source of discontinuities and disruptive effects on the research planning. In addition, from the state's perspective, interferences could come from higher political levels such as the office of Prime Minister or even the President. Under these institutional conditions, it was highly probable that political action would affect research conditions (in a positive or negative way). A rough indicator for this phenomenon may be, for example, the variations in the development of the INRA budget or in the growth of INRA staff. In fact, in this respect INRA was sometimes subject to very radical changes.

While the period before the 1970s was characterized by rapid growth, the 1970s were marked by austerity. To some extent, this could be related to the general economic situation of France, but the scope of the changes could not be attributed to economic factors alone. For example, at the end of the 1960s under de Gaulle, the staff of INRA was expanded each year. In stark contrast to this, during the 1970s, there were years when INRA did not hire hardly any new personnel. French governments during the 1970s gradually reduced public expenditure on scientific and technological research in general. As a consequence, the percentage of national expenditures for research and development fell from 2.1% of the gross national product in 1967 to 1.8% in 1980 (Papon 1988: 497). In this context, the legitimation by economic arguments was probably only half of the truth and concealed the actual political motives, inspired by a rather liberal ideology which implied a depreciation of state-financed research. During this period, the relationship between INRA and the ministerial authority was rather difficult. The general decrease of public funds gave INRA scientists the impression that the state authorities were trying to "strangle" research.

Essentially, two major disturbing effects could be observed: first, the unpredictable changes in the creation of new positions by the state jeopardized the continuity of research with regard to its internal criteria of assessment. Radical changes in recruitment policy, imposed by the state, implied strong variations of the selection criteria of scientists. Whereas in "good" years even a number of mediocre researchers could be hired, in "bad" years not even the best could be hired. The fact that abrupt changes were possible in the short term affected the research activities of INRA laboratories in different ways. It represented a factor which could seriously handicap their normal and long-term research planning. Secondly, the autonomy of research laboratories vis à vis social demands was greatly weakened. In comparison to the present situation, the different research groups and laboratories were extremely dependent on external resources at that time.

It is difficult to get exact empirical information about the situation of laboratories during this period, but it seems that the capacities of research groups to cope with these elements of trouble varied markedly according to the domain of research, and, most significantly, according to the scientific reputation of the respective research group, i.e. its recognition by the peers and its position in the scientific community. In this respect, there is every reason to believe that the "good" laboratories, which principally oriented their activities according to the internal logic of the research system, succeeded better than the "bad" laboratories in obtaining research contracts and therefore being able to finance basic research. It was essentially a matter of convincing the clients who financed research projects of INRA research groups that application-oriented projects always needed at least some basic research. With such a strategy, laboratories could use external money to finance long-term research programs. But this could not hide the fact that a part of the research capacities were paralyzed by the rather costly, incessant search for external funds, and that laboratories generally became more and more susceptible to external demands from political or economic actors. It is clear that under these conditions, the research conditions of already weakened laboratories sometimes worsened dramatically and that the respective research groups were increasingly at the mercy of all sorts of powerful actors who were able to give money for research. In fact, a laboratory which had to accept a great number of small contracts, and thus had to submit to very specific non-scientific demands, was in an almost desperate situation if it wanted to maintain a certain reputation in the scientific community and remain in touch with ongoing scientific developments. The decline here resembled a vicious circle.

In general, the long-term orientation of research required a steady and predictable development of public financing, as well as a certain guaranteed minimum of basic state funding for laboratories. Not all INRA laboratories were necessarily exposed to the same degree to external pressure, and they did not have the same coping strategies at their disposal. The unpredictable changes in the recruitment policy greatly handicapped the research activities of laboratories, which needed a certain margin for staff renewal to be able to support the work of new first-rate researchers. To cope with this obstruction of the continuity of scientific developments, the laboratories often had to invest a great amount of time and energy into getting additional private funds. Unfortunately, we do not have any exact information about the real extent of external resources, especially with regard to the payment of scientists, because of the lack of information concerning the informal financing structures. Since administrative regulations hindered the direct employment of researchers by the laboratories, the different laboratories developed their own strategies in order to bypass these administrative difficulties. It is rather awkward and difficult to ascertain the real importance of different financing mechanisms. But it can surely be supposed that the research actors developed such non-official practices under the given institutional structures.[13]

It can be assumed that the political and economic context of the 1970s rendered INRA more susceptible to external influences than in periods of sufficient public financing. Often, the relations to the state during this period have been characterized as rather difficult by INRA representatives.[14] Since INRA's position had already been weakened, state actors also seemed to

13 One possibility, for example, was for a lab to create an association on the basis of the law from 1 July 1901. Using private funding, an association could facilitate the temporary employment of researchers. The state had little opportunity to control this type of organization because it was hard to get centralized information about it.

14 According to my interviews with representatives of INRA.

double their efforts to impose their views on the INRA management. In this respect, a former director general, for example, reported rather contradictory attempts of political actors to interfere in the affairs of INRA during the presidency of Giscard d'Estaing. At that time, INRA sometimes received very contradictory orders from the Minister of Agriculture, the Prime Minister and the office of the President. Political actors asked INRA to set priorities in certain scientific domains where it should principally recruit new scientists, but the state created only a fraction of the jobs requested. Above all, this was a question of the balance of power between political actors and the management of INRA. On the one hand, the fact that the INRA management had comparatively intensive relations with political actors could be a source of political interferences. But on the other hand, this could also provide a basis for promising defensive strategies.

The decisive factor in the centralized, state-oriented system, which implied bilateral relationships between state and research actors, was probably the specific role of the INRA management as a mediator between the political system and the respective scientific communities. In order for INRA's leaders to be successful mediators, they had to have a reputation for authority among the communities. A good example is the case of Jacques Poly, the director general from 1978 to 1980 and president/director general from 1980 to 1989, with an independent, strong personality, good knowledge of the institution and important social connections. He profited from this strong position in his interactions not only with political actors, but also with scientific actors inside INRA. On the one hand, he could realize his own policy for the institution (even against certain internal research actors) and, on the other, he had to defend the interests of agricultural research in relation to the state. In this ambivalent relationship, the autonomy of INRA was based partially on the successful strategy of its director general or president/director general. The fact that he was directly responsible to the minister (with the theoretical possibility of being sanctioned and dismissed) favored an intensive relationship with political actors which easily could increase the political importance of the INRA management. For example, Poly reported[15] that he participated in all important meetings of the ministry and that many Ministers of Agriculture even summoned him to the meetings of their directors, where it became an unwritten rule that the director general of INRA was the last to speak.

15 In an interview on 14 February 1992.

The political attempts of state actors to interfere in the affairs of INRA tended to concern problems of an administrative character more often than scientific orientations. But administrative conflicts could also have negative effects on research activities. To keep a rather independent, strong position in relation to other political actors, the INRA management seemed to find it advantageous to oppose attempts at political interference even in mere administrative questions. An example was the case of the appointment of a deputy to the director general for administrative affairs *(directeur général adjoint administratif)* in the 1980s, who normally had to be appointed by the Minister of Agriculture and the Minister of Research upon the recommendation of the president/director general. When the Minister of Research at that time, Jean-Pierre Chevènement, tried to appoint his own candidate, the president/director general of INRA opposed him successfully. If the minister had really wanted to appoint his candidate, he would have had to dismiss the president/director general. This would have represented a disproportionately strong intervention with unpredictable political consequences. Often, the conflicts with the political authorities seemed to be rather risky for the president/director general of INRA. But a successful strategy here could help to increase the autonomy of the institution significantly. Perhaps the most disturbing effects in the everyday life rather came from the lower levels such as the technocracy and the minister's political advisors, who often had a great deal of power without being politically responsible.

After the major institutional reforms in the early 1980s resulting from the new research policy conducted by the left-wing governments,[16] the political responsibility over INRA was divided between the Ministry of Agriculture and the Ministry of Research and Technology. The reform implied an important loss of power for the Ministry of Agriculture, the main part of the INRA

16 For a short overview see Papon (1988). Important elements were that from now on research was defined as a national priority and that the civilian budget for research and technology was to increase annually by 17.8% (13% for basic research) for the period from 1982 to 1985. In addition, the number of scientists in public research institutes was to increase by 4.5% every year. The objective was to increase the national expenditure on research and development from 1.8% of the gross national product in 1982 to 2.5% in 1985 (see *Journal Officiel* 1982: 3-4 and 9). For the first time in French history, a Ministry of Research and Technology was established (Papon 1988: 498). Such research institutions as the CNRS or INRA were transformed into *établissements à caractère scientifique et technologique* (EPST), a new legal status created for research institutions.

budget being negotiated from now on with the Ministry of Research. However, the division of political responsibility, especially significant with regard to appointments at the top of INRA, still offered certain opportunities for the Ministry of Agriculture to exert political influence. Since the INRA management had an important function at the interface between the government and the scientific community, political conflicts concerning the appointment of the INRA management could have far-reaching effects on the work and orientations of research actors. This could be another kind of trouble for the research activities of INRA. On the other hand, the disturbances also could be merely budget-related. The importance of the role of the INRA management in the context of coping strategies emerged, for example, after the change of government in 1986, which led to radical cutbacks in public spending on civilian research.[17] It was true that the budget which had already been voted on was a relatively generous one, but the abruptness of the decrease was not easy for research actors to deal with. Accordingly, during the period of the Chirac government from 1986 to 1988, the relations between INRA and state authorities were sometimes strained. In the case of one particular Minister of Research under Chirac, the INRA management considered itself confronted with a rather crude attitude toward agricultural research; the minister, for example, saw no need for agricultural research institutes to be supplied with sophisticated material or installations. In fact, the political authorities' expectations were somewhat contradictory at that time. For example, the political actors wanted INRA to make more efforts in the diffusion of scientific knowledge. To satisfy this demand, INRA would have needed, for instance, a certain number of highly qualified engineers with correspondents in the regions or partners in industry. But the Ministry of Research did not want to create new positions for this task. In such conflictual, problematic relationships, an active strategy of the INRA management turned out to be crucial for the capacity of the institution to maintain a certain autonomy and distance from its politi-

17 When the neo-Gaullist government led by Chirac came into power in 1986, the 1986 budget was annulled and the budgets of the large research organizations virtually frozen (see Coles/ Maddox 1990: 125). The objective was to reduce public spending on civilian research in general, hoping that in return the private sector would carry more of the burden of research support. This only applied, however, for a short period. After the renewed change of government in 1988, the public spending on civilian research increased significantly once again.

cal environment. As a reaction to the cutbacks in public spending on research, the INRA management intensified its public propaganda (in conferences for example) and mobilized its social contacts (even through meetings with politicians) in order to denounce the contradictory, substantial interferences of political actors. This strategy unexpectedly proved to be partially successful: in one fiscal year the French Senate gave back additional resources to the INRA budget (which can be considered unusual in light of the tradition of French state–science relations).

INRA also employed internal adaptive strategies, however, to cope with the trouble caused by the cutbacks between 1986 and 1988. Today's INRA representatives sometimes characterize the cancellation of the research budget in 1986 and the general reduction of public research spending by the Chirac government as not having been very disturbing, because INRA had been able to build up certain reserves in the preceding "fat" years ("organizational slack," as it is referred to by Cyert/ March 1963: 36-38), and since the decrease of public research spending only lasted a short time. Above all, it is true that the Finance Bill of 1986, voted under the socialist government, was a comparatively generous one. The effects of the reduction in public operating and investment funds by approximately 10%, imposed by the Chirac government, were mitigated as much as possible by a selective reduction of budget titles by INRA. Among other things, INRA had to reduce its capital expenditures considerably after the cancellation of the budget. For example, the original plan to buy expensive data-processing computer equipment was suddenly stopped by the change of government in 1986. But since the austerity phase in public research spending lasted only two years, the political change implicated only a postponement of this purchase.

Compared to this more budget-related trouble, trouble pinpointing political responsibility, which resulted from the formal hierarchical relationship between the ministers and the directors general or presidents, could affect the research activities and the functioning of the institution much more strongly. The division of political responsibilities between the Ministry of Agriculture and the Ministry of Research in this context created additional sources of potential trouble. An example was the conflict between both ministries concerning the appointment of a successor for the long-standing president/director general of INRA in the second half of the 1980s. Rather than being politically motivated, the conflict was related to the candidate himself. The candidate was rejected by the Ministry of Agriculture probably because he was not a graduate of a *Grande Ecole* for agricultural sciences, but a *polytechnicien* who had much authority. The minister seemed to fear that the new president of INRA would escape from the ministerial sphere of influence and obtain a too independent position. The conflict finally was the source of serious trouble which affected INRA as a whole. The rejected candidate, who until then had been the deputy to director general for scientific matters *(directeur général adjoint scientifique)*, left the institution together with the former president/ director general. The politically induced paralyzation of the INRA management appeared to have quite negative effects on the scientific orientations of research groups. The president/director general's dual role in agricultural research and politics and his broad range of responsibilities had already become an essential element of the system as a whole. Within INRA, he monopolized the information needed to develop suitable strategies for dealing with political actors. In addition, because the long-term research policy of INRA was essentially his creation, the others were not familiar enough with it to implement it properly. Consequently, there was a power vacuum when he and his deputy left INRA. As a result, the institution lost much of its "memory,"[18] because potential successors did not have enough information about adequate strategies in the game with political actors and because most of the important elements of its program scarcely existed in a written form.

The conflicts between the two ministries finally created uncertainty among the scientific communities with respect to the future scientific orientations. In the course of the rather uncertain succession of the INRA management, INRA's research policy gradually began to dissolve. As a reaction to this uncertain stage, many scientists seemed to retreat more into their respective scientific community and to cut themselves off from the social environment. This represented a kind of passive coping, a strategy of seclusion, which simply ignored the politically induced trouble on the level of the global research policy of INRA. Apparently, this period was characterized by a general tendency toward basic research, while the applied researchers and research

18 Herbert Simon pointed out the negative effects of the turnover of personnel for the organizational memory: "Since much of the memory of organizations is stored in human heads, and only little of it in procedure put down on paper (or held in computer memories), turnover of personnel is a great enemy of long-term organizational memory" (Simon 1991: 128).

groups became increasingly isolated. INRA representatives have characterized[19] this development as a certain deterioration of social relations, each research sector having the tendency to make its own particularistic policy and to protect its own domain against the rest. Scientific cooperation across different sectors became quite rare.

The problems concerning the nomination of the president/director general by state authorities finally led to a temporary appointment of an uncontroversial candidate who remained in office for only about two years. Because of his advanced age, it was clear from the beginning that he would not remain in office for a longer period. Moreover, it was generally known that he assumed this function out of a sense of duty rather than out of conviction. Interestingly enough, after this period of transition, the candidate who had been rejected in the late 1980s was appointed president of INRA by nearly the same government in 1991; only the Minister of Agriculture had changed, and the political decision had been transferred to a higher political level (Prime Minister) in order to avoid conflicts between the Ministry of Agriculture and the Ministry of Research. Because the offer had increased political legitimacy, the once-rejected candidate could now afford to accept it without losing face.

Finally, the important interface role of the INRA management was still evident after the definitive nomination of the present president. In fact, while the president can theoretically be an important element within the framework of coping strategies of INRA, he can only be successful if he also has an effective strategy in dealing with the scientific communities. To consolidate its position, INRA must have its own research policy and its president must rally sufficient support from internal actors such as laboratories and scientific communities. In this context, a first step perhaps was the president's demand that the scientific communities of different sectors should each prepare a document about the future scientific development in their respective sector (agriculture, food and agricultural industry, environment). This interaction alone could help consolidate the position of the INRA management (within the institution and in relation to state authorities). Perhaps a strong position of management, which makes the double role described above possible for a longer period, could discharge the scientific communities from isolated defensive coping strategies, making them more open for intensive mutual interactions and for potential internal and external cooperation.

19 According to my interviews.

As seen above, the role of the leadership of research organizations such as INRA appears to be particularly important in the centralized state-oriented system. The leadership can play a decisive role within the framework of coping strategies of the big public research institutions. Compared to the German system, where the federal structures favor a certain continuity in the public financing of research institutions and thus a reduction of uncertainty for research actors, the French system is much more dependent on the interaction between the leadership of research organizations and the respective state authorities. Since political interferences are easier and since radical changes in the public resources of research actors are more likely in the centralized state-oriented system, having an internal mechanism to cope with this politically induced uncertainty is a successful strategy of the management of a research organization.

4 The French Pattern: A Combination of Real and Mock Trouble?

Analyzing the state–science relations for the French agricultural research sector, we found three main sources of trouble which are related to the specific political structures in France. First, it seems to be typical for the French system that political changes may cause important discontinuities in the public financing of research organizations. In this respect, every change of government creates uncertainty for publicly financed research institutions. Unpredictable and erratic decisions concerning the allocation of public funds may lead to serious troubles for these research actors. Secondly, the centralization of political competences also facilitates arbitrary attempts at political guidance by different political actors. As seen above, the close relation of the agricultural research institutes to the political power centers makes such political intervention probable. Thirdly, the great influence of the political authorities on the choice of the leadership of research organizations may be in some cases an important source of trouble. The appointments at the top of these corporate actors are often seen as a decisive act which ensures a certain control over them.

With regard to the three kinds of trouble described, several strategies of coping are conceivable. A research actor can try to remove the source of existing trouble: he can get the political actor to revoke his original decision or provide adequate compensation for the induced trouble. In reality, trouble is rarely coped with in this way. In our study we find only few examples of this type, one being INRA's partial success when it reacted to the cutbacks called for by the Chirac government in 1986: the management played a crucial role in representing INRA's interests and in organizing and guiding the protest against the government. In contrast to this, *adaptive* coping seems to be the more "normal" type of action in situations of trouble. This presumes that research actors will often find it difficult to do away with the trouble altogether. Trouble then becomes an "irreversible loss" and coping a "response to irreversible loss."[20] In this context, we can distinguish primarily three kinds of adaptive coping in our case study: 1) individualistic coping by individuals or research groups, 2) collective adaptation and 3) passive coping. The first type of coping is particularly interesting because it represents not only an adaptive form of coping but equally can turn into the more rare form described above when individual researchers or research groups succeed in compensating for the politically induced disturbances individually. Thus, these researchers enjoy, in fact, a relatively high degree of research autonomy and are rather independent from the political environment. Individualistic coping by persons or research groups means, in general, that these actors try to get a certain amount of external resources in order to reduce their dependency on public funds. But this strategy amplifies the inequalities and the contrast between the "good" – i.e. "rich" – and the "poor" or already weakened laboratories. Whereas the former often succeed in compensating politically induced troubles almost completely, the latter do not, but try to cope with the troubles in a rather adaptive manner - in most cases in vain. In accordance with the adage that capital attracts capital, researchers or research groups already possessing high amounts of "scientific," "economic" or "social capital" (Bourdieu 1981b; 1983) get external money for basic research projects more easily than their colleagues with less "capital."[21] The capacity

20 Comment by Fritz W. Scharpf during the final discussion of the workshop on "Coping with Trouble," 5 November 1992.

21 An interesting aspect of the field theory of Bourdieu is that the different kinds of "capital" are convertible. For example, social capital can be converted in economic capital, economic capital can be converted into social capital, "scientific capital" (Bourdieu 1981b) can be converted into economic capital or into social capital and so on (see

of research actors to cope with trouble thus depends on their position in the research system. Merton has called this phenomenon the Matthew Effect in Science (Merton 1968), which designates the mechanism amplifying existing inequalities between research actors.

The second form of adaptive coping, which we call "collective adaptation," is related to the capacity of research organizations to realize an internal redistribution of resources in order to limit the negative consequences of financial cutbacks. For example, after the cutbacks in 1986, the INRA leadership decided to selectively reduce a variety of expenditures and, thus succeeded in reducing the disturbances for INRA laboratories. The success of this strategy was, once again, based ultimately on the strong position of the INRA management, who watched over the unity of the institution.

The third form, passive coping, finally appeared in an extraordinary situation precisely when the INRA leadership could not assume its function as a mediator between the political and the research systems. As a result of the political struggles concerning the appointment of a new INRA leader, many INRA research groups isolated themselves considerably from the outside world, which meant that external troubles became less and less relevant for them. In this case, the different actors inside of INRA were not able to act collectively or did not see that, in the long term, this very individualistic kind of coping must harm INRA's interests as a corporate actor.

However, in sum, it seems that the actions of political actors rarely caused really big trouble for INRA. The consequences of potential trouble in general are limited ex ante by adequate preventive strategies of the INRA representatives and by the nature of the bargaining interaction. For example, the negative effects of the cutbacks in 1986 (among others) were limited because the institution had been able to build up reserves in times of slackness and of generous public funding before, which considerably mitigated the immediate effects of *dirigiste* political interventions. On the other hand, under the given institutional conditions in France, the management of big research institutes such as INRA plays a crucial role concerning the settlement of conflicts with political actors. In fact, on this level, the social relations are rather conflictual. This is a logical consequence of the high discrepancy between the apparently strong influence of the state and its de facto small impact on the research

Bourdieu 1983; 1981b: 267-269).

orientations. Organizations such as INRA have a monopoly on certain research activities and therefore are not easy for political actors to control.

Thus, we often have to ask if, in reality, trouble may not be part of the ordinary bargaining relationship. In the French centralized system with apparently considerable opportunities for political guidance by the state, the political actors may be under pressure to demonstrate their capacity to act. But, at the same time, they may be aware of their insufficient capacities to realize their goals. The logic of this behavior indicates the existence of a *double jeu* (Bourdieu 1981a: 8) or two levels of action: a symbolic one and a real one. On the symbolic level political actors try to demonstrate their political will to change things radically. This means that political actors show that they are willing, if necessary, to create serious problems for research organizations in order to realize their political objectives. The political centralism seems to demand such pronounced statements by political actors with regard to a symbolic satisfaction of politics. On this level, political actors can create trouble which, in reality, proves to be only *mock trouble* (dramatization of trouble). Here, coping principally means that the research actor symbolically satisfies political actors. In practice, however, the political actors are not content with only symbolic action and will definitely try to impose their view on the research institutions. It is therefore important for these research institutions to remain in close contact with the political authorities in order to prevent real trouble.

The study of the French agricultural research sector reveals a preponderance of the symbolic dimension of trouble. The intensive relation of the INRA leadership to the political system, for example, explains the success of INRA in preventing real trouble. On the other hand, the prevention of real trouble also may be successful because of the staging of pseudotrouble. Often, politically induced trouble in the French agricultural research sector is only mock trouble and coping activities only represent a kind of pseudocoping. None the less, this kind of coping with trouble serves an important function for the preventive coping of real trouble. Mock trouble, therefore, is a staged spectacle for primarily symbolic uses in politics (Edelman 1967) and is represented as trouble only for the uninitiated spectators outside the political arena. The construction and resolution of trouble take on the character of a prearranged affair. In the terms used by Goffman (1959), the actors play on a "front stage," where they present themselves in front of the public, as well as on a "back stage," where things really happen. Both dimensions are important interrelated elements of a pattern of mock trouble, though they seem to be independent from each other.

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Interference between Scientists and Research Policy in a French Research Institution: The Case of the CNRS

Christine Musselin and Catherine Vilkas

Introduction

Generally, the relationships between the state and the research community are viewed as being antagonistic. The former tries to intervene in scientific development and to manage it while the latter attempts to resist state interventionism. Nevertheless, as the recent evolution of the modern state and science clearly shows, the state provides a large share of the research funding and plays an important role in the definition of the scientific issues.

The CNRS *(Centre national de la recherche scientifique)*, France's largest extrauniversity research institution, serves as a good empirical example of this interrelationship. As a state-financed organization whose researchers are public servants, it provides an opportunity to analyze the interactions between the state and the research community, the decision-making processes occurring at different political, administrative and institute levels of the research system, and the ways researchers "cope with trouble" when they feel threatened by their own institution or by the ministry. This analysis will enable us to show that while some of the scientists face decisions by reacting to them, others cope with trouble by intervening in the decision-making process and thereby influencing it. We will examine the interactions between the political actors and academia in France and show that the research community participates to a certain extent in the political discussion of research issues, so that the division between researchers, the administrative authorities and the political sector is not as clear-cut as might be expected.

Our empirical evidence will demonstrate that coping cannot be understood as a mere reaction to unforeseeable actions and that we have to go beyond an antagonistic conception of the relationships between political actors and researchers which views researchers as victims of decisions to which they can only react, either negatively or positively. If this antagonism does exist at certain times, it should not hide the fact that the researchers and their research centers also act *with* the political actors as well, and that all of them may not view the same decision with the same prism.

Furthermore, viewing the relationships between the state and academia as being purely antagonistic also induces de facto an oversimplified picture of the state and the researchers as separate sets of actors, when instead the following questions should be raised: Are the researchers to be considered as an undifferentiated community? Is the state a single actor? We will address these questions in this chapter, attempting to show that the emergent groups of actors may not reflect the a priori gap between the state and the researchers. To provide a better understanding of the close connection between scientific and political matters within the CNRS, our analysis, which is based on the examination of two types of trouble, will highlight the different processes trouble can set in motion.

The first type of trouble involves the decisions concerning the allocation of resources and the interactions between the administrative agencies, the scientific sections and the researchers themselves. In this part, we will describe the characteristics of a specific procedure: the evaluation of the research units. We will focus on what happens when a new unit can only be created if an existing one is dissolved or reduced in size. From a formal point of view, this process is from the bottom up. The procedure foresees that experts consulted at various levels give advice about what should be done, whereupon the directorate of the CNRS may take a decision.

The second type of trouble involves a more political change and its effect on the functioning of the CNRS: the reform of the sections and, particularly, the efforts to reorganize them. We will describe the reactions of the researchers and the members of the former sections, and their embeddedness in the decision structure of the CNRS. This is a decision from the top down prepared by the directorate after consultation with the ministry.

In examining these two types of trouble, we will not only focus on the reactions of the researchers to the decisions that have been made, but also describe how the scientists *intervened* in the decision process and *influenced* the content of the decisions. But before arguing this point, let us describe some characteristics of the French research system and of the CNRS itself.

1 The Structure of the French Research System

In most countries, research activity is mainly managed in and by the universities. This is not the case in France, where, for historical reasons, a large research system has developed outside the universities which is autonomous from the university sector (Blancpain 1974; Picard 1990; for an English-language overview of the French research system, see Battaglini et al. 1992). A variety of large research institutions with general scientific purposes, such as the CNRS,[1] or with specific domains, such as INRA (National Research Institute for Agricultural Science) and INSERM (National Research Institute for Medical Sciences), have been created over time in order to compensate for perceived "deficiencies" of the universities.

Hence, these institutions have developed their own staff of researchers who pursue their careers in their own national institutes. Since 1984, almost all the CNRS researchers have been public servants. They are divided into two main groups: the *chargés de recherche* (who can be compared to the *maîtres de conférence*[2]) and the *directeurs de recherches* (who can be compared to the professors).

There have been many attempts to reinforce the links between the researchers outside the universities and the faculty members of the universities. One of them is the so-called *association* (Prost 1990), which means that teams of faculty members whose research activities are recognized as being scientifically important by scientific experts[3] may be considered "associated" *(associées)* with one of the national research institutions, for instance the CNRS. Associated laboratories receive funding from the CNRS and, sometimes, staff (either administrative or scientific). By contrast, the research units of the CNRS that are entirely financed by the CNRS and do not belong to the university system are called *unités propres* (in the sense of "its own"). The *association* is a kind of contract that is reviewed every four years. The quality of the team's research is (re)evaluated, and then a decision as to renewal or cancellation is made. Usually the contract is renewed, but recently

1 In 1990, there were 11,135 researchers at the CNRS and 14,245 technical and administrative staff working in seven scientific departments.

2 The equivalent in the German system would be an *akademischer Rat* and in the American system an associate professor.

3 In the CNRS these experts are organized in the "sections" we will describe later.

the policy of the CNRS has changed (due to budgetary constraints), and the practice at present, even if it is not officially stated, is: If a new institute is to be associated, another will have to be disassociated first. In this kind of zero-sum game where one wins what another loses, the stakes are very high. Being associated with the CNRS is a scientific reward of vital importance for the units in the universities with which they can prove their quality to their peers and tap other sources of funding.

1.1 The Structure of the CNRS

The CNRS, like many other research institutions, is connected with a ministry; however, it is not usually connected to the one responsible for the universities.[4] The CNRS executive staff has to negotiate its budget and human resources with the ministry. The head of the CNRS is nominated by the council of ministers based on a proposal from the minister for research. Traditionally held by a physicist (Pestre 1984), the post is now occupied for the first time by a biologist, François Kourilsky. He is assisted by a scientific council of eleven elected members.[5]

The administrative structure of the CNRS is made up of functional departments, the central administration and *délégations régionales* (regional agencies). At present, it has seven scientific departments[6] to which the various research units belong. Each department has a director who was previously a faculty member or a researcher in one of the disciplines of the department; he is assisted by administrative and technical staff as well as faculty members

4 The formal structure changes with each new government! But, generally, universities and the research institutions are connected with different ministries. In the ministry dealing with the universities, there is a department for university research (as opposed to extrauniversity research). But, between 1986 and 1988, and once again as of April 1993, higher education and research have been the responsibility of a single minister, M. Fillon.

5 The scientific council is made up of: 3 *directeurs de recherche* from the CNRS, 2 *directeurs de recherche* from another research institution or university professors, 2 *chargés de recherche* from the CNRS, 2 *chargés de recherche* from another research institution or *maîtres de conférence* from a university, and 2 administrative agents.

6 "... [N]uclear and particle-based physics; mathematics and pure physics; physical sciences for engineers; chemistry; land, oceans, atmosphere, space; life-related sciences; science of man and society" (Sevin 1992: 33).

or researchers with whom he has chosen to work. It is easy to understand that this structure is highly political in at least two ways. First, the department has to define and (try to) implement a scientific policy and, second, governmental changes often (but not systematically) induce the replacement of some department heads.

In addition to the administrative and scientific structures, there is a *Comité national de la recherche scientifique* composed of 40 sections, each of which is responsible for a discipline or a subdiscipline[7] of the departments. Thus, each department has its own diversity of sections.[8] These sections are internal evaluation and advisory bodies composed of elected and appointed faculty members or researchers representing different categories.[9] Each section is responsible for the scientific evaluation of the CNRS researchers (recruitment and promotions), of the CNRS-associated research units (*association, reassociation* and *disassociation*) and of the units completely attached to the CNRS for its discipline in France. The sections meet three times a year. In the fall, they evaluate the units and individual researchers; in early spring, they give advice on promotions; later in the spring, they make proposals for appointments (either for first appointments or for appointing *chargés* to *directeur* positions). The recommendations provided by the sections, which are not binding, are then discussed in the so-called *conseil de département* (department committee), which is made up of elected representatives of the sections and the administration, and appointed representatives from the ministry. The final decision is made by the department head.

7 There are also sections that are multidisciplinary and are connected to several departments, but we will not go into detail about them here.

8 In the department for social sciences, for instance, there are 9 sections: Ancient and medieval worlds; Emergence of the modern world; Representations – language – communication; Philosophical thoughts – the science of the texts – artistic, scientific and technical creation; Sociology – norms and regulations; Economy and society; Unity of mankind and diversity of cultures; Spaces, territories and societies; Policy – power – organization.

9 The 14 elected members include: 3 *directeurs de recherche* from the CNRS, 3 *directeurs de recherche* from another research institution or university professors, 3 *chargés de recherche* from the CNRS, 2 *chargés de recherche* from another research institution or *maîtres de conférence* from the university, 3 administrative agents. There are also 7 members nominated by the minister on proposal of the director of the CNRS (in fact, the staff of each department proposes names to the director).

Thus, it can be stated that the management of the CNRS is rather centralized, whereby the sections oversee the scientific management of the researchers, and the departments are responsible for allocating funds to the administrative positions in the units.[10]

Further, it can also be stated that the CNRS is a special kind of professional bureaucracy (Mintzberg 1979), since the separation between the professional categories of scientific personnel and the administration is not clear. Research activities and careers are managed primarily by academics and scientists, i.e. peers; some of the executive positions in the CNRS are also held by academics rather than by administrators. This is especially true of the department heads, for example, who have a rather important role (they do not necessarily follow the advice of the sections), even if it cannot be compared with the hierarchical authority of a business executive. They are under greater pressure to prove their legitimacy (and everybody knows this is very precarious in academic affairs) in order to find a modus vivendi with the units (which may have many outside resources) and the sections (which may, through their advice, try to develop a policy different from the one chosen by the department).

In the following two sections, we will illustrate some types of trouble affecting these different levels (the department, the sections, the research units and the individual researchers), showing that the interplay between the levels is much more complicated than the formal description would lead us to believe. It will become clear that the emergence of trouble and the efforts to cope with it are far from being a linear "top-down-top" process.

2 Evaluation as a Stimulus for Action

The regular evaluation of the CNRS units (examination every two years, new contract every four years) is a source of ordinary trouble for the laboratories, in the sense that it is scheduled and, thus, foreseeable. Most of the time it

10 To be fair, we must say that there have been attempts to decentralize the CNRS and to give more leeway to the *délégations régionales*. In the provinces, they have developed some autonomy and are an important partner for the research units. But this is sometimes accompanied by conflicts between the *délégation régionale* and the department.

is a routine procedure, especially for the units completely attached to the CNRS, but also for most of the "associated" units. It can turn into severe trouble, however, if the lab is disassociated. The CNRS no longer provides funding, and the loss of the prestigious "CNRS label" makes it hard for the lab to obtain funding from other sources; the CNRS employees (researchers and staff) working in the lab have to look for another lab; the academics are no longer affiliated with the CNRS, so it is much more difficult for them to conduct high-quality, expensive research. This is why researchers in basic science will desperately try to remain in the institution, and scientists who do not belong to the CNRS will do their best to be integrated. While being associated is a great opportunity, the CNRS evaluation process is also a hardship for the individual laboratory, even if it is a routine decision for the evaluation commission.

For a long while, the policy of the CNRS and the ministry to which it reports was oriented toward supporting associated labs. In recent years, especially since 1986,[11] the CNRS has had to limit access to association. The sections are asked to propose which units should be disassociated, associated or reassociated; the scientific department then makes the final decision. The scientific department also asks the sections to rank the units.[12] Being at the end of the list, of course, endangers the survival of a unit. As most of the disassociations imply a redistribution of means and personnel, the threatened units will be given a delay either to disaggregate or find a way to recover, which is called the "SDI" process.[13]

Another recent development of the CNRS policy (since about 1990) is to promote the units in the provinces in order to re-equilibrate the distribution

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11 In 1986, during the first *cohabitation* government (*cohabitation* refers to the forced cooperation between socialist President François Mitterrand and the government led by conservative Prime Minister Jacques Chirac), the existence of the CNRS itself was threatened. Some politicians and some scientists proposed to dissolve it and to integrate its members in the university as faculty or administrative staff. At this time the research sector was also subjected to budgetary cuts.

12 Some sections are very reluctant to do this, however.

13 The *Structure diverse d'intervention* was a statute referring to an intermediate period during which a unit's CNRS funding is half the usual amount. This structure applies when a new team is working toward being fully associated or when a group is going to be closed down. This procedure is now referred to as the UER: unit in restructuralization.

of research institutions in France, which tend to be concentrated around Paris. Of course, this is seen as a threat by many "Parisians," but as an opportunity by the provinces.

In this section, we will try to understand how laboratories – and scientists – manage to cope with the trouble posed by evaluation. Using two short case studies, we will show that scientists have many capacities of action or reaction when they are faced with trouble. The first is the story of a laboratory which underwent a disassociation process, but happily succeeded in getting reassociated. The second describes a case of *mobilité*, i.e. the creation of a new lab in a provincial town by a Parisian team, which included fusion with a local unit. We will conclude with a comparison of these two cases of managing trouble.

2.1 A Case of Disassociation Followed by Reassociation

Once upon a time, there was a small university lab in a provincial town, with one professor and two *maîtres de conférence* working in molecular spectroscopy, a branch of physics. Their research results attracted the attention of the CNRS, and in 1978, the lab was associated with the Department of Physical Sciences.

2.1.1 A Progressive Process toward Disassociation

But a few years later, the lab was criticized mostly because it was too small according to the CNRS criteria. When its association with the CNRS was renewed in 1982, some physicists and chemists joined the team. Their common aim was to work in a CNRS unit in order to go on with basic research under good conditions. The physicists came from an academic lab which had specialized in more applied research of regional interest; the chemists came from a lab that had been disassociated from the CNRS, so they had to find another CNRS lab dealing with their subject.

The next step shows, however, that the lab's position remained unsteady in the eyes of its evaluators. In 1986, it was not reassociated and had to submit to restructuralization according to the SDI statute. This decision was made by the scientific director against the displayed will of the commission. Some of the critical comments made by evaluators of the commission in their assessment of the unit's research activity had, in fact, been used by the scientific director to justify his decision, but the commission later claimed that "[it] did not want to kill [the unit]." An analysis of the actors' interactions reveals a combination of factors that may lead to a disassociation. Let us examine them briefly.

First – and this should not be underestimated –, the disassociation happened when the CNRS was threatened by the new *cohabitation* government. This political situation, translated into the reduction of the associated units, is one of the arguments put forward by those interviewed to explain the disassociation. They stated that it could not have happened earlier, and that the new context meant that there was pressure to decide in favor of disassociation. Nevertheless, this political context is not sufficient to explain the decision made; rather, a coincidence of more or less manageable constraints can be discerned.

At that time, the lab was confronted with a painful succession of directors for an unusual reason. The problem was not due to internal conflicts in the lab, but rather to the CNRS, which required the candidate to be a professor. The designated successor, who had been a student of the director and was now his closest collaborator, was accepted by everyone in the lab, but he was still a *maître de conférence.* There were no other professors in the lab at that time; every *maître de conférence* was trying to get a position as a professor.

The field of research was criticized, too. Classical molecular spectroscopy had a good reputation until the 1980s, but data on molecular spectra had accumulated with almost no practical applications. This scientific activity was then seen as a kind of routine, and the field was no longer looked upon favorably by the evaluation commission. Moreover, neither the commission nor the scientific director considered the productivity of the lab to be high enough. This can be attributed to problems with material and human resources without casting doubt upon the scientific quality of the team itself. A shortage of expensive but necessary equipment (such as lasers) slowed down the activity, and the university was lacking computers for a long time. Hardly any funding could be obtained from the regional authorities, who were interested in research activities other than physics (especially those related to the food industry). Furthermore, there were only two full-time CNRS research workers on the team. Finally, the lack of DEAs[14] in physics at the local university

14 The DEA is a diploma between the *Magister* and the PhD.

until 1991 hurt the lab; these young people would have been an important asset both at the time and later, when they could be appointed as full-fledged scientists. Thus, the scientific environment represented a severe constraint that contributed to the temporary disassociation of the lab.

2.1.2 How to Cope with the Threat of Disassociation

The lab members actually succeeded in getting fully reintegrated into the CNRS in 1991. How did they manage to cope with the trouble of disassociation? Here again, many factors interfered. Some of them are the direct results of the strategies employed by members of the lab, others have to be considered good luck.

Following the recommendations of the evaluation commission, the scientists directed their research activity toward more applied problems related to the environment and the atmosphere. Interestingly, one of the main reasons they were able to do this so readily was because they had previously collected a great deal of data and spectra on atmospheric molecules. These molecules, such as nitrogen dioxide and ozone, had become objects of great scientific interest because of pollution problems, so the scientists seized the opportunity to capitalize on the increased value of their results. One could say that the first director, who had initiated the research topics more than fifteen years earlier, had a great sense of intuition which bore fruit (or was he simply lucky?). In addition, some professors from American universities who were extremely interested in the data from the small French lab were able to provide funding for costly equipment for the observatory.

During the SDI period, the scientists went to great efforts to get their work known abroad, not only by means of publications, but by developing a more assertive communications policy which entailed attending international conferences on atmospheric problems. Being invited to present research results at a conference (which is different from simply exhibiting a poster) is considered a criterium of high international recognition by a scientist's peers. And the reputation which is gained by the individual scientist also benefits the laboratory to which he belongs.

Some of the physicists joined national expert committees responsible for allocating resources in the framework of huge research programs about atmospheric topics; the scientist who had worked on ozone for a long time was particularly in demand. This was a way of penetrating into the scientific establishment or elite that manages the community working in these fields. The lab also found an institutional supporter in the Department of Land, Oceans, Atmosphere and Space of CNRS. The community of atmospheric scientists there was very interested in the lab's results and sent a letter to the Physical Sciences department supporting the group when disassociation was being considered.

But it was most important to persuade their own department that their research topic and their results deserved to be supported. The scientists developed, or reactivated, relationships with some important laboratories of their section, especially with influential members of the discipline belonging to the commission. They had to find allies interested in their results and capable of defending them in front of the other evaluators – and, later, in front of the scientific directors. Expertise on ozone and atmospheric problems appeared to be a vital asset once again when the commission members had to decide which lab they would recommend among those competing for reassociation. In their evaluation activity, the members of the commission could not ignore scientific orientations, especially the ones valued by the scientific directors (Vilkas 1993). As the commission only had the capacity to advise, it knew that if it wanted to be heard, it had to find a compromise between its own preferences and those of the scientific directors.

Finally, from an internal point of view, solidarity developed among the lab members when their institute was endangered. None of the scientists left the lab,[15] and they all agreed to accept as their director the first among them to succeed in obtaining a professorship in 1988, even if he was not the logical heir. They could not afford the luxury of a conflict in front of the CNRS. The positive result of this threatening situation was that it enforced cooperative management of the lab.

The scientists' strategy, including taking full advantage of the "ozone opportunity," proved to be successful: The lab finally got reassociated. The cooperation, the activation of the "natural" network (i.e. the members of the commission) and the mobilization of an extended network toward "unusual"

15 Of course, this solidarity was a "choice" under constraints stemming from the local environment and the lack of exit opportunities for the scientists, who would either have to cooperate and try to find a solution, or return to their previous faculty situation (i.e. scarce research resources).

partners inside the CNRS institution itself (i.e. the Department of Land, Oceans, Atmosphere and Space) each played important roles in the process.

2.2 Politics and Networking: Components in the Adventure of Relocation

Our second example of how an institute dealt with trouble demonstrates these relationships even more explicitly, showing the very narrow and transversal links that exist between scientists and administrative or political actors. This case study involves the move of a physics team from one of the most prestigious laboratories in Paris to a provincial university far less developed in the discipline, located in a town we will call Towin. It also involves a research team in Towin whose lab faced the trouble of not being recognized by the CNRS. We will show how the anticipation of potential trouble enabled both labs to take advantage of a good opportunity.

2.2.1 Two Ways of Describing a Political Decision

This case can be described in two different ways. The first could stress the political intentions of the French government as expressed by the policies of the National Education Ministry, in particular by those of the Directorate for Research and Doctoral Studies (DRED).[16] Like the Ministry for Research, the Education Ministry has a policy of strengthening higher education and research activities in the provinces. Campaigns are launched, for example, to build up certain departments at certain universities: in our case, to reinforce certain fields of the physical sciences at a rather large provincial university. The CNRS cooperated with the university; each committed itself to allocate an agreed amount of positions and funding for a period of three years.

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16 As we explained above, the research and the university sectors are managed separately, even when they are assigned to the same ministry. Since there are research activities at the university that are not supported by research institutions, there is a directorate for university research (and doctoral studies) among the central authorities in charge of the university sector: the DRED. As a matter of fact, the cooperation between the directorate responsible for the research institutions (DGRT) and the DRED has not always been very strong.

But this simple presentation could lead us to think that political and institutional hierarchies have the sole prerogative to take initiatives and make decisions, and that the scientists can only submit or react to them after the fact. This, however, is far from reality.

In order to show just how complicated the real situation was, we will tell the story in quite another way. Some scientists did indeed play a very important role in the decision-making process. In the very beginning, the ministry's aim was "simply" to avoid wasting its efforts; in order to ensure that the campaign would be impressive, one discipline was to be concentrated upon. Within this policy context, it did not matter so much *which* discipline would be chosen; at this point, there was no instruction to put priority on supporting the physical sciences. Then, personal relationships intervened. The DRED (and the National Education Ministry) have a mix of administrative and academic staff[17] similar to that of the CNRS, so that the distinction between the political, administrative and academic levels is not that clear. Hence, certain researchers at universities and institutes have a fair amount of access to information that can be valuable for their institution's survival. This was certainly true in our case, where the sifting down of information from the central administration to certain members of the scientific community was closely connected with the social network formed by the former students of a well-known *Grande Ecole*.[18]

A famous scientist working in Towin, the founder and ex-director of a chemistry laboratory who had been coopted for a short time by the physics commission of the CNRS,[19] was contacted by a leader from the DRED – both had attended the same *Grande Ecole*. The leader from the DRED said that funding and positions were going to become available and invited the famous scientist to think about possible weak points in his discipline that might benefit from being reinforced. Of course, it was not hard for him to

17 For instance, the Director of the DRED comes from academia. He is responsible not only for many administrative sections, but also seven scientific directorates (which do not correspond exactly to the seven of the CNRS) made up of academic experts providing advice on research projects.

18 A *Grande Ecole* is a highly selective college that trains elite students in science, the humanities, public administration or business.

19 A member of the commission had been dismissed; when this occurs, rather than conducting a new election, the commission has to coopt another member.

think of some! Soon after that, the DRED officially announced its decision to consolidate physics.

2.2.2 *Looking for Units to Be Reinforced*

During the spring of 1990, an ad hoc expert committee was created, composed mainly of Parisian physicists, but also including the "famous physico-chemist" mentioned above. The expert committee was responsible for identifying needs and defining priorities for the allocation of supplementary resources. The role of these scientists must not be overlooked when we try to assess how this science policy developed. The report submitted to the DRED stressed two essential gaps: the lack of theoreticians in a unit for solid-state physics, and the fact that experimental molecular physics were not represented in the physico-chemist's lab. These conclusions, arrived at by a small fraction of the scientific community, formed the basis of the campaign's orientation.

2.2.3 *Looking for an Opportunity to Leave*

The campaign could have been conducted by simply reinforcing the existing physics groups. But the physico-chemist suggested that some Parisian scientists he had once collaborated with (in a temporary research group) – who happened to have attended the same *Grande Ecole* – should leave Paris and come to Towin. This solution was accepted by all the actors concerned. From the beginning, the DRED, engaged in a prestigious campaign, wanted Towin University to welcome scientists who would lend a certain aura to the community. The Region of Towin also supported this idea, seeing it as an opportunity to enhance its image – scientists from one of the most famous Parisian labs imparted prestige. This suggestion corresponded, too, with CNRS's interest in getting started with the decentralization away from the Paris area announced by the General Director in 1989.[20] The relocation of the particular group that had been recommended would serve as a very good example of the CNRS efforts, because it would be affecting a section in which Paris predominated. The interdisciplinary character of the operation at the interface of physics and chemistry also spoke in its favor. In addition, the creation of

20 This plan was followed by the more rigorous "2/3-1/3" rule prescribing two thirds of the new appointments in the provinces and one third in the Parisian district.

numerous positions at Towin University since 1988 in response to an increase in student enrollment opened up career opportunities for the newcomers.

Thus, the institutional actors were very much in agreement with the project. Now, what factors might cause a team leader and some of his researchers to decide to leave their present laboratory, especially when it is one of the most prestigious of this Parisian section? First, there is certainly a pioneer spirit coupled with open-mindedness. The creation of a new group, which is a real adventure, like the creation of a firm, can be very attractive for the scientist charmed by the novelty and even the risk.

But relocating can also be a means for gaining more autonomy. In our case, the Parisian team felt it was not receiving adequate recognition within the Paris lab. Far from being a homogeneous actor, a laboratory is made of sub-units (groups, teams, individuals) working on different topics and often competing for the allocation of resources (Latour/ Woolgar 1988; Shinn 1988); relationships between these different components will be marked by a balance of power. For the team, this feeling that their true worth was not being recognized was underlined by a number of facts and attitudes.[21] But it is not important to decide whether the team leader was really undervalued or not. He and his group *perceived* that they were and, hence, found some advantages in the opportunity that opened up for them. The fact that the team members had previously successfully collaborated with the group led by the famous physico-chemist and knew that molecular physics had a better reputation in Towin than in their Parisian unit also played an important role in their decision to move.

Another point that must be stressed is that, in this case, the departure of part of the lab is regarded as a "natural" phenomenon in the Parisian lab, which has a high concentration of scientists who went to the same *Grande Ecole*. It is the usual way to manage the internal competition and to provide new career opportunities. The lab's directors take a personal interest in the work of their scientists and make efforts to get positions outside the lab for those who want to specialize in other fields. Two advantages of this swarming strategy are that it partially eases the internal competition among scientists

21 During a scientific committee meeting of the lab, the team leader was advised to change his topic, despite the fact that a Nobel laureate had told him a few days before at a conference that his work was very interesting. Moreover, he had difficulties attracting DEA and PhD students.

applying for promotions and leads to a large network which the lab can tap when necessary. A young *chargé de recherche*, who otherwise would have had to wait many years before getting promoted to a *directeur de recherche* position, took advantage of this operation and successfully applied for a position as a professor. Swarming was also seen as an individual solution by some of the team members who had remained in Paris at the beginning. They soon realized that the dissolution of the former group meant that the probability of their being promoted within the CNRS was weak, and succeeded in getting professorships at new universities near Paris.

Three technical and administrative assistants joined the group. Beyond these human resources, the operation brought very important funding to the lab: 7 to 8 millions francs spread over three years, equally shared among the CNRS, the university and the region. Indeed, the physics department gave 3 millions francs to enable one of the young scientists to start a new experiment. The last resource required for the relocation was space, which was obtained thanks to the watchfulness of the local physico-chemist. One man's joy being another man's sorrow, a disassociated lab in another discipline provided the room for the newcomers.

2.2.4 The Physics Commission Was Passed Over

The creation of a reinforced unit in Towin and the allocation of positions to it was directly affected by the kind of decisions made by the members of the physics commission. But it seems as if the commission was barely included in the decision-making process. By the time the session was held, many consultations and negotiations had already taken place which included all the parties involved (research workers, lab directors, "experts," scientific department, university, individual commission members, region, etc.); the final choice remained in the hands of the scientific director. The commission was compelled to accede to the operation that had been launched, and was only asked to choose candidates for positions already allocated in the new lab.

The role of the scientific director was very important and decisive in this case. When the Parisian team finally moved to Towin, the scientific director initiated a regrouping with a small associated physics unit located on the campus. The small unit, aware of its weak position in relation to the CNRS, accepted the merger with a star group and the future loss of the unit directorship in order to avert the threat of disassociation.

2.3. How to Face Evaluation

These two case studies may appear to be very different, but they allow us to compare two different situations occurring within one political context. The science policy, with its priorities and budget constraints, is contingent. Whether certain scientists play an active part in its definition – as in the relocation case – or suffer because of a political constraint regarding the number of associated units – as in the case of the first lab's disassociation in 1986 –, the evaluation of the labs and the allocation of resources strongly depend on the shifting frame set by science policy. But this political context can play two roles simultaneously. It may provide scientists with opportunities which they may or may not successfully grasp (they may, in fact, have even contributed to creating the opportunities in the first place), or it may be the source of constraints with which research workers cope more or less efficiently.

For both of the cases studied, the political orientations were the same: reduction of the number of associated units and, then, efforts to encourage decentralization away from Paris. The implementation of these orientations is left to the scientific directors and the commissions, who are not bureaucratic administrators, but academics: the directors are charged with managing the research activities, and the commissions with managing their evaluation. The difference between them is that the scientific directors are supposed to agree with the general orientations of the CNRS, while the commission members are expected to be independent. Nevertheless, the commission knows that the final decision will be made by the scientific director and cannot ignore this constraint.[22]

2.3.1 Perceptions and Anticipation

In both cases, we have shown how the researchers are able to directly or indirectly influence the scientific director or the commissions. But before they can do this, a cognitive aspect plays a significant role: their ability to assess the value of their work, which is felt in a comparative perspective, and the

22 A commission can, for instance, decide not to respect the "2/3-1/3" rule. But it would be risking that the scientific director might decide who is to be appointed and, more importantly, where the new appointments will be; the commission will usually (but not always) prefer to avoid this situation.

way they perceive constraints, resources and threats. For instance, the Parisian team members felt they deserved higher recognition from their lab and from their commission than they had been accorded in their lab of origin, but they were not really endangered. The directors of the small spectroscopy lab were aware that the research they conducted was not at the top level of competition, but it still had some significance and deserved to be supported. And each lab's capacity to cope with these different kinds of trouble depends on its members' ability to anticipate developments, to grasp opportunities and adapt to evaluation criteria. Therefore, scientists will develop strategies at both an individual and a collective level. Borrowing from economic terminology, they talk about the "available gap to fit into" or "choosing the most competitive topics and coordinating [their efforts] so as to win the war."

Hirschman's "exit" strategy (1970), describing how a peripheral group moves with the anticipation of becoming more significant in the next place because of its past and new resources, corresponds with our comparison between the two scientific environments and our portrayal of the anticipated and actual benefits of relocation. The "loyalty" strategy is used by the lab members in the first case to cope with more serious trouble when the CNRS association is at stake. The scientists, already in a peripheral position, are in danger of being pushed out of the CNRS into very bad research conditions. When the scientific circumstances suddenly become favorable, they manage to fit the evaluation criteria of the CNRS, make themselves known abroad and at home, and set up a larger network.

This shows that researchers do not wait until decisions are irreversible in order to act; most of the time, they are able to "hear" signals, to seize opportunities. But, of course, to perceive that trouble may happen and to anticipate what could happen is not enough; action is also necessary. We will now analyze the levels at which it can occur.

2.3.2 Playing with Scientific Topics

The first lesson we can find in both our cases is that because the interest on scientific topics varies, it is important to be able to play with them in order to cope with trouble. It is generally agreed that scientific quality is difficult to define, and that measuring scientific value is a difficult exercise because of the uncertainty prevailing in basic research (Roberts 1989; Menger 1989). Criteria such as publications and conference papers, reflecting the judgment of the international community and serving as a benchmark for peer review, contribute to reducing this uncertainty (Merton/ Zuckerman 1971). But evaluation remains contingent. Research topics are not indifferent to recognition, and the position of a topic on the scientific stage has a great impact on evaluation and, consequently, on the allocation of resources. The first case, for instance, illustrates how scientific and social demand can affect the survival of a laboratory. Research formerly considered routine may turn into a hot topic with unanticipated applications because of a new need. Suddenly, it is easier to publish results and be invited to conferences on a subject that is fashionable; more funding and positions are allocated by government sources. The strategic importance of the scientific product is mainly defined by its environment (itself consisting of many other actors that will not be analyzed here), although the labs may also be able to create demand (Lemaine et al. 1972). While lab members do participate in the creation of scientific circumstances, this environment must at the same time be seen as a component of their context and, hence, as a source of uncertainty to which they are capable of adapting.

At a lower level, the position of a certain topic within the section has a great impact on the evaluation. Obviously linked to the scientific circumstances as well, it is part of the balance of power that exists at a point in time between the different themes. When a topic is judged as becoming outdated, it has less chance of catching the interest of the evaluators, who will not tend to be working on this subject or using this technique anymore. Similarly, a theme which is far away from the "center of gravity" of the section will seldom be warmly supported. Therefore, recognition is strongly connected with the context in which scientists move about (Merton 1968). In order to cope with trouble, researchers have to try to adapt to this context or change it.

2.3.3 Networking

Researchers attempt to adapt to their situation by networking. Actually, networks are always present in laboratory life; there is a constant give and take involving scientific materials, equipment, papers, collaborators inside and outside the lab, administrative people, firms, etc. (Latour/ Woolgar 1988; Callon 1988). But not every lab has the same access to the same network, and not every pole within a network is equally important, so that each relation is not equally activated. Setting up networks and consolidating them is an important part of scientists' activities enabling them to market their products.

In the case of the disassociation, scientists present their data on ozone and then set out to find allies. They reactivate old relationships and manage to persuade some influential scientists in their section that the lab's research matches their own interests. And they extend their scientific network to another community extremely interested in their results, which supports them when the time comes to deal with the scientific director. In the relocation case, however, the resource relationships are already available because of the lab's institutional status and the collaboration it has previously taken part in. Thanks to its direct connections with key administrative and political figures, this network even bypasses the commission level.

This shows how closely intertwined science and politics are in France, and how important the scientists' capacity to take initiative is, provided they have made a name for themselves and can use their connections to rally various kinds of resources. There is a bottom-up element in this decision-making process, entailing many interpersonal relations and informal transactions between scientists and their hierarchies (who come from the scientific world even if they are involved for a time in a management career).

2.3.4 Collective Action

A final, but important aspect of the ability to cope is the ability of the group of threatened researchers to act collectively. Cooperation prevailed inside the lab that faced the threat of disassociation. This decision to cooperate, initiated by the scientific director, obviously depended on the scientists' perception of their situation and on their preferences. We can note that "negative" factors may bring about "positive" actions. The risk of losing CNRS association motivated group members to integrate each other's interests and to cooperate, strengthening their ability to cope with trouble – both within the small lab and between the units in Paris and Towin.

3 Facing the Reform of the *Comité national de la recherche scientifique*

We will now examine a process which – contrary to the evaluation procedure – occurs at an intermediate level (between the researchers and the directorate of the CNRS), initiated from the very top (some say from the minister himself). It ended with publication of a decree in the *Journal Officiel*[23] of 19 February 1991, listing 40 sections altogether instead of the former 49. This list had been the subject of very big trouble inside the CNRS during the winter of 1990-1991 because it implied a redifinition of the boundaries between the disciplines. Since we cannot go into great detail here, we will briefly present the most important phases of the process. We will especially stress that, from the beginning, the process was a consultative one and, secondly, that the strategies of the researchers and commissions differed greatly depending on whether they saw the reform as an opportunity or a threat. First, however, we have to describe the context in which the reform occurred and why the directorate initiated it.

3.1 The Aim of the Reform from the Point of View of the Executive Director

For the Executive Director of the CNRS, François Kourilsky, the reform of the National Committee was part of the modernization plan he announced in June 1989,[24] one year after he took office. Drafts of a three-year strategic plan were to be written; the results of the deliberations were to be published in February 1990. According to Kourilsky,[25] there was nothing unusual about remodelling the National Committee, since this often happens when the committee is reelected every four years.

23 The administrative journal in which governmental decrees are published.

24 cf. *Le plan de modernisation du CNRS*, a press conference held by François Kourilsky, Executive Director of CNRS, on 27 June 1989: "The institution, in order to adapt to scientific evolution, has to modify its management structures by bringing the administration closer to the laboratories and better utilizing its human resources."

25 *Le Monde*, 6 March 1991.

Consolidation and interdisciplinarity were the two key words of this modernization. Barriers between disciplines, between the sections of the National Committee or between departments were to be broken down in order to depart from the logic of disciplines and to organize the sciences in "scientific fields," in "systems." Interface topics were to be observed very carefully, and it was now to be possible for one section to have labs reporting to different departments.

3.2 A Proposal, Consultation, and a Decision

3.2.1 The Directorate Prepared a First Draft ...

During the first phase of the process, everything was rather informal. The fact that there would be a reform was known, but the CNRS directorate prepared its first draft on its own. When he announced the plans for modernization at the press conference in 1989, Kourilsky explained that by 1990, a proposal for new department outlines and a revision of the National Committee commissions, including a moderate reduction of the number of sections, would be presented. Adjustments were planned to allow one commission to intervene in several scientific fields and to call upon external experts for the evaluation of specific topics. Contradictory rumors were heard throughout the rest of the year, some predicting an extensive reform, and others anticipating only minor changes.

At the end of October 1990, a first draft of the reorganization was presented to the sections and the unions. Proposing only 32 sections (plus 2 for research management and valorization), it foresaw more than a slight alteration. Some disciplines (such as atomic and molecular physics or plant biology) had disappeared from the headings of the sections; many research themes which had not previously been in the same section were now grouped together in a section for the first time.

The proposed changes were also to affect the scientific departments, decreasing their number from 7 to 6.[26] The names of some of the departments

26 The Department of Nuclear and Particles Physics was to be joined with the former Mathematics and Basic Physics Department in order to create one big Department of Physical and Mathematical Sciences, including the IN2P3 Institute (one of the big insti-

were also to be changed; Physical Engineering Sciences would become Engineering Sciences, for example. But more important were the shifts between Departments. The Chemistry Department was to welcome laboratories coming from the Biology Department in order to regroup research on biologically active molecules. The Life Sciences sector itself would not grow any further and was encouraged to give up positions to the other sectors. Finally, some interdisciplinary sections were created, for instance between physics and chemistry.

3.2.2 ... and Stimulated Consultation within the Scientific Community

A continuing consultation among all the parties concerned began in October 1990 and went on until the end of January 1991. The outcome still seemed to be open, because Kourilsky explained that the number of sections was not immutable and that the creation of complementary sections at the interface between several fields might avoid forming sections with too many scientists. The first draft was presented to the section presidents, to their boards and to the union organizations[27], and then to the Scientific Council on 8 November 1991.

The new draft issued at the end of November was not very different from the previous version. Entitled *The new CNRS: departments and sections of the National Committee*, it began by introducing the major principles of the project, presented the five prongs of the modernization plan and suggested improvements in the functioning of the National Committee such as appealing to external experts, modifying the juries, and introducing rules that would

tutes for nuclear research at the CNRS).

27 Unions had once been very powerful in France. In 1956, some scientists from the CNRS and the Pasteur Institute succeeded in creating a national union for researchers, the SNCS, with the idea of participating in science policy making. It provided a valuable contribution to enhancing the recognition of the profession and working out the researcher's statute in 1959. The weight of the different unions (SNCS, SNESup, SNTRS-CGT, SGEN-CFDT) in the National Committee increased after the events of 1968 because of the new list vote. But they certainly had no monopoly on new appointments. And today, although members from nonprofessorial ranks and members from the technical and administrative staff are elected on union lists, the work of the National Committee is far from being led by the unions. In the CNRS as elsewhere in France, they have lost a great deal of influence.

allow one section to be linked to two departments.[28] The second part of the document presented the new sections. A few changes in their names and those of the subdisciplines could be observed, and some linkages to the departments were different, but otherwise it was very similar to the first October draft. Table 1 shows what the global change would have been before the open consultation.

The draft from the end of November, with the detailed list of sections and subdisciplines, was used for a broad consultation; it was sent to the laboratory directors with a request for their opinions and suggestions. Each department conducted its internal consultations on the proposal as it wished. The Scientific Director of the Department of Mathematics and Pure Physics, for instance, explained in a letter that the names of the sections could still be modified and that the repartition of the subdisciplines among the sections was still open. He also presented four suggestions for interdisciplinary sections involving the Chemistry Department and the Engineering Sciences Department. The laboratories were invited to discuss these proposals, and the unit directors were asked to state their preference regarding a variety of possible scenarios by 8 December. It was added that the Departments were welcome to discuss any problem regarding the lab's evaluation with their scientific director.

These consultations led to another temporary working document issued by the directorate at the end of December 1990. It still included 6 departments instead of 7 (the Departments of Mathematics and Pure Physics and of Nuclear and Particle-Based Physics were to be merged). But now there were 39 sections (plus two on research valorization and management), instead of the 32 sections which had been proposed in October. Some sections which had disappeared in earlier versions reappeared, and other subdisciplines were joined in different ways.

On 31 January 1991, yet another version was approved by the directorate, based partly on the laboratories' answers. This draft was very similar to the one from late December, but included one more section inside the Department of the Science of Man and Society. This draft was followed by another pre-

28 The text also stressed that the new names of the departments and their subdivisions should be readily understandable for French scientists as well as for foreigners. We are not sure this goal has been achieved when we consider the problems we had in the first part of this paper translating the names of some commissions!

sented on 7 February to the Scientific Council. On 18 February, the Minister for Research signed the decrees necessary to put the new structure into effect; they were based on the draft of 7 February, with almost no modifications. The issue of how the National Committee sections were to be linked to the scientific departments was to be discussed on 21 March by the directorate.

Table 1: Proposed Departmental Structure of CNRS in November 1990 compared with Actual Departmental Structure in 1989

Department name		Number of sections		Average number of scientists per section	
actual (1989)	proposed (1990)	actual	prop.	actual	prop.
Physique nucléaire et corpusculaire	Sciences physiques et mathématiques	2	1	216	431
Mathématiques et physique de base		5	4	293	366
Sciences physiques pour l'ingénieur	Sciences de l'ingénieur	4	3	242	323
Chimie	Sciences chimiques	6	5	326	391
Terre, océan, atmosphère, espace	Sciences de l'univers	4	3	238	317
Sciences de la vie	Sciences de la vie	11	8	268	327
Sciences de l'homme et de la societé	Sciences de l'homme et de la societé	13	8	152	248
Total		45*	32		
* without the interdisciplinary sections					

Sources: Internal reports of the CNRS; Union press, CNRS

3.3 Different Perceptions of Trouble and Different Ways to Cope with It

Even if no one claimed to be opposed to consolidation and interdisciplinarity (since this would be seen as standing in the way of progress), the implementation of the reform was not going to be an easy thing. The extent of the debate that occurred throughout the CNRS and beyond its borders shows that the project hit on a point which was crucial to the scientists. The National Committee actually plays a key role in the life of the institution, that of evaluating the activities of laboratories and scientists and giving advice on appointments and promotions. As we described in Section Two, the way the resources are allocated by the scientific directors is mainly based upon this evaluation. Therefore, any reform affecting the National Committee is a very sensitive subject for the various scientific communities. Their reactions to this "political trouble," however, were far from homogeneous. The trouble was not so severe for every group, and, for some of them, it could even be translated into an opportunity.

3.3.1 A Divided Community

Opportunities for Change

For the disciplines or subdisciplines that had suffered from feeling marginal or underrecognized within the former organizational structure, the reform of the National Committee was seen as an opportunity to improve their position. This was especially true for disciplines located at the interface of several fields. Some of the molecular physicists, for example, were in favor of closer relations with certain chemists or physico-chemists working on similar topics or using similar techniques. The part of the group of the solid-state physicists studying amorphous matter regarded the interdisciplinary section between the physics and chemistry departments as an opportunity to build up their reputation and gain the recognition they believed they deserved.

Indifference

In other cases, the reform did not involve many modifications. For many, the name of the commission was new, but the shape of the section barely changed; these research units were satisfied, feeling that they were in agreement with their commission. These scientists appeared to be more indifferent toward the reform when they were asked about it, and answered that it had had very little impact as far as they were concerned.

In the section to which one of the labs examined in Section 2 belonged, we observed some individual strategies. Some scientists working on specific subjects wondered whether they should take advantage of the new constellation and switch from their original section to one closer to their own subject.

Severe Trouble

But some sections were faced with severe trouble. It sometimes became a matter of institutional life or death. As we described above, the first draft of the proposal called for a significant reduction of the number of sections. The reactions were very heated, of course, in the sections which were to be eliminated. During the autumn session, when, for instance, a *chargé de mission* presented the whole plan to the commission of a section which was to be dissolved, the commission – especially the section president, a famous scientist – protested vehemently. They did not want to be split up and join three or four other sections (in the same department or others). They viewed the proposed reform as an attack on their community, even though their section had been struggling with an internal conflict between two of its main specialties for a long time. Another threatened section equated its impending elimination with the disappearance of its entire discipline, along with its own special terminology and its particular approach.

In both cases, the defensive response of the affected scientific community was motivated by the fear of being relegated to a subordinate position in another section. Sections are actually made of groups (subdisciplines, regional groups, laboratories, individual scientists, etc.) competing for resources (positions[29] and funding). The prospect of merging with physics specialties known to be redoubtable adversaries in such a competition was not really appealing, even for brilliant scientists well known in their fields. In addition, even if some of the threatened biologists were able to publish in the same journals as their colleagues, they themselves said that their rate of publications

29 The reshaping of a section may modify the demographic structure of the researchers concerned and amplify or, on the contrary, reduce the problems of appointments and promotions.

could never be as high as the rate of scientists working, for instance, on AIDS, because of their own biological materials and because of the relatively low visibility of their research topics.

It is clear that in the new partition, recognition was at stake and all that goes with it: resource allocation, career opportunities, etc. Such a change in the scientific environment requires great efforts in order to make research results known, understood and appreciated by the new colleagues and evaluators.

3.3.2 Avoiding Trouble: Argumentation, Persuasion, Solidarity and Networking

Argumentation in Order to Persuade

Very quickly, the unions proclaimed their hostility to the executive director's plan in their publications and called upon scientists to raise strong protests by writing letters and bringing forward motions. But the reaction went far beyond the traditional opposition between the directorate and the unions; now the conflict was between the directorate and the researchers. Different kinds of argumentation therefore had to be developed, three of which we will discuss here.

As shown in Table 1, the reduction to 32 sections would have involved a great increase in the number of scientists in each section. The 21 (instead of the former 22) members of each commission would then have to evaluate more scientists and laboratories during the same period. Many of our interviewees, including commission members and unionists, argued that this would have been done at the expense of the quality of the evaluations.

Another frequent argument pointed to the fear of too wide a spectrum of themes in one section, stressing that peers would risk losing their scientific evaluation ability and, hence, their credibility. Some disciplines which had succeeded in creating a real niche for themselves within the CNRS (which some had not accomplished at the university) were quite afraid that they would lose the benefits of their institutionalization as a section in the CNRS if they were integrated into a larger section. They tried to prove that their section was a concrete and logical entity held together by intricate interactions that would be disturbed or even severed by the new partition. In their opinion, interdisciplinarity already existed, being the result of scientists' initiatives when they felt they needed it to solve a scientific problem; it could not be the consequence of a "technocratic" decision with artificial outlines. The scientific results in their section were put forward by the presidents when they met with their scientific director. Actually, scientists had to persuade the head of their department, who had great power in deciding which sections would be eliminated, that their community was worth being kept. The past accomplishments and promising research in progress were presented; common terminology and techniques, which contribute to defining a discipline, were demonstrated. The specificity of the biological material could be used as a strong argument in one of the sections, as well as the spectacular recent progress made in this discipline (mainly because of the energetic scientific director's guidelines) and revealed by the publications.

A third common argument against the reform had to do with the directorate's suggestion to resort to outside experts. Commission members maintained that they themselves were best suited to evaluate the evolution of the research performed within their own scientific community. And union organizations were afraid that promotions would take place outside the National Committee, with a more administrative management at the department level.

Solidarity against the Project

Beyond these arguments, solidaristic behavior can also be understood as a reaction to the reform. As we have shown elsewhere (Musselin 1990), the formal structure plays an important role in academic or scientific matters, not because it defines constraints, but because it creates territories. When these territories are endangered, scientists tend to group together and then develop a sense of unity that leads to solidarity. That is also what we observed in the case of the reform of the National Committee and in the cases of laboratory evaluation. In the threatened sections, a majority of the scientists[30] put their differences aside temporarily and adopted an almost unanimous stance against the reform. Many meetings took place within the different sections while negotiations went on, some of them bringing together all the laboratory directors, others the commission members or only the section office. Many sections developed some more or less corporatist attitudes that were frequently encouraged by the research units under their charge. These attitudes were sometimes

30 Those who saw an individual opportunity in the reform tried to be linked to another commission; we will come back to this later.

criticized by individual researchers of the same commission who viewed the reform as an opportunity.

Networking and Lobbying

Another way of trying to influence the final shape of the reform proposal was "networking," i.e. the mobilization of social connections in order to persuade. Numerous meetings took place, among researchers themselves and between famous – and consequently influential – scientists and their respective scientific departments. In some cases, counterpropositions were made through this channel. Some of the most influential members from the different scientific communities met with Curien, the Minister for Research.

But this mobilization seems to have gone beyond the CNRS borders. For instance, the prestigious French Academy of Science also came to the rescue of the threatened sections, through its CNRS member. Some advisory bodies dealing with science policy were consulted. When Kourilsky submitted his proposal to the CSRT[31] (chaired by Curien), council members belonging to the CNRS – and others who did not – acted as spokesmen for the fears of the scientific communities. A CSRT session took place in November 1990 in which comments were made on the vagueness of the scientific and political objectives of the new partition and on the confusion in the use of terms such as discipline, interdisciplinarity and multidisciplinarity. The CSRT suggested that the new partition should ensure that the names of the sections be easy to understand and avoid the scattering of disciplines among numerous sections which could lead to a double speed CNRS with "hot-topic" sections on the one hand and traditional fields on the other. It also advised the CNRS to limit its spectrum of fields within each section in order to preserve a certain unity of terminology and concepts. Thus, while the CSRT was not against a reform, it clearly supported some of the "demands" of the CNRS researchers.

But networking does not only work through official channels. It also encompasses efforts to mobilize the researchers' own connections in order to be even more persuasive. Famous academics from other countries working in fields which were threatened in France were asked to come to the rescue.

31 The High Council for Research and Technology (CSRT), presided over by the Minister for Research, discusses the civilian budget for research and development and other related issues.

For instance, some biologists intervened and told the respective scientific director how impressed they were by the recent research conducted in a biological sector that was to be eliminated as a section. They also argued that this discipline was institutionalized in other countries. Some directors of big industrial firms came to the defense of scientists with whom they had cooperated.

It is thus clear that the fields capable of acting collectively and mobilizing external interventions were in a good position to defend themselves and to negotiate.

Individual Strategies

The last type of coping we can describe here has to do with individual characteristics and the way the individual researchers chose the section to which they belonged at the end of the process. For some of them, the choice was obvious, because their research topic was dependent on one section. For others, careful deliberation was required because their research topic could fit into at least two sections. The individual criteria of choice were intertwined with expectations regarding the future configuration of the sections. Scientists had to guess who was going where, and to anticipate which group would be best for them to join.

It was important for a researcher to join a section where his research topic would be adequately represented in order to avoid a position that would be too marginal. But potentially fierce competition was an important consideration, too. And the fact that respected colleagues would be considering the same choice had to be taken into account. Thus, the decision process included many discussions revolving around collective group linkages, which were more or less achieved in the new partition.

The case of the reform of the National Committee shows that the "actors at the bottom" can play a significant role even when, at first, the decision seems to be a very top-down one. In a community used to self-management, which usually means management by peers, it is an established custom that the directorate and the unit directors consult the researchers before making a decision. The National Committee – or, as it is sometimes referred to, the "Parliament of Science" – is a symbol of this scientific democracy. A reform affecting this "parliament" therefore cannot be implemented in an authoritarian way. Scientific communities – which really become visible when a danger induces fellowship – are able to make themselves heard by using scientific arguments, the power of persuasion and networking. And the administrative authorities they have to convince very often come from the scientific community itself, which can facilitate the dialogue aimed at coping with trouble.

4 Conclusion

We have presented two different types of trouble confronted by French researchers of the CNRS. The first, dealing with association and disassociation, is, from a formal point of view, a rather routine decision-making process prepared at the intermediate level for the top.

The second, dealing with the reform of the *Comité national de la recherche scientifique*, is an unusual process launched by the top. The empirical study of both cases shows that the notion of a bottom-up or top-down process is not applicable and that each case is in fact a mix of successive, complex sets of relations, where bottom, top and intermediate levels are engaged in various kinds of interactions that are not linear. We cannot boil this complexity down to one typical pattern. The way decisions are prepared and finally made, as well as the way researchers cope, is contingent and differs from one type of trouble to another.

Nevertheless, if we look for structuring events in each of our cases, we will find some repetitions. First, networking is one of the most prominent common factors. Trouble is often a stimulus for mobilizing previously established relationships, reactivating those that have been dormant or even creating new ones. Second, each case is characterized by the existence of interferences between administrative, political and scientific actors. We must not imagine three parallel or superimposed spheres "fighting" against one another, but rather a multitude of interactions between the spheres of action breaking down the traditional distinction between them. Third, successful coping is usually associated with the capacity of the affected researchers to develop some collective action, i.e. to overcome disputes which may have existed among them and act collectively in the face of the actors causing the trouble. When it is happening, this collective action is accompanied by an important exchange of ideas and opinions. In the same way they use publications and conferences to prove to their peers that their results are true and interesting, scientists can – and do – instrumentalize scientific arguments in order to cope with trouble. Believing strongly in their legitimacy, they set a process of justification in motion which is not very different from their usual scientific activity. When they are successful, this leads to a redefinition of their own situation which will have consequences for their perception of the scientific context.

It also seems to us that the case studies we have described, even if they present different kinds of interactions, alliances and conflicts, are deeply structured by the characteristics of the system in which they took place. For instance, a consultative process is rather common in the French system (Friedberg/ Musselin 1993). Furthermore, the formal organization of French research imbricates the political, administrative and professional levels, facilitating the interplay between these different categories of actors. Some actors are thus at the interface of different domains of action, being (or having been) researchers and at the same time engaged (to a greater or lesser extent) in the definition of scientific policy, in the management of research activities or in administrative tasks. These persons have an important weight in the network mobilized by the researchers. Perhaps we can say that, in order to be effective, networking has to mix different spheres – the variety within the network is more important than the networking itself.

These connections between different categories also encourage the development of direct linkages between different levels, bypassing the hierarchical structure and standard procedure. Some researchers are in close contact with political actors and/or key administrative officials. This lobbying activity – which is sometimes more an expression of corporatism than lobbyism, since the negotiations do not always involve the defense of particular projects, but rather the defense of an entire profession (Girod de l'Ain 1989) – is very important for providing impulses leading to desired changes, for getting information about potential trouble (or opportunities), and for influencing the decision-making process. Therefore, we can say that scientific policy is more incitant than directive and that decisions are far more impulsed than imposed. And, far from being the monopoly of the top, the initiative may come from members of scientific communities who are sufficiently persuasive and influential.

Another insight which emerges from the empirical case studies is that "trouble" is a very relative notion. A political decision dealing with research activities may be understood very differently according to the way the scientist perceives it. This complicates the study of how scientists cope with trouble considerably for two reasons. First, we are forced to reflect upon what trouble actually is. Should we consider every event that changes the previous situation to be trouble? Or should we restrict trouble to situations that are perceived as a change *and* as a threat to the previous situation? The first definition seems to be more interesting for the analysis of case studies, since it suggests that one event can be seen either as an opportunity or a constraint. Coping with trouble should not be separated from exploiting opportunities. Both are connected to the perception of the system to which scientists belong. A good knowledge of the evaluation criteria and mechanisms is, for instance, a resource that can help a scientist to be successful – or to perceive a warning signal early enough. And there is a learning process of coping with trouble or grasping opportunities in which anticipation and prevention (which go beyond simple reaction) play a very important role.

The fact that trouble is relative leads us also to the realization that "researchers" are not a relevant category, and that an antagonistic understanding of the relationships between the state and the academia produces a rather inaccurate segmentation of both actors (the state and the researchers). The categories should not be pitted against each other, because the structure of alliances in the network cuts through the categories. Being in one category of actors does not necessarily mean solidarity with that category. In the case of the first lab, the unit succeeds in using another scientific director to persuade its own director on its own behalf. In the case of the second lab, a member of the commission (the physico-chemist) acts alone, parallel to the commission. Furthermore, we demonstrated that the network is based on individuals who belong to different categories which are linked. This is particularly clear in the case of the second lab, where a chain may be drawn from the DRED to the team leader. In this linkage, the role of the so-called *"marginaux-sécants"* – persons active in different systems and, hence, able to serve as intermediaries between them (Jamous 1968; cf. Crozier/ Friedberg 1977) – is of great importance: The physico-chemist links the commission up with the decision makers at the ministry and with the team leader; the scientific director links the politicians up with the scientific community, and so on.

We must, therefore, go beyond the formal categories (government, administrative authorities, research executive team, institute directors, researchers, etc.) and deconstruct them in order to rebuild the relevant groups on the basis of the conflicts and the alliances observed in each case study.

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Academy of Sciences in Crisis: A Case Study of a Fruitless Struggle for Survival

Renate Mayntz

1 The Threat to Organizational Survival

Research organizations may be confronted with "normal" and "extraordinary" trouble. A threat to the very survival of an organization that appears suddenly and comes to a head quickly certainly belongs to the second category. Such events are relatively rare, and therefore particularly worthy of study. The case of the research organization that was part of the former East German Academy of Sciences, whose dissolution was stipulated in the German unification treaty signed on 31 August 1990 and implemented by the end of 1991, provides an example; it will be analyzed in the following pages.[1]

The events that can endanger the very survival of an organization vary with its character. State-financed research organizations are not threatened by the same events that institutes living off the market for contract research must fear. Publicly financed organizations engaging in basic research may be particularly vulnerable as they are dependent on a single financial source for which there is normally no substitute, especially if the research organiza-

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1 The analysis is based on data collected in the framework of a larger project started in May 1990 and dealing with the transformation of extrauniversity research in Eastern Germany; preliminary results have also been presented in Mayntz (1992) and Mayntz (1993). Interviews and inofficial documents of the Academy and of the Federal Ministry for Research and Technology are the main sources of the case study presented here. Supporting evidence can also be found in Gläser (1992) and Klenner (1992).

tion is large and its support correspondingly expensive. On the other hand, it should take something akin to a major political earthquake before a large national research organization financed by the central state must fear not only to be cut back, but to be closed down completely. This is exactly what happened in the case of the East German Academy of Sciences.

State-financed research outside of industry and the universities was organized in the former East German Democratic Republic along the lines of the Soviet model, i.e. in academies. By far the largest and most prestigious of these was the Academy of Sciences of the German Democratic Republic (*Akademie der Wissenschaften der Deutschen Demokratischen Republik*, AdW). The academy included the traditional society of scholars, and in addition about 60 research institutes covering the whole range of disciplines (including the humanities and social sciences). The academy was headed by a president, who was an ex officio member of the GDR council of ministers and reported directly to the head of government. This form of research organization stands in sharp contrast to the institutionalization of publicly financed extrauniversity research in the Federal Republic of Germany, which is characterized by the existence of a large number of functionally specialized research organizations, most of which are financed jointly by the federal government and the federal states, and none of which is directly associated with a society of scholars. In fact, in West Germany academies existed only at the level of the federal states.

It is evident that when East Germany acceded to the Federal Republic on the basis of Article 23 of the West German Basic Law, the fate of the East German research institutions in general, and of the AdW in particular, became an issue. Without going into legal details it is important to note that while the mode of unification chosen implied the extension of West German law to East Germany, it would not have been legally impossible to preserve the AdW, or at least its two main components – the society of scholars and the research organization – separately. Thus, the society of scholars could become the academy of one or several of the new federal states (which is what actually happened). The research organization could similarly have become an institution jointly supported by the new federal states, or it could have come under the provision of joint federal/state financing which covers the German Research Foundation (*Deutsche Forschungsgemeinschaft*), the Max Planck Society (MPG), the Fraunhofer Society (FhG), the Big Science Centers, and the heterogeneous assembly of so-called Blue List Institutes. Surely, the realization of both legal possibilities would have met with substantial difficulties. But the point is that neither of these solutions was ever seriously attempted; instead, the Unification Treaty formulated by delegations of East and West German officials within a period of less than two months set down the binding decision that the AdW research organization was to be dissolved and its worthwhile parts integrated into the existing structure of the West German research system.[2]

It is, of course, impossible to assert that the dissolution of the AdW could have been avoided if it had only engaged in more effective coping. In fact, the overall situation was highly unfavorable for the survival of the AdW research organization. But for the outside observer it is possible to identify situational opportunities and potential responses to them which conceivably *might* have led to a different outcome. Irrespective of the chance of ultimate success, the coping efforts of the AdW were deficient in themselves, and in this sense the AdW at least contributed to its own downfall. Our explanandum is not this outcome, however, but recognizable coping deficits in the way the AdW reacted to a threatening situation.

2 Coping Deficits and Rational Problem-Solving

The requirements of successful coping are basically trivial if stated in a general way. There are, on the one hand, cognitive requirements, in particular (1) the correct and timely recognition of a threat – its nature and its causes; and (2) the identification of promising survival strategies, which include recognition both of the availability and the resources of possible coalition partners. On the other hand, the threatened organization must act effectively – adopt countermeasures, exert pressure etc. Obviously, there are threats for which effective countermeasures are not available: some problems are objectively unsolvable for a given type of actors under real-life conditions. A coping deficit attributable to an actor exists only if he fails to adopt the best *available* strategy.

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2 See Article 38 of the *Einigungsvertragsgesetz* (1990). Negotiations started immediately after the realization of the monetary union on 1 July 1990 and were concluded before 31 August, the date on which the unification treaty was officially signed.

So far, this sounds like the familiar model of rational problem-solving: we have an actor, decision alternatives, and some information about their consequences (the pay-off matrix in game theory). But as the empirical case of the AdW will show, this model needs to be extended to afford a satisfactory explanation of coping failure. In particular, it is important to extend its temporal dimension.

The set of strategies available to a given actor at a given point in time (time 1) is, among other things, influenced – enlarged or restricted – by that actor's own behavior at a previous time (time 1-n); time 1-n behavior may open future coalition chances or, on the contrary, foster opposition. In this connection, not only outward-directed actions, but also self-referential behavior by which an actor changes himself is of importance; thus, an actor can change his attractiveness for others, or his behavioral capabilities. This holds for individuals as well as for corporate actors. It follows that coping cannot meaningfully be analyzed as if it were a single-choice situation. Coping reactions (as well as other variants of strategic choice) are part of an ongoing process, where actions are linked sequentially so that future options are co-determined by past choices.[3] Coping is path-dependent.

The potential relevance of present choices for future options is rarely visible a priori. This is not (only) a function of cognitive limitations. Uncertainty with respect to the consequences of a given action (or strategy choice) is (also) an objective feature in cases where the effect of a given action is (a) not immediate, and (b) depends not only upon the outcome of the *present* choice situation, but also on future choices involving other actors. Complex processes whose course is determined by the uncoordinated choices of multiple actors thus meander unpredictably from bifurcation to bifurcation. In such cases it is impossible to collapse the multilevel "decision tree" into a single-choice situation confronting one focal actor. If this is so, the present choices of a given actor may lead him into a future trap, but they may also turn out to be unintendedly functional. Success in dealing with trouble is therefore sometimes not the result of deliberate problem-solving, but a chance result of "right behavior for the wrong reasons."

3 This corresponds to evolutionary processes where the forces of environmental selection impinging upon a given organism or species are, at least in part, a function of past adaptive behavior.

A third point is of importance. As emphasized in the introductory chapter in this volume, coping behavior involving formal organizations is a multilevel process composed of the – interdependent and interacting – behavior of the organizational leadership, subunits enjoying an independent action capacity, and individual members. The trouble-causing external event may be perceived – and responded to – in different ways by the actors on different organizational levels, and these responses become constraining or facilitating, helpful or harmful elements of the situation confronting each of them.

In accounting for the responses of the AdW to the events that threatened its survival, the *process and multilevel character of coping* plays a crucial part. The rapid, but still stepwise build-up of the crisis situation and the parallel sequence of AdW reactions to the (changing) perceived situation will be analyzed in two major phases. These phases are defined by the course of the external events from which the threat to the survival of the AdW derived. This threat did not originate in the AdW's own environment, the science system of the GDR, but was caused by the breakdown of the East German political system as a whole.

In the fall of 1989, when the East German regime began to crumble and Honecker, who headed both the government and the socialist party, had to resign, German unification was neither sought nor held to be possible by political actors in East and West. In the course of less than one year, several dramatic changes in the overall political situation occurred, and each change caught most of the participants in the drama by surprise. This holds, of course, especially for the revolution in East Germany and the opening of the Wall (on 9 November 1989) that ushered in a phase of friendly cooperation between the two German states and constituted Phase I of the process analyzed here.

Phase II began with the – at the time equally unexpected – shift from cooperation (and a possible future confederation) to unification. This shift can roughly be set at March, 1990. On 18 March, the first free elections in the GDR produced a landslide vote against the formally governing socialist party. By the end of April, the parties of the East German coalition government had agreed to seek accession to the FRG on the basis of Article 23 of its Basic Law, and public pressure for a speedy unification had grown explosively. Up to this point, it may have appeared realistic for East Germans to assume that a reformed, and possibly no longer socialist East German state would continue to exist. Only when this was no longer a realistic assumption was there any reason to fear a political decision to break up and (partly) dissolve the AdW. Skeptics (or optimists, depending on the perspective), however, might have anticipated at least the *possibility* of the disappearance of a separate East German State in the foreseeable future already on 11 February, when the Soviet Union accepted in principle that Germany might become unified. Phase II ended with the political decision, taken in August 1990, to dissolve the AdW.[4]

3 The AdW in Phase I: Opportunistic Responses

In the fall and winter of 1989/90, most East Germans interpreted the general political situation as one of internal reforms. Such a situation presents both opportunities to seek redress of long-suppressed complaints, and a threat to defenders of the status quo. As is likely to happen in all vertically differentiated social systems, the opportunities as well as the threats that the political events implied were different for actors on different structural levels of the AdW. Here as in nearly all other areas of social life in East Germany, the political events of October and November 1989 unleashed a wave of reform initiatives. Most of these initiatives pointed in a similar direction, i.e. against the previous forms of centralized hierarchical control. But this meant different things on different organizational levels of the AdW, and thus led to conflicts that had ultimately fateful consequences no one could have predicted at that time.

For the AdW leadership, the political situation seemed to offer the opportunity of gaining autonomy from the formerly strict political control. Characteristic of these claims addressed mainly to the government in East Berlin, but more generally to all influential political forces in the GDR, is the Open Letter published by the AdW presidium on 28 November 1989, soon after the opening of the Wall. In this letter, which explicitly assumes the persistence of a (reformed) socialist East German state, greater institutional autonomy, improvement of the technological infrastructure of research, access to the international community of science, and a removal of the restrictions

4 For a time table of the major events see Schäuble (1991: 289-314).

placed on basic research by the erstwhile strong pressure to produce applied results were demanded. Soon afterwards (on 7 December 1989), a plenary meeting of the academy formally enunciated the strict separation of the AdW from political parties and other mass organizations. To secure the goal of a greater autonomy and at the same time a voice at least in the development of science policy, the establishment of a Science Council and the introduction of a Science Law was proposed by the AdW president (*AdW Pressedienst*, No. 2/90). On the other hand, the academy leadership[5] did not subscribe, on its own initiative, to any far-reaching internal organizational reforms. The strong insistence on autonomy may later have contributed to the distant relationship between the AdW and the field of politics, and hence to its political marginalization at a time when its active involvement would have been highly propitious.

Passing on to the level of the individual research institutes, we see that their main goal was similarly greater independence from hierarchical control. Their claim, however, was not only directed toward external authorities, but mainly toward the academy leadership itself. In a radical form, the striving of the research institutes for independence harbored the threat of secession of the AdW research organization from the academy as a whole. In fact, this threat was quickly perceived by the academy leadership and strongly resisted. From December 1989 on, the AdW leadership missed no occasion to emphasize that the unity of the AdW should be maintained. As yet, however, the research institutes did not seek to leave the fold of the AdW individually. What they sought was collective independence, and in striving for this, they in fact formed a collective identity that previously did not exist. Thus, from early 1990 on, the institutes that had previously been fitted into the unitary hierarchy of the academy and had been loosely organized into disciplinary groups now began to define themselves as the *AdW Forschungsgemeinschaft* and to demand a board of their own, distinct from, even if formally a level below, the academy president.[6]

5 The academy leadership – the *Präsidium* – was composed of the president, several vice presidents, the secretary general, the secretary of *Präsidium*, the chairmen of the different classes of the scholarly society, the coordinators (*Sekretäre*) of the different research fields, the district officials of the socialist party SED and its youth organization FDJ, and a union representative.

6 In effect this meant returning to a structure that had existed, in broad outline, before

At the micro-level of individual researchers, finally, emancipation from hierarchical authority was sought in the form of new democratic procedures. Thus the establishment of works councils in the individual institutes as well as for the academy as a whole was demanded, as was the creation of special elective bodies of the AdW scientists, who wanted a voice in research management. The democratic reform movement also called for the election of a new president and of new institute directors. These demands for intraorganizational democratization and renewal challenged the authority of the established leadership of the academy at large, and of its institutes.

The reforms sought by actors at the lower organizational levels thus produced a conflict between conservative defenders of the organizational status quo and those advocating organizational reforms. Since the AdW leadership did not espouse intraorganizational reforms, the lower level reformers articulated their ideas in the *Initiativgruppe Wissenschaft*, which had been formed with more general, political goals in mind. This loosely organized group was instrumental in the formation of a Round Table of the academy, following the model then practiced in East German politics, and in setting up further representative organs, such as the works councils (*Rat der Institutsvertreter* at the academy level, *Institutsräte* at the institute level), a special women's group, and a number of other working groups. The activities of these diverse new bodies overlapped in their concerns. The fact that the AdW leadership did not itself become the spearhead of the reform movement thus led to a fragmentation of the decision structure which was polarized between the old formal authorities, whose claim to leadership was widely contested, and the representatives of the reform movement, who lacked official recognition and formal authority. This impeded the action capacity of both sides. Later, the two sides established a modicum of cooperation[7] that channelled the conflict into a process of organizational reform.

The gradual erosion of the academy's old decision structure, which was not speedily replaced by a new one, resulted from a strategic choice on which there was full agreement on all levels of the AdW and which was generally characteristic of the "bloodless revolution" in the GDR: the option for orderly reforms, rather than for a quick coup d'état or a bloody upheaval and radical

the academy reform of 1968/69 abolished it in favor of strict hierarchization.

7 The ensuing cooperation between reformers and the old leadership is particularly emphasized by Klenner (1992: 164-168).

abolition of the old governing elite. This was not a deliberate choice, however, but appears to follow naturally from the emphatic assertion of democratic principles, the core value that guided the "bloodless revolution." Along the same line, the decision to draft a new charter and *elect* new leaders was also an affirmation of the newly claimed emancipation from heteronomy. Such a choice was, moreover, congruent with the then widespread belief that the socialist regime, though it had gone astray, rested on basically sound principles, so that reforms rather than a radical turnabout were the appropriate route to take.

It is in the light of such shared convictions that the old AdW leadership and the reformers were able to embark on a process of intraorganizational reform that soon crystallized around the development of a new statute. The new statute was to give a legal basis to the desired, and partly already practiced, reforms. In the course of time, numerous drafts were produced by different groups and individuals within the AdW, with the debate focussing on the different aspects of intraorganizational reform already alluded to. Altogether, it took only three months from the time a first draft statute was officially introduced for discussion by all bodies of the AdW (18 January 1990) until the day that a newly established elective assembly (*Konsilium*) voted in favor of a new statute on 26 April 1990. On 17 May 1990, a new academy president was elected on this basis. Given the existing differences of opinion, this is a relatively short time to achieve a working consensus on a new statute and elect a new leadership, especially in view of the absence of any direct external pressures to engage in such intraorganizational reforms. This relative speed of the decision process reflects a surprisingly low level of manifest conservative opposition – a phenomenon which Western observers of political reforms in the GDR generally noted with surprise.

4 The AdW in Phase II: The Effects of Time Lag and Cognitive Deficits

What may have been a rather speedy reform process if compared to other cases of bottom-up initiated reforms in large organizations was, however, too slow in the face of the continuing political upheavals. The coincidence of the AdW's choice of reform rather than revolution (or stasis) and of a significant change in the external political situation goes a long way to explain the AdW's strategic action deficits in Phase II.

Although it is obviously impossible to prove, it seems likely that the Modrow government, which was in office until the elections of 18 March 1990, would have accepted the outcome of the AdW's internal reform efforts. But by April, when the academy decided on its statute, the new de Maizière government was in office and the overall definition of the political situation had changed from the paradigm of cooperation and possible confederation to the paradigm of unification, which meant that the future of all major East German institutions was suddenly at stake. The de Maizière government, conscious of its transitional nature, hesitated to take decisions that seemed only meaningful in the context of an independent East German future. There may have been other reasons as well,[8] but it was in any case consonant with this orientation that the government withheld official recognition from the newly elected AdW president until late June (i.e. roughly the time when negotiations about the unification treaty began) and rejected the new statute, asking for a revised version. When this was finally presented, it had become obsolete, since by that time it was clear to all concerned that the AdW would not persist in its old form; the internal reform efforts had produced the "right" results at the wrong time. As a consequence, during the crucial period between March and May of 1990 the academy was practically without a leader accepted both by its members and by its institutional environment. As a corporate actor, the AdW could therefore not take part in the informal negotiations that laid the groundwork for the unification treaty in this period.

There were, however, also cognitive deficits. As briefly mentioned above, the change in the definition of the political situation from cooperation to unification came about stepwise. Starting in February 1990, unification gradually became more probable, so that it was possible, with a bit of political imagination, to anticipate a possible threat to the persistence of the AdW already before the election of a new academy leadership in May of that same year. However, unification did not appear imminent in this period, nor were the implications of an accession on the basis of Article 23 of the FRG's Basic

8 Both Gläser (1992: 40) and Klenner (1992: 170) suggest, for instance, that the East German government resented the autonomy which the AdW claimed in its new statute, including the right to elect its president instead of having him appointed by the government.

Law understood well enough to make the perception of a threat unavoidable. In this cognitively ambiguous situation, the AdW as a corporate actor failed to display the necessary amount of political imagination that would have permitted it to develop in time strategic countermeasures for the possible event that its existence should be challenged.

Several reasons probably contributed to this failure. The leadership void at the top of the academy is certainly one of them. Until May, there existed only the old, discredited establishment that withdrew into passivity; between May and the end of June, the new leadership concentrated on gaining acceptance within and recognition without. Not only was the – old and new – leadership busy solving its own problems; activists on all organizational levels focussed their attention so completely on the difficulties of the reform process paramount on the AdW agenda that they were blind to the hints that a threat might be approaching.

A second factor of importance is the selective orientation of AdW officials toward the East German state, whose imminent disappearance they probably neither wished nor anticipated. The AdW did not receive any clear signals from this particular environment indicating a threat to its very survival. In East Berlin, the problems of extrauniversity research were decidedly at the periphery of political attention. The political Round Table that had been the center of political reform activities until the March elections did not set up a working group dealing with science and research until its very last meeting. In de Maizière's governmental platform, the field of scientific research was only very briefly touched upon, and his affirmation that the state should guarantee the promotion of basic research fitted well with the demands of the AdW, which tried to shake off its former dependence on contract financing by the large state enterprises with its attendant pressure toward applied research and development.[9] The only fact that might have been interpreted as an alarm signal was the increasing orientation of the new East German Ministry of Research and Technology to achieve compatibility with the West German system of research promotion; this orientation had already been evident in the time of the Modrow government (i.e. before the March elections), but became increasingly so after the constitution of the new government in

9 See de Maizière's *Regierungserklärung*, reprinted in *Neues Deutschland*, 20 April 1990: 6.

April. But while advocating structural reforms of the AdW, the responsible minister never openly questioned its continued existence as an organization.

To the extent that the political turbulence did affect the AdW negatively, the problem was perceived mainly as a financial one. In early 1990 it had already become evident that the AdW would be confronted with severe financial strictures, which became increasingly acute when the East German state enterprises had to cancel more and more of their contracts with AdW research institutes. As the East German government was not able to make up for the lacking funds from industry, the AdW was soon forced to rationalize and cut down on its expenditures. Next to the concern with intraorganizational reforms, these financial problems absorbed the attention of the AdW on all levels. The measures devised to cope with the financial crisis included an attempt to privatize production- and service-oriented institutions (or subunits of large institutes), and efforts to promote the transfer of research units both to universities and to private firms forming in East Germany. Perceived as means of rationalization and scaling down, these were ironically also measures fitting a strategy of organizational dissolution and subsequent reintegration of valuable basic research units into other structures.

The fact that the AdW's *East* German political environment did not signal a threat to its institutional survival could be reassuring only if one overlooked the fact that the real challenge to its survival came from actors in the *West* German research policy network. After the March elections, these actors began to discuss the institutional structure of a future unified German research system. In the beginning of this process of opinion formation, maintaining the AdW research organization in a structurally modified and scaled-down form was one of the options considered. But this lasted only until May, by which time the conviction had gained ground within the West German research policy network that the AdW research organization should be dissolved and its worthwhile parts integrated into industrial research, the universities, and other existing forms of state-financed research organizations.[10] There existed, thus, a small window of opportunity for the AdW. Had it recognized this window *and* realized it would only be open for a short time, and had it managed to enter the decision process early enough, the AdW might have favorably influenced the opinion of the relevant policy makers.

10 For details of this process see Mayntz (1992).

5 The Strategic Requirements of Successful Coping

Even if the AdW – its official leadership or some person or persons able to act on its behalf – had recognized the threat to its institutional survival and its source in time, the question is whether it would have been able to make use of the window of opportunity. To do so, it would have been necessary to convince especially West German policy makers that it made sense to opt in favor of the AdW's maintenance. Negotiation with the new (East German) federal states, which might jointly have supported the AdW, was hardly possible because their establishment, announced by de Maizière at the beginning of May 1990, took place only after the ratification of the unification treaty. In the meantime, fiduciary representatives of the new states had been appointed, but they would have hesitated to make such weighty future commitments as the joint support of the AdW implied.[11] The alternative was to include a reformed AdW research organization among those publicly financed institutions supported jointly by the federal government and the federal states under the auspices of an agreement concluded in 1975 (*Rahmenvereinbarung Forschungsförderung*; see Bentele 1979). This was in fact the AdW's first preference, not only because it appeared more viable, but also because it clashed less with the history of the AdW as a central state institution, and with the "statist" orientation characteristic of the GDR in general. However, the AdW leadership does not seem to have appreciated how unfeasible this solution was in the eyes of the Western policy makers, whose support at this time would have been essential.

It was widely accepted among Western *and* Eastern actors that the AdW was grossly overstaffed, and undersupplied with modern research technology (Meyer 1990; Meier 1990). This made its support an expensive proposition, harboring future resource allocation conflicts among the different organizations vying for state support. The AdW recognized, and was quite willing to

11 Here the fact that the former GDR did not join the Federal Republic as *one* "Land," but that several new federal states were created instead, is crucial; this *one* political actor might well have decided to keep the AdW intact. Simon (1992: 29) considers this to have been the decisive factor for the fate of the AdW.

respond to, such criticisms by accepting severe cutbacks ex ante.[12] But there were other critical arguments which the AdW did not dispel.

One important feature that made the AdW research organization unattractive in the eyes of Western policy makers was the presumed low quality of the research conducted there. At the time when this conviction gained ground among West German policy makers, it was based on fragmentary and mainly impressionistic evidence. Scientists in the AdW recognized the importance of projecting an image of scientific productivity and high innovation potential, but the efforts which they, and subsequently the new academy leadership, undertook to this purpose remained unsystematic and occasionally took the form of a clearly euphemistic self-evaluation. It was the (West German) *Stifterverband*, a sponsoring organization financed by private industry, which, upon the initiative of the (West German) Science Council, commissioned bibliometric studies comparing East and West German scientific productivity; but this took place too late to influence negotiations in preparation of the unification treaty. Preparations for a systematic evaluation by the Science Council started in July 1990; their purpose, however, was not to justify the perpetuation of the AdW, but to provide a basis for recommendations concerning the future of individual AdW institutes after the formal dissolution of the academy.

Another feature that made the maintenance of the AdW research organization appear unattractive was its close association with the overturned socialist regime. The AdW's dissociation from this past was not very convincing. Neither the AdW as a corporate actor nor many of the scientists individually had been actively involved in the reform movement that triggered the downfall of the East German regime. As late as August 1990, only every second institute director had been relieved of his post,[13] sometimes only to be replaced by some other person from the former elite. The members of the scholarly society, in spite of some prodding even on the part of their newly elected

12 In a comprehensive document presented on 23 June 1990 (*Kurzcharakteristik der Institute und Einrichtungen sowie konzeptionelle Vorstellungen für deren Entwicklung und Zuordnung*), the AdW accepts as feasible a 60% reduction of its 1989 personnel of some 24,000.

13 As evidenced by a comparison, performed by Hans-Georg Wolf, of information in the academy's last official yearbook with the information supplied by the institutes to the Science Council in September 1990.

president, were unwilling to give up their membership, though it was an open secret that a certain number of them had received the honor of membership for political reasons rather than for their scientific excellence. Demonstrative actions of rehabilitation likewise remained few. Not even the election of a new academy president (and other academy officials) served to dispel the apprehension that if the AdW survived, large parts of the politically tainted scientific establishment would survive along with it. Klinkmann, the new president, had been a member of the scholarly society only for a few years and was neither a long-term nor high-ranking member of the SED, but he was a well-known member of the former GDR science policy elite who never tried to hide his personal attachment to the East German state. Maybe it would have been difficult to find a prominent presidential candidate *not* identified with the regime, given the specific conditions of the GDR, where the escape route to West Germany had impeded the growth of a strong intellectual opposition. However that may have been – the AdW at any rate did not take the steps that could have dispelled the politically motivated skepticism.

The political conservatism of the AdW did not only harm it in the eyes of Western decision makers, but it also meant that it could not count on a great deal of goodwill on the part of the political forces now governing in East Berlin. Both the East German government and the minister responsible for research policy had a rather ambivalent attitude toward the AdW, wanting to preserve it on the one hand as one of the institutions which East Germany could still identify with, but being nevertheless highly critical of its present state and promoting more or less radical reforms. Not even the fact that scientists who formerly belonged to the AdW now held high positions in the Ministry of Research and Technology in East Berlin proved helpful, as these persons did not identify with the AdW, but with the new government and their minister – a Social Democrat whose major aim was to make the East German scientific system as compatible as possible with the West German one in order to facilitate their future integration. Since neither Klinkmann nor other high-ranking AdW officials had close ties to one of the political reform groups, the AdW remained isolated from both the network of East German political decision makers and from the West German policy network.

But even if the AdW had successfully countered accusations that its scientific work was of dubious quality and that its researchers were politically tainted, there would still have been resistance on the part of the West German research policy network against its preservation as a major German research organization. This resistance grew out of a feeling shared by the major corporate actors in this network that the institutional structure of the West German system of scientific research was functioning very well. After extended conflicts in the 1950s and 1960s, this system had reached a relatively stable equilibrium in the 1970s, based on functional specialization and a generally agreed-upon distribution of domains (see Hohn/ Schimank 1990 for a detailed analysis). This institutional consensus covered the major research organizations and different categories of research institutes, as well as the research promotion powers of the federal government and of the federal states. In the 1980s, this institutional system had been characterized by high structural stability and a very low rate of conflict. Unavoidably, the introduction of a new research organization into this system would have reopened the Pandora's box of conflicts about legitimate domains and spheres of influence – exactly what the West German policy makers wanted to avoid (Mayntz 1992). To gain the full support of at least some of the major West German corporate actors, the AdW would have had to project the image of an *attractive addition* to the existing system that did *not endanger* its equilibrium. To be fitted into a research system based not on competition but on functional specialization between different organizations or categories of institutes, the AdW therefore would have needed to identify a niche not yet occupied. This was not an easy task, as all the *recognized* components of the chain reaching from basic research to practical application appeared to be represented by existing (West German) research institutions. But as functional needs are, at least in part, a matter of definition, a niche to be filled by a reformed AdW research organization might still have been carved out.

There was widespread recognition within the AdW that, in order to survive as a research organization, it would have to develop a new profile.[14] But there was disagreement among the AdW planners as to what this profile should look like. Some favored the return to basic research and wanted to model the future AdW research organization (for which the name "Leibniz Gesellschaft" was now sometimes used) on the Max Planck Society. Others

14 As Gläser (1992: 42) correctly points out, there was considerable willingness in the AdW to accept advice from the West in doing this. A leading West German scientist was even asked to advise the (new) AdW president in drawing up a plan for the future AdW research organization. But this is an indicator of insecurity rather than of a correct and independent assessment of the nature of this particular strategic necessity.

saw the hallmark of a future Leibniz Society in the close interrelationship between basic and applied research that had been the AdW's leading principle. Still others proposed to split the research organization up into two parts, one of which would parallel the West German Max Planck Society, the other the Fraunhofer Gesellschaft (more involved in applied research). The AdW planners accepted that they would thus enter into competition with existing German organizations. This was in conformity with the affirmation of market principles then en vogue. What the planners probably did not realize is that the principle of competition ran in fact counter to the institutional consensus in the West German research system. Nor would the suggested territorial segregation of basic research domains between the Max Planck Society in the West German states and the Leibniz Gesellschaft in the East German states have solved the problem, as this ran counter to the very idea of unification by accession, which implied not only *one* government and *one* law, but the territorial extension of *all* major West German organizations into the new federal states.

The strategic requirements outlined above were exacting, but not *in principle* impossible to fulfill for a large research organization. If the AdW failed to do so, this is partly due to cognitive deficits, i.e. its failure to perceive what was needed to make it appear, in the eyes of West German policy makers, an attractive addition to a research system of the West German type. But it is not clear that, even in case of a correct and timely perception of the situation, the AdW would have been *able* to do what was necessary. This obviously holds in particular for the projection of a politically acceptable image, something that required much more than skillful impression management. We shall return to this point in the end, after having considered what the AdW *did* do when it realized the threat.

6 Coping Efforts

Between June and August of 1990 it became increasingly clear that the future of the AdW research organization was in peril, even if the brute term "dissolution" was not openly used by the responsible policy makers.[15] Just as the situation in Phase I offered different reform opportunities to actors on different organizational levels of the AdW, the threats now differed between them. For the AdW, a formal dissolution meant its disappearance as a corporate actor, even if the scholarly society was later to be recreated (in a deeply modified form). But the fate of individual AdW researchers and institutes was not completely tied to that of the AdW. Some of the AdW institutes, or at least subunits of such institutes, saw opportunities for an independent institutional survival, possibly even in a form they preferred over the past – for instance as a Big Science Center or a Max Planck Institute. Individual scientists, in turn, might have found employment elsewhere if the AdW and their own institute ceased to exist. For them, the severe cuts envisaged by the AdW's own reform plans already spelled danger that could have motivated a search for alternatives, while a simple change in their institute's organizational affiliation need not have affected them at all.

Since the imminent policy decision to formally dissolve the AdW had different meanings for the academy as a whole, the institutes, and individual researchers, their strategic alternatives also differed. Individual researchers for instance could look for employment elsewhere, while institutes could try to be adopted by a new carrier organization. For the AdW as a corporate actor, the only meaningful response was to try to prevent the threatening decision, and this is in fact what its new president attempted.

The event that threatened the survival of the AdW was a *policy decision* taken at the national level. The decision process began officially with the first meeting of the East and West German delegations that were to formulate the unification treaty.[16] The working group that was to draft the section con-

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15 Agreement in the West German policy network to break up the AdW research organization and fit those of its parts which met Western qualitative standards into the preexisting West German structures had been reached by the beginning of July 1990. In the ensuing formal negotiations, however, those who preferred a less explicit formulation that would leave the Science Council some leeway for its recommendations won out. Even in the final version of Article 38 of the unification treaty, the dissolution of the AdW research organization is not thus called, though the legal stipulations to this effect are quite clear.

16 For a detailed description of the negotiations see Schäuble (1991); as Minister of the Interior, Schäuble led the West German delegation, while de Maizière's state secretary Krause led the East German delegation. Below the level of these two delegations, working groups were active at the departmental level to draft the proposals for their respective

cerning the East German research organizations met for the first time on 13 July 1990; on 31 August, the unification treaty was signed. The working group was composed of high-ranking officials from the East and West German Ministries of Research and Technology; additional members were representatives from some other ministries and from three of the (West German) federal states. Occasionally other corporate actors were invited to a meeting. AdW representatives took part in one meeting only. West German research organizations did not participate at all in the negotiations. The decision about the survival of the AdW was thus taken by a small number of officials in a negotiating system in which the academy did not regularly participate. As a corporate actor, the AdW therefore had to rely mainly on indirect representation. Here the East German research ministry was its official advocate – and in fact its only one, the AdW having failed to win the support of Western policy makers in time. But as we have already seen, even the AdW's "born" advocate had an ambivalent attitude toward it and could, in the course of the negotiations, be convinced to accept the – then firm – Western view that the AdW should be dissolved. Last-minute attempts of the academy president to mobilize the East German government in its defense also failed; in any case they came too late in the decision process.

Even though the fate of individual AdW researchers and institutes was not completely tied to that of the AdW as a corporate actor, they still were vitally interested in the outcome of the political decision process. From the very beginning, there had been consensus among East and West German policy makers on the goal to preserve the valuable AdW research potential. For AdW institutes and researchers the big question was how that goal would be translated into effective policy.

In contrast to the corporate actor AdW, even its larger research institutes could not attempt to enter or influence the decision process directly – let alone individual scientists. In policy development, corporate actors preferably interact with other corporate actors. Even if a corporate actor such as a ministry *can* and sometimes does interact with individual research institutes and even with individual scientists, for plainly practical reasons such contacts are limited. Contact patterns therefore tend to be level-specific, with higher-level corporate actors seeking out other higher-level corporate actors. The West German Minister of Research and Technology thus consulted with the big

policy sectors.

research organizations MPG and FhG by talking to their presidents, while the Blue List institutes which have not formed a higher-level research organization played no role whatsoever in the decision process about the AdW.

Lacking effective representation by the corporate actor AdW, the AdW researchers and institutes had no way of influencing the negotiations directly. Strictly speaking, there are no special-interest organizations within the research sector, and there was not even a powerful union ready to defend the interests of the East German researchers threatened by unemployment. The East German scientist's union was being dissolved at the time, and its members were given a choice of two West German unions (*Erziehung und Wissenschaft*, GEW, and *Öffentlicher Dienst, Transport und Verkehr*, ÖTV) to join if they wished; the more relevant GEW, however, was much more concerned about the infinitely larger group of East German teachers than about the fate of the AdW scientists. The virtual exclusion of the AdW from the policy network therefore meant that its members were without a direct representative in the policy process and had to rely on the advocacy of the East German research ministry to articulate their demands. In this, they were in fact better served than the AdW as an institution, as the East German science minister successfully pressed for a "moratorium" for the AdW personnel, an employment guarantee until the end of 1991, by which time the future of the various institutes was expected to be settled. The East German research minister, a Social Democrat sensitive to welfare issues, had repeatedly been faced with the fears of the academy personnel in the weeks preceding the negotiations, and had assured them of his support in trying to avoid unemployment for them. But his insistence on a (limited) employment guarantee would have been to no avail in the treaty negotiations without the support of the West German representatives. We will shortly come back to this point.

As far as their own active coping strategies were concerned, AdW scientists and institutes could either try to muster their collective force, or they could try separately to save their own skins by seeking alternatives outside the AdW research organization. The opportunities for individual researchers to find employment, and for complete institutes to find new carriers on their own, were much too insecure to motivate exit reactions on a large scale. Where exit was in fact attempted, it was motivated less by the pull of attractive alternatives than by the push of the AdW leadership's perceived inability to ensure the survival of the organization. But if the extent of realized exit reactions was limited, and restricted to individual scientists and at best small groups, this did not mean that solidarization, and collective activity in support of the AdW, predominated; the most common reaction was rather a kind of stunned helplessness. A reaction profile polarized between flight and total passivity is a well-known consequence of extreme stress (or danger). In this case, the withdrawal into passivity was reinforced by the old habit of expecting to be taken care of by some superior authority. Many leading AdW scientists felt, moreover, ambivalent about the academy, from whose strict hierarchical control they were just trying to emancipate themselves. Therefore, except for some demonstrations on the occasion of the academy's traditional Leibniz Day in June, there was no self-organized solidary action on the part of the AdW institutes, nor massive and publicly visible protest on the part of the academy personnel against the plans to dissolve the AdW – a protest that might have been translatable into political pressure. The resulting pattern of lower-level reactions, i.e. little public protest, and a limited amount of definite exits did nothing to strengthen the defensive capacities of the corporate actor AdW; it rather impaired its stability.

A group of actors that can neither directly participate – nor is vicariously represented – in a decision process affecting its future can still bring to bear its hopes and fears upon the decision makers by – intentionally or not – changing *their* situation. Higher-level actors observe spontaneous developments at lower levels attentively if these are preconditions for reaching stated goals, or pose threats to their achievement. In this particular case, East *and* West German decision makers involved in the negotiations about the future of the East German research system feared that an uncoordinated and speedy migration of AdW researchers to West German industry or new private companies, to foreign countries or into nonscientific professions might lead to an erosion of what was to be preserved. It was also feared that West German research organizations, but also industrial firms would try to pick out and take over the best of the research units, leaving only the blighted rest and thus rendering the restructuration effort futile. Therefore, as soon as impressionistic evidence called attention to the possibility of such developments, there was agreement on the need for provisions to reassure AdW scientists. It was on the basis of this consensus that the East German research minister's demand for a "moratorium" was accepted. The systematic evaluation of all AdW institutes by the Science Council as well as a limited employment guarantee were thus written into Article 38 of the unification treaty. Not being able to negotiate with them individually, the policy makers hoped that the scientists would collectively change their behavior in response to these measures.

7 Coping Failure: Fault or Fate?

It holds generally that promising coping strategies presuppose the possibility to identify a *manipulable* point, an event or variable within the reach of the troubled actor where successful intervention would avert or mitigate the threat. This "intervention point" may, but need not be the original source of the trouble. In our case, such an identifiable intervention point existed – the policy decision about the future of East German research; this was obviously distinct from the original source of the AdW's troubles. However, access to the promising intervention point was restricted to corporate actors belonging to, or being able to find their way into, the policy network where the decision took shape even before it was formally made. The AdW as a corporate actor could in principle have gained this access; as we have seen, it missed the opportunity. Individual members and organizational subunits of the AdW did not even have the chance of direct access. If they wanted to influence the crucial policy decision, they had to rely on representation or advocacy. In our case, it was not so much a collective effort such as lobbying, or political pressure mobilized by AdW scientists, that ultimately secured some success, but – quite unintentionally – the threatened, and realized, exit reactions, i.e. individual adaptive responses. Individual "save your own skin" reactions achieved what might have been sought by solidary action. This constellation, while not unique, does rest upon very special preconditions: Those actors whose behavior (or decisions) constitutes the imminent threat must in turn *fear* the uncoordinated, individualistic coping reactions or their aggregate effect. This indirect way of exerting influence is thus highly selective in favor of threats to the interests of the higher level (corporate) actor(s), while there is no chance to use persuasion and to argue normatively, to enter into a moral discourse as it were and to bring values to bear upon decisions.

As for the coping deficits of the AdW, we have found evidence in the preceding account of both a fatal *temporal disjunction* and a *substantive incongruence* between coping responses and situational exigencies. With respect to the first point, we have seen how the reform process, started in a situation that was primarily perceived as offering opportunities, absorbed the attention of the participants and incapacitated the corporate actor in its external relations, producing in the end a result that no longer fit the changed political situation, thus prolonging the period in which the AdW was without a recognized leadership. In this way, the response of the different ranks within the AdW in Phase I *jointly* contributed to coping deficits in Phase II. For the AdW leaders, the threats that were perceived in Phase I came from within the organization, i.e. the danger of secession of the research organization from the academy, and the challenge to the established AdW authorities. Their partly resistant and partly cooperative responses to these internal threats made an internal reform process possible and in this way helped to delay the recognition of, and response to, the external threat.

Much later, reformers in the AdW became aware, and regretted, that they had helped to destabilize and incapacitate the AdW by their reform activities – an effect they could hardly have foreseen at the time. Had they refrained from reform activities and chosen a strategy of status quo maintenance, this would, however, also have been to the AdW's disadvantage. In fact, our case illustrates very well that if the membership of an organization does nothing to challenge its status quo, this is *not* necessarily functional for the corporate actor. A strategy of status quo maintenance would have preserved the (old) AdW leadership intact during the crucial period in the spring of 1990, and might have turned attention more to events in the academy's environment, but it would later have been taken as a sign of intransigent conservatism and made the AdW inacceptable in the eyes of West *and* the new East German policy makers. A revolutionary response of the lower ranks in the AdW, on the other hand, could have produced the kind of "creative destruction" that might have changed the image of the organization in such a way as to enable it to form a "winning coalition" in its fight for survival. But again, this could have come about only unintentionally, for at the time when the choice between reform and revolution had to be made, nobody could have anticipated the future functionality of a more radical response.

An outside observer could easily have recognized that early in Phase II it was imperative for the AdW to enter the political decision process and to try and influence in particular Western decision makers, and corporate actors to whom they would listen. That the AdW failed to act accordingly cannot only be explained by the attention-absorbing effect of financial problems and the internal reform process, which moreover incapacitated the organizational leadership for a certain time. Misperceptions, too, apparently played a role. Far from merely being simple cognitive mistakes that could just as well have been avoided, almost all of these misperceptions stemmed from strong beliefs and ingrained habits of thinking, such as centralism and "statism," a belief in the future of the GDR, lack of familiarity with federal structures, etc. Given such historically and biographically rooted beliefs and orientations, most of the crucial misperceptions were in fact hard to avoid.

It is questionable, however, whether a correct and timely recognition of the external threat and the countermeasures it called for would have made much of a difference for the coping behavior of the AdW. Its strategic options were *objectively* restricted by previous "choices" of all its component groups. Thus, in order to find support among the relevant decision makers, the AdW needed to project an "attractive" image, but in this it was seriously handicapped by the previous response to the regime change – reform efforts rather than revolution and the immediate and radical displacement of the old AdW elite. But again, this was a "mistake" that appears nearly unavoidable – not only because at the time its later consequences could not be anticipated, but also in view of the *general* normative preference for an orderly and democratic reform process on the one hand, and the low revolutionary potential among the members of the AdW on the other. The AdW, after all, had enjoyed a privileged position in the GDR, and as a consequence of deliberate recruitment policies, the political involvement with and attachment to the socialist regime was relatively high at all ranks of the organization. Having for these reasons acted the way it did in Phase I, the AdW could no longer opt for some of the objectively most promising strategies, including the formation of a strong defensive coalition with the new East German political leaders, in Phase II.

Aside from confirming the propositions about the sequential nature of coping, where past decisions influence present options, the analysis serves to throw doubt on the assumption of deliberate strategic choice. On all levels of the AdW, there was apparently relatively little conscious *choice* among alternative strategies; the actors rather did what they thought the situation (as they saw it) called for – they made what seemed to them the one correct response. This is true of the reform activities on all levels of the AdW in Phase I, and it is also true of the different reactions to the recognized survival threat in Phase II. There were misperceptions, and wrong strategies were enacted, but at each given moment, there were no *obvious* alternatives to what was perceived and done. Thus, in identifying the *best available strategy*, it is not enough to take into account the (limited) action potential; the action orientation, the beliefs and values of an actor are equally important restrictions, first for what he will perceive, and subsequently for his coping response. Even in critical situations actors often do not survey alternatives, try to anticipate their future consequences, and calculate costs and benefits, but they rather enact what they feel is the "right" response. Their "mistakes" in these cases are such only in hindsight, and if looked at from the outside.

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German Unification as a Steamroller? The Institutes of the Academy of Sciences of the GDR in the Period of Transformation

Hans-Georg Wolf

1 Introduction

One of the results of the unification of East and West Germany was the dissolution of the East German Academy of Sciences (*Akademie der Wissenschaften*, AdW). The fate of this organization can be examined on different analytical levels. According to the general logic effective during the process of unification, the AdW as a whole fell victim to the "transfer of institutions" (Lehmbruch 1993) from West to East Germany. Mayntz's analysis (1992; and in this volume) concentrates on this analytical level. The fate of the AdW set certain conditions for the development of its single research laboratories. However, the paths on which these institutes developed were quite dissimilar, resulting in a broad range of organizational "success" and "failure." This chapter employs the concept of "Coping with Trouble" for an examination of these different paths, adopting the perspective of the institutes as focal organizations to analyze their coping with the "trouble of unification."[1]

After a brief description of the AdW and the transformation process (1), the extent and character of "trouble" will be discussed (2). The changes connected with the revolution in the German Democratic Republic (GDR) and the unification will be interpreted as an abrupt and extraordinarily profound

I am very grateful to Uwe Schimank, Andreas Stucke, Jochen Gläser and Grit Laudel for their helpful comments and to all my interview partners for their kind cooperation.

1 The article will not be concerned with the fact that the institutes had to already cope with trouble during the years of the GDR; nor will it take into account that unification also brought them various kinds of *relief from trouble.*

change in the organizational environment of the institutes of the AdW, which was aggravated by far-reaching changes within these institutes. Nevertheless, I will argue (3) that a certain scope of action was available to them. How they actually tried to cope with the menace will be illustrated by the example of three institutes. Referring to a metaphor used by various commentators, I will conclude (4) that it is not appropriate to depict the transformation process as a "steamroller," as an externally induced catastrophe which did not leave the affected institutes with any coping opportunities. Rather, differentiation is necessary: Coping behavior did have a significant impact on the organizational fate, albeit only in a subgroup of institutes.

1.1 The AdW within the Research System of the GDR

Comprising a society of scholars and 60 autonomous research laboratories with 18,285 R&D employees in 1989 (cf. Stifterverband 1990: 70), the AdW employed the largest percentage of East German non-industrial R&D personnel. Its inner structure was strictly hierarchical. Its Presidium had executive authority over both the society of scholars, subdivided into "classes," and the research institutions, subdivided into "research departments" (*Forschungsbereiche*[2]). Internally, most of the institutes consisted of several hierarchical levels. They were subdivided into departments (*Bereiche*) which in turn consisted of smaller sections (*Abteilungen* and *Arbeitsgruppen*).

Very often the AdW was perceived as the principal center for fundamental research of the GDR, and that is how it presented itself in official texts (cf. Academy of Sciences/ UNESCO 1985: 41; AdW 1987: 136). This seems to be the reason why, during the unification period, the AdW as a whole was frequently associated with West Germany's Max Planck Society (MPG), as if it were the MPG's East German counterpart in basic research (see among others: Terpe, cited in: *Berliner Zeitung* 1990; Püttner 1992). However, it seems that, at least during the academy's final years, the share of basic research diminished significantly. In accordance with the "primacy of economy

2 From June 1989 on, the *Forschungsbereiche* were labelled *Wissenschaftsgebiete* (see Wangermann 1990). They included: physics; mathematics and informatics; geography, geology, and space research; chemistry; biology and medicine; social sciences. In the GDR as in other socialist states, the label "social sciences" also included the humanities.

over science and technology," a basic tenet of the SED's science policy (cf. Lauterbach 1976: 26), the political leadership launched several initiatives to push the AdW in the direction of applied research. From 1985 on, the AdW was obliged to secure at least 50% of its research funding from industry. Most of the institutes have developed intensive relations to state-owned industrial conglomerates known as combines (*Kombinate*). Many of the institutes performed tasks which in Western countries would be considered typical industrial R&D.[3] It was certainly inappropriate, therefore, to identify the AdW with the MPG. Although quite a number of research groups performing pure basic research did exist, the majority of them was nearer to applied research than to basic research. In spite of numerous difficulties, according to many observers (see for instance the statement of the President of the West German Science Council in *Der Spiegel* 1991: 40) several disciplines of the AdW performed outstandingly well, in fundamental and in applied research.

1.2 The AdW and the Process of German Unification

In a brief, general overview of the transformation process since autumn of 1989, three phases can be distinguished analytically.

The *first period,* beginning with the peaceful revolution, was one of *inner transformations* within the AdW. In most of the institutes, new councils for codetermination were created (as a rule by democratic elections). On the one hand, so-called *Personalräte* (personnel councils) were put up. Representing the researchers as well as the staff of the institutes, these councils took the place of the *Gewerkschaftsleitungen* (the committees of the GDR's trade union on the level of the institutes; cf. Gläser 1992: 39). On the other hand, *Wissenschaftliche Räte* (scientific councils) were established.[4] Reserved for the scientific personnel and members of the management, they served as advisory bodies to the director and had a say especially in decisions on the scientific

3 For instance, a member of the Science Council called the three large chemistry institutes in Berlin-Adlershof the "national centers of chemical industrial research of the GDR" (interview dhw012193; translation by the author).

4 In some cases, these councils already existed before the revolution, but had no influence on the development of their institutes (cf. Gläser 1992: 39).

orientation and strategy.[5] In many cases, the influence of these councils within the institutes was rather large. However, since in this phase virtually no standardized rules existed, the actual distribution of power between personnel council, scientific council and director differed from one institute to another.

The leadership members of the institutes had to submit themselves to a confidence vote. By summer of 1990, about half of the directors of the institutes had been replaced (cf. Mayntz in this volume). The structure of the institutes was partly reorganized in some cases, as were the research agendas. But, without doubt, the period of inner reforms was too short for a fundamental redirection of the research; the same holds for major organizational changes. This phase was also marked by a significant weakening of hierarchical control between the higher organizational levels of the academy and the institutes.

During this first phase, most of the actors perceived the situation as one of changes going on within a sovereign national state, the GDR. However, this perception was gradually replaced by the anticipation of a quick unification. This anticipation became dominant at the latest when the conservative party alliance won the East German elections to the parliament (*Volkskammer*) on 18 March 1990. This was the beginning of the *second period* considered here, the period of *strategy formation.*

As it gradually became apparent to them that unification was imminent, the members of the institutes of the AdW realized that they had to prepare themselves for changing conditions in their environment. However, during the first half of 1990, the actual form this change might take remained uncertain. While a reduction of the academy's research personnel could be foreseen – and in fact the AdW itself started such reductions soon after the revolution – it was neither clear whether the academy as an association of research institutions would be sustained nor which public agency would be responsible for them (cf. Mayntz in this volume). The extent of the threat to the institutes remained unclear until the first days of July 1990, when one of two basic decisions on the future of the AdW was fixed: The West German Science Council[6] was officially engaged to evaluate the institutes and to give recom-

5 Gläser (1992: 39) gives an example of the task definition of a scientific council.

6 The Science Council, made up of officials from the federal and *Länder* governments and professors from various fields, advises the public authorities on higher education and research policy (see Krull 1992).

mendations about their worthiness of public support and financing. The Science Council started the evaluation procedure by sending the institutes an extensive questionnaire,[7] which was to be answered by the end of August. The other basic decision followed on 31 August 1990 (i.e. after the deadline of the questionnaire) when the Unification Treaty was signed. According to this treaty, the AdW as a combination of scholarly society and research association was to be dissolved. The newly created East German federal states (*Länder*) were to become responsible for the institutes of the academy situated in their respective territories. The temporary financing of these institutes was ensured up to the end of 1991.[8] The treaty also made clear that the East German research institutions were to be adapted to the "well-established methods and programs of research promotion" used in the Federal Republic (Art. 38; translation by the author).

Thus, from the end of August 1990 on, the future path of the institutes could be seen more clearly. Since the maintenance of the AdW's association of research laboratories as an autonomous research organization (like the MPG) was ruled out, each institute knew that its only options for the future were integration into the established West German structures or complete dissolution. More specifically, this meant that the scientific potential of the AdW was to be integrated into laboratories of the big extrauniversity research organizations (MPG and Fraunhofer Society, FhG[9]) or of other state-financed research institutions (Big Science Centers, institutes of the so-called *Blaue Liste*[10] and institutes financed directly by the federal government or by individual *Länder*). The only other possibility was integration into the university system or into the field of private R&D.

The beginning of the evaluation in September 1990 marked the start of the *third period* of the transformation process examined here, the process of *evaluation and implementation.* An Evaluation Committee and nine expert

7 Its 23 questions concerned the past research activities of the institutes and their ideas for their future research orientation.

8 Although this notion does not correspond exactly to the juridical facts, this part of the unification treaty has been called a "moratorium."

9 In contrast to the MPG, the FhG concentrates on application-oriented research.

10 The institutes of the *Blaue Liste* (blue list) are jointly funded by the federal government and the *Länder*.

groups were established by the Science Council.[11] These groups performed the main part of the evaluation procedure. They looked through the answers to the questionnaire, visited the institutes (between the end of September 1990 and February 1991), talked to the scientists employed there and tried to get an idea of the quality of their research work. The results were discussed within the expert groups and, later, in the Evaluation Committee. Finally, between January and July 1991, the General Assembly of the Science Council passed its recommendations, which were crucial for the future of the research laboratories of the AdW.[12]

Altogether, the Science Council recommended the foundation of about 100 new research institutes and branches of existing West German institutions in East Germany which were to integrate personnel from the AdW. These institutions employ approximately 7,500 people.[13] The number of the scientists and other employees of the research laboratories of the AdW had declined to 15,000 by September 1991;[14] therefore, according to the recommendations of the Science Council, roughly every second employee of the academy had a chance to get one of the new positions. Moreover, the Science Council proposed to transfer some 2,000 positions from the extrauniversity to the university sector. For this purpose, a special program financed jointly by the federal government and the new *Länder* was created – the *Wissenschaftler-Integrationsprogramm* (WIP). As to the types of research institutions suggested by the Science Council, there are only a few deviations from the established West German repertoire; in general, the established institutional forms of research organization and funding were reproduced.

11 The vast majority of the evaluators came from West Germany, but professors from East Germany and from abroad took part in each group.

12 The Science Council evaluated some 130 East German research institutions, including all the extrauniversity research establishments. From the late 1970s up to 1990, the Science Council had only conducted some 40 evaluations in the Federal Republic of Germany (Krull 1992: 14); only in one case did these evaluations lead to the closure of an institute (Block/ Krull 1990: 435). This shows how extremely the evaluation of the East German institutes differed – in quantity *and* in quality – from earlier evaluations in the west.

13 Calculation by the author; derived from Wissenschaftsrat (1992).

14 This number is taken from documents of the *Koordinierungs- und Abwicklungsstelle* (KAI-AdW), a temporary agency set up for the purpose of controlling and coordinating the transformation of the AdW.

The employees of the institutes of the AdW had to find their way into this pattern of publicly funded institutions. Although the number of recommended positions sufficed for roughly half of the personnel, this half was not distributed evenly among the new institutions. Rather, with regard to the future of the 60 research establishments of the AdW, the recommendations of the Science Council embraced a broad range of different types of "organizational fate." Firstly,[15] there were six institutes whose *winding-up*[16] was recommended by the Science Council without providing any substitute worth mentioning (see Case Study 3 for an example). The vast majority of their personnel was not designated for further public support. Secondly, 28 institutes were intended to be *broken down* into smaller units, some of which were to receive public funds, others of which were not. A moderate percentage of the personnel of most of the institutes in this large group was to remain within the system of publicly funded research (in some cases, however, a rather high percentage could remain: cf. Case Study 1). The same holds for the third category, i.e. the five institutes that were to be completely integrated into existing West German research establishments. Fourthly, 21 institutes were recommended to be *converted* into newly founded research establishments. Their organizational integrity was to be maintained, but their organizational form was to be adapted to the established West German pattern. It was in this category that the highest general percentage of personnel was rehired. Nevertheless, in many cases the organizational conversion entailed a noticeable reduction in personnel (cf. Case Study 2). At the most, about ten institutes of the AdW were to be maintained without any significant reductions in personnel.

By and large, the recommendations of the Science Council have been carried out. Although numerous implementation problems did – and still do – exist and some recommendations were never realized, at least the aggregated flows of personnel from the former AdW to new extrauniversity institutes roughly correspond to the proposed numbers.

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15 The distinction between these different types can only be an analytical one – reality was, of course, less clear-cut.

16 The German word is *Abwicklung*, meaning liquidation or dissolution (see Young 1993, Footnote 4).

2 The Problems Facing the Institutes of the AdW

2.1 Affected Interests of the Institutes and Individual Researchers

If one wants to analyze which aspects of the unification process *troubled* the institutes, it is not just trivial to first consider their specific interests. *Firstly,* on the level of *corporate actors,* one can draw on the concept of reflexive interests which organizations pursue regardless of their specific functions. To put it briefly, these include "organizational survival, autonomy and growth" (Scharpf 1989: 45; Schimank 1992: 175 and Weyer 1993: 14-16 argue similarly). Applied to the institutes of the AdW, within the socialist state these interests were met to varying degrees. After a profound reorganization of the academy in the early 1970s (the *Akademiereform*), major organizational restructuring did not occur too frequently. Thus, the survival of the institutes was generally ensured, albeit on a resource level that often did not satisfy their demands. Most of the institutes also grew considerably after the reform. Their organizational autonomy, however, was narrowly limited.

Can the concept of reflexive interests also define the interests of the institutes of the AdW during the unification process? Quite remarkably, the fundamental interest of organizational *survival* was not shared by all of the institutes.[17] In their answers to the Science Council, approximately one quarter of the institutes did not express an unambiguous will to survive as intact organizations. Instead, they presented concepts for their disintegration into smaller units (see Case Study 2 for an example). Maybe this behavior was partly due to the fact that the institutes had no hope of finding a practicable way to survive within a unified Germany. Perhaps a more important reason can be found, however, in their organizational past: The *Akademiereform* had generated very heterogeneous research institutions. The individual character of the formerly autonomous institutes remained, and cooperation among them often did not work well.[18] Consequently, as soon as the change in the politi-

17 This seems contrary to basic tenets of organization theory; cf. Hage (1980: 425): "Any theory about the functioning of organization in an environmental context must start with the simple assumption that the organization's dominant coalition is interested in some kind of survival."

18 This observation is documented in many evaluation reports of the Science Council (see, for example, Wissenschaftsrat 1992: 94).

cal system gave them the chance, a number of subunits within the institutes made an effort to regain their organizational autonomy. In these cases, the concept of reflexive interests can be applied, but only on the level of organizational subunits which followed their individual interest in *survival.*

In an abstract sense, both of the other reflexive interests were probably pursued by all the institutes of the AdW. But some restrictions applied from the very beginning, compelling the institutes to lower their aspiration level. As to the goal of organizational *autonomy,* the dependence of research institutes upon public funds must be stressed (cf. among others: Mukerji 1989: 4). Since the possibility of transforming parts of the institutes into private enterprises was narrowly limited by the difficult economic situation, it was obvious that autonomy could only be achieved within a larger organizational setting (such as the MPG or the FhG). A limitation on autonomy would always exist, however, varying considerably among different institutional types.

The goal of organizational *growth* was obviously out of reach for virtually all the institutes. Since most critical examinations of the research laboratories of the AdW concluded that they were overstaffed (by West German standards), the institutes could only endeavor to maintain their size or to minimize its reduction.

Secondly, the interests of the *individual employees* were not necessarily identical with the interests of the institutes they were employed in. Since it seems to be even more difficult to define the basic (reflexive) interests individual actors pursue in any given situation (cf. Scharpf 1989: 45), I would like to base my argument upon the following simplifying assumptions for now: In the first place, the individual actors were interested in securing their regular income. In the second place, they were interested in a job that matched their personal qualifications and preferences (with regard to researchers: one that allowed them to follow their personal research interests). Sometimes these interests corresponded with the interests of the institutes, sometimes they did not (as illustrated in the case studies in Section 3).[19]

19 This relates to the interplay between individual and corporate actors within multilevel systems discussed in many contributions to this volume (see Braun; Schimank; Stucke).

2.2 Why Were These Interests Threatened?

My proposition is that the trouble which threatened the interests described above arose mainly in the second and third phase of the transformation process; the existential problems began during the advent of unification. The internal transformation after the revolution undoubtedly caused many problems for the institutes of the AdW. The process of democratization entailed disorder and heated discussions about questions of political involvement, thus impairing the research conditions within the institutes. Moreover, their economic situation was getting more and more precarious because most of the funds from industrial partners were being withdrawn. On the other hand, the budget of the academy was not seriously endangered in 1990 and 1991. Several months before the unification, the West German Ministry for Research and Technology (*Bundesministerium für Forschung und Technologie*, BMFT) already began to subsidize the AdW. Not a single institute of the AdW was closed before the end of 1991 (in fact, three institutes were founded during that period). Also, dismissals against the will of the employees were rare during the first phase. In most cases, those who did leave had found other employment or gone into retirement. In the second and third phases, however, the survival of the whole academy and its institutes as well as the jobs of all the employees became uncertain.

Concentrating on the second and third phases, how can we describe the "trouble" the institutes had to cope with? I will distinguish between the external and internal side of trouble.

2.2.1 External Trouble

From the viewpoint of the institutes, the unification process can be conceptualized as environmental variation. Organization theory (cf. among others: Child 1972; Hannan/ Freeman 1977) provides various concepts of environmental variation, which – as Wholey and Brittain (1989: 869) have demonstrated – have three dimensions in common: *frequency, amplitude* and *predictability* of environmental change. The transformation considered here can be modeled as a single – and unique[20] – event, converting the institutional

20 Lehmbruch (1990: 464-467) gives a skeptical answer to the question whether there are historic precedents that could be called upon to analyze Germany's unification.

system of the GDR into that of unified Germany. As this is no matter of long-term analysis, the *frequency* and *predictability* of environmental change (does experience from the past help to anticipate future variation in the environment?) are not relevant measures.

Amplitude, however, seems suitable for demonstrating the great importance of this event. To show that the *amplitude* of the variation was very high, one can – again drawing on different concepts from organization theory (clearly summarized by Sharfman/ Dean 1991) – distinguish three dimensions which describe organizational environments: *Complexity*, *dynamism and stability*, and *resource availability*.

Complexity refers to "the level of complex knowledge that understanding the environment requires" (Sharfman/ Dean 1991: 683). During the years of the GDR, the institutes of the AdW were quite experienced in understanding the opportunities and hazards of their environment which, in this sense, was not very complex. But, from their viewpoint, understanding the emerging environment of unified Germany was an extremely complex task. Since communication between East and West Germany used to be very restricted, East German researchers possessed only limited knowledge about the organization of scientific research in West Germany (and vice versa). Nor were they experienced at undergoing external evaluations. Moreover, mere knowledge about the formal (legal and organizational) structure of the environment often does not suffice. Knowledge about informal structures, relevant actors and network connections within the field of research policy can be even more important. If one also takes into consideration the fast pace of the transformation process, it becomes evident that the situation brought a very high degree of complexity and uncertainty to the institutes – while at the same time very much was at stake.

Similarly, the unification process involved a switch from a comparatively *stable* to a dynamic environment. Though external (mainly: political) disturbances occurred time and again, the environment of the institutes of the AdW in the years of the GDR (after the *Akademiereform*) was stable enough to secure the survival of virtually all of them. In addition, the majority of researchers was employed in the AdW (often in only one institute) from the time they left university until they retired. That is why traditional structures persisted for a very long time within the academy. As many observers have noticed (see, for instance, *Nachrichten* 1991: 810), the research orientation given to an institute by its founder in the 1950s often survived without significant modifications up to the 1980s.[21] A similar stability was no longer possible with the advent of unified Germany. Not only did it become difficult for the institutes to acquire the necessary knowledge about resources, the conditions for getting funds also changed over time or remained uncertain.[22] Thus, research institutes and individual researchers in Eastern Germany were (and still are) forced to adapt to dynamic changes of their environment much more frequently than before.

Finally, the unification process implied a fundamental change with regard to *resource availability*. The difference does not lie primarily in the total amount of available resources, but rather in their structure. Organizations are able to acquire resources to the extent that they are well adapted to the environment which supplies these resources. By and large, the institutes of the AdW were rather well adapted to the environmental conditions of the GDR; they had found suitable niches. Many of them performed highly specialized tasks lying within the immediate economic interest of the GDR; another – relatively small – group[23] had found niches allowing its scientists to concentrate on basic research. Individual researchers displayed a similar degree of specialization. In many cases, a specialization that matched the environmental requirements of the GDR became a mismatch under the circumstances of unified Germany (e.g. Case Study 3).

Additional problems were caused by the breadth of the transformation process. As the institutional upheaval in the course of the unification process simultaneously affected every subsector of the East German research system, the problems multiplied. A sharp decline of the research personnel took place in the sector of industrial R&D, the university sector and in all the other publicly-financed extrauniversity research institutions. Since no safe ground

21 Such persistence of a research orientation over a long time is not, of course, peculiar to the AdW, but has often been described in sociological studies. See, for example, Chubin/ Connolly (1982: 303).

22 For instance, the formal regulations of the WIP were modified several times. Under certain conditions, it was more attractive for an East German scientist to apply for funds from the labor office (which offered a special sponsorship program for them – a part of the job-creation program called *Arbeitsbeschaffungsmaßnahmen*, ABM) than for funds from the WIP. But the legal regulations concerning ABM also changed several times.

23 Among them the Institute of Solid-State Physics and Electron Microscopy and the Institute of High-Energy Physics.

could thus be found within the entire transformation process, deliberate coping behavior became much more difficult.

Environmental conditions changed not only with respect to the structure of available resources, but also with respect to the set of organizations competing for them. Thus, to be formally able to fulfill the requirements for acquiring resources was one thing for the institutes; to hold their own against competing research institutions was quite another. The unification combined two formerly separated populations of organizations: extrauniversity research laboratories in East and West Germany, now both vying for public funds.[24] The restructuring according to the recommendations of the Science Council, however, virtually affected only the Eastern subpopulation. Consequently, the situation was particularly troublesome for East German groups pursuing kinds of research which were also being worked on by established West German institutions. On the other hand, groups that filled gaps within the Western research system (i.e. research topics that were considered promising by the relevant actors, but not covered by West German institutions) found an easier way into the research system of unified Germany. Thus, different degrees of trouble were related to different research topics.[25]

All in all, the preceding section has demonstrated that the amplitude of the environmental variation the institutes had to cope with was extraordinarily high. It even seems appropriate to refer not to an environmental *variation*, but to the sudden *replacement* of one environment by a completely different one.

2.2.2 Internal Trouble

The capacity of the institutes to cope with the troublesome change of their environment depended – among other things – on the situation within these research organizations. In this sense, one can speak of "internal" trouble, or

24 The German federal system does provide, however, for a partial regionalization of available research funds. The funds for extrauniversity research come not only from the federal government, but also from the *Länder* (see Hohn/ Schimank 1990 for a detailed description of the system). Thus, not *all* the research institutions in Germany compete for a piece of the *same* pie.

25 The branches of the humanities most deeply involved in Marxist-Leninist ideology were, of course, in the biggest trouble. This aspect of the problem cannot be treated here.

of the "specific trouble situation" of the single institutes. As described above, from 1989 on, the institutes of the AdW underwent a period of internal transformation. Hannan and Freeman (1984: 159) make a clear prediction regarding the effect of such a transformation on an organization's chance of survival: "Attempts at reorganization increase death rates. Organizations undergoing structural transformation are highly vulnerable to environmental shocks." According to Hannan and Freeman, this is due to the fact that reorganization processes make organizational action unstable and impair "quality and timeliness of collective action" (ibid.).

Research on the stated correlation between transformation and failure, however, has not yielded unequivocal results.[26] And in the case of the institutes of the AdW, a specific dilemma appears: On the one hand, internal stability probably made it easier to cope with the crisis. For instance, it was problematic to vote out a director because of his ideological activities if he was the only member of the scientific staff with experience in managing an institute, or the only one who had good relations to researchers in the West (cf. Raible 1991). A long-lasting period of controversies over political issues or structural reorganization implied the risk of making the subunits of an institute drift apart, thus impairing its capacity for purposeful collective action. On the other hand, the institutes depended on the public acceptance of their legitimation. A positive judgment by the Science Council was hardly conceivable if an institute did not perform a minimum of democratic reforms. Thus, the institutes were forced to walk a tightrope between organizational stability and necessary reforms. In fact, with regard to this decision, the institutes followed a variety of paths.

Since each institute had a history of its own which could not simply be erased, the institutes' starting positions varied considerably. Though the Science Council concentrated on the evaluation of the potential for *future* research available in an institute, it did not discount history when it decided on the preservation or dissolution of a laboratory. The shadow of an institute's past was often an important part of the trouble it now had to cope with. For

26 See Baum/ Oliver (1991) for a discussion. Haveman (1992: 49) posits in her study of the Californian savings and loan industry that organizational change may prove beneficial particularly in situations of "dramatic environmental shifts that threaten the organizational form with extinction ... ."

example, some institutes were handicapped in the evaluation procedure because their leading members, having been very active within the political system of the GDR, had pushed the research program of their institute into a direction which fulfilled economic requirements of the GDR, but which did not prove to be promising in terms of scientific progress.

Up to this point, the trouble for the institutes has been referred to as "internal trouble," although, from the perspective of the individual researchers, this was still "external trouble." What troubled an institute did not necessarily trouble its employees, and vice versa. A scientist did not have to care about the difficult situation his institute was in if an attractive exit option was available to him. Conversely, the chance of survival of an institute declined if too many qualified researchers turned their backs on it. Thus, the internal trouble situation of an institute resulted from a permanent interplay of individual and collective decisions; in some cases, the coping behavior of one level created trouble for the other level.

The preceding sections have demonstrated that the incorporation of East Germany into the West German institutional system represented a case of extreme external trouble to the institutes and to their members, a complete exchange of the organizational environment relevant to them. Since most of the institutes were undergoing critical intraorganizational transitions at the same time, the external trouble was often aggravated by internal trouble. In the following sections I will discuss the scope of action left to the institutes faced with this troublesome situation.

3 Coping with a Steamroller?

The concept of "Coping with Trouble" can only be of analytical value in examining situations in which the affected actors can be assumed to have at least a minimum of coping opportunities. Otherwise, it seems more adequate to speak of an environmental "catastrophe," to which the notion of "coping" cannot apply: People suffer through catastrophes, rather than deliberately coping with them.

Clearly, the transformation process considered here was perceived as this kind of catastrophe by quite a number of observers. One metaphor frequently used to describe the process illustrates this perception: "bulldozing" (in German: *plattmachen*, cf.: Etzold 1990; Peche 1990; Weber 1991). For Maier (1991), the East German research system was run over by a "merciless steamroller" steered by West German drivers.[27] According to this point of view, East Germany's research system underwent a procedure of evaluation and restructuring completely under external control. In the following two subsections I will try to demonstrate that this view overly simplifies matters and that at least some of the institutes did have a chance to cope with their trouble, instead of simply having to suffer it.

3.1 Perception of the Trouble Situation and Hypothetical Coping Opportunities

The first question to be answered is how the actors within the institutes perceived their own coping opportunities. In interviews with former employees of 12 institutes of the AdW,[28] I asked if, at the beginning of the evaluation phase, they had thought there was a significant scope of action for them. Without exception they affirmed that they had; they had believed that their own actions would have an impact on the final outcome of the process. In retrospect (more than one year later), roughly 50% of the interview partners saw things differently. Looking back, they could not recognize any opportunity for their institutes to influence their organizational fate.[29] Regardless of

27 Translation by the author; the English word is employed in the context of German unification by Dickman 1990.

28 The sample cannot be representative for the whole academy, since I did not take account of institutes in the social sciences.

29 Probably this result can be partly explained by mechanisms of "cognitive dissonance" (Festinger 1957). Strikingly, most of the institutes to which those interview partners belonged who, in retrospect, did not see any scope of action had emerged from the transformation in relatively bad shape. In contrast, most of the institutes whose members said they were able to influence the process had performed rather successfully. It seems reasonable to assume that in the former case the interviewees had a psychological need to blame the circumstances for the unwelcome outcome, while in the latter there was no reason for them to shift away the responsibility for the positive outcome (Kaufman, 1991: 69, describes exactly this mechanism). However, this potential distortion does not affect the crucial point that during the evaluation the members of the institutes assumed some coping opportunities were available.

that change of opinion, this result indicates that during the evaluation period the institutes as corporate actors perceived the situation as being trouble rather than a catastrophe. It also supports the assumption that they tried to make use of their coping opportunities.

Additional support for the assumption that a certain scope of action was available to the institutes can be drawn from statements made by experts involved in conducting the Science Council's evaluation (interviews dhw012193; dhw011493; dhw100692; dhw050393; Gabriel, cited in: *Physikalische Blätter* 1991). In particular, they stressed that the institutes' proposals regarding their institutional future were important, and that a sound proposal was likely to have a positive impact on the judgment of the evaluation committee.

Starting from the assumption of a certain range of action opportunities on the part of the institutes, the next question is: What hypothetical types of action can be thought of? Three types of coping strategies will be proposed.

Firstly, the institutes could try to improve their position by means of *networking*.[30] Their transformation took place within a network of various actors; it was socially embedded (Granovetter 1985). Thus, what the institutes had to do was arrange their external relations in a way favorable to the evaluation process. They had to find supporters among the relevant actors in their environment.[31] The latter included corporate as well as individual actors. On the one hand, since the crucial decision on the future of the institutes was taken by the expert groups and the Evaluation Committee of the Science Council, a process of "peer review" was central to the decision-making process. One important measure, therefore, was to acquire support from influential scientific peers within or outside the Science Council, or to reinforce such support if it already existed. On the other hand, the decision was also heavily influenced by corporate actors. Each big West German research organization and each authority on the federal or *Länder* level which took part in the evaluation followed its own strategy. Thus, an institute or research group not only needed a positive scientific evaluation, but also depended on sufficient support

30 The notion of *networking* refers here to the actor's efforts to find support within existing networks; the systematic *construction* of a network by the focal actor is not necessarily implied here (cf. also Musselin/ Vilkas in this volume).

31 Baum and Oliver (1991) have demonstrated that institutional linkages significantly decrease the likelihood of organizational mortality.

among these corporate actors. Hence, it was well advised to try to get in close contact with these organizations, too.[32]

Secondly, the institutes could try to employ the strategy of deliberate *niche selection*. Within certain limits, the transformation process offered them the chance to select the type of environment in which they would operate.[33] To achieve this, they had to scan the emerging "research landscape" in their discipline for research topics which seemed promising but were not being worked on by too many competing research groups – they had to find a research niche with sufficient available resources. Of course, this also had to be a research program the institute could credibly claim to be able to conduct with some success. The institutes which were not forced to change their research orientation to fill a gap in the emerging research landscape were in the best position.[34] For the other institutes, there was a limit to the extent of reorientation they could realistically perform.[35]

Thirdly, the institutes could engage in *impression management*.[36] Irrespective of their actual scientific quality or of the actual public demand for their work, they could try to represent both as positively as possible. Among the possibilities for the intentional use of impression management were the careful formulation of the answers to the Science Council's questionnaire and the thorough preparation for the expert group's on-site inspection of the Science Council.

Clearly, with regard to all these strategic opportunities, strong limitations applied. In order to build up social support, the researchers had to possess

32 This was complicated by the fact that the administrations of the East German *Länder* were just being established in autumn 1990. For quite some time, reunified Berlin was the only *Land* possessing a functioning R&D administration.

33 For this mechanism of purposeful niche selection see, for instance, Child (1972); Hage (1977); Sharfman/ Dean (1991).

34 The positive development of the institute specialized on research on the Sorb ethnic minority may illustrate this; it was the only institute in the humanities which was to be converted into a successor institute. Another example is the institute for research on vertebrate animals linked with the East Berlin zoo.

35 See Case Study 3 for an illustration of this problem; with regard to a different type of trouble, see also Gläser et al. in this volume.

36 See Goffman (1987: 207-222); Schlenker (1980); Chatman/ Bell/ Staw (1986) for more information about this concept. Schlenker (1980: 6) defines it as "the conscious or unconscious attempt to control images that are projected in real or imagined social interactions."

sufficient information about central actors and their relations. The ability to gain such knowledge by travelling to capitalist countries had been reserved to a small subgroup (the so-called *Reisekader*) before the borders had opened up. On the other hand, in spite of the high pace of the transformation process, roughly six months remained in which the institutes could improve their relations to other actors. Thus, even the scientists once classified as politically "unreliable" and excluded from trips to the West now had the opportunity to make up for lost time: They had a chance to act.

The same information problem existed with respect to the other strategies described above. An extensive amount of information about the relevant environment was necessary to define a research program and an organizational form promising sufficient resource flows in the future. Successful impression management required sufficient information about the criteria and motives that guided the evaluation procedure. But again I want to state that the transformation process lasted long enough to allow for learning processes. As will be shown below (particularly in Case Study 2), the evaluation was not a one-shot situation, but an iterative process with actions and reactions on the part of both the evaluators and the evaluated laboratories.

As I mentioned above, the institutes of the AdW started from very different positions. Each of them had a different past and found itself in a different internal state. What seems particularly important is that the institutes also differed with regard to the extent of their recognition outside of the GDR. Some of them were already acknowledged as part of the international scientific community before the revolution, while many others were quite isolated from it. Thompson (1967: 33) stresses the importance of *prestige* as a means for organizations to acquire the necessary support from the environment (and to reduce dependence on environmental elements). Of course, the prestige of a scientific institution crucially depends on the prestige of its leading scientists. Thus, it could be considered an important advantage in the transformation of an institute of the AdW if it had one or several outstanding scientists among its personnel.

This leads back to the question of different actor levels. To simplify matters in the discussion of hypothetical action opportunities above, I only dealt with institutes as actors. Nevertheless, while these strategies are conceivable elements of the *corporate* action of the institutes, they were always carried out by *individual* actors. The impression an expert group acquired from its inspection of an institute was composed of numerous single impressions from discussions with the individual researchers.[37] Similarly, the strategies of niche selection and networking were available to individual researchers as well as to research groups and institutes. Thus, the organizational fate of an institute depended on the interplay of these different levels.

As this section has shown, it is indispensable to study the particular cases of single institutes if one aims at identifying their coping opportunities. Before doing so in the following section, I would like to add a last remark concerning the different actor levels. One might assume that the organizational levels above the institutes played an important part in the transformation process, but this was not the case. Rather, these actor levels had only marginal impact on the fate of the institutes.

The Presidium of the AdW did make its "fruitless coping efforts" (see Mayntz in this volume), trying to find a suitable niche for the academy as a whole. However, since the major goal of the Presidium was to preserve the community of institutes in toto, it did not concentrate on the task of integrating the single laboratories into the research landscape of unified Germany. Moreover, it took the top level of the academy a long time to cope with its own democratization and reorganization. Thus, when the institutes faced the difficult task of coping with the trouble of unification, they did not receive much help from the top level of the academy.

A somewhat more active part was played by the *Forschungsgemeinschaft*[38] and the management of the research departments. From May 1990 on, the board of the *Forschungsgemeinschaft* discussed concepts for the future of some institutes, particularly proposals for Big Science Centers at the major research sites. It also initiated a self-evaluation of the institutes (completed by June 1990), including the formulation of proposals regarding their future as research institutes.[39] However, these activities had only limited impact on the final outcome of the transformation process.

The research departments of the AdW served as a forum for discussing the future of the institutes. However, the activities of the various disciplines

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37 The expert groups of the Science Council usually attached great importance to individual conversations with the scientific personnel at all levels, not just with the leading figures (cf. Raible 1991).

38 The *Forschungsgemeinschaft* was the association of the research laboratories of the AdW founded after the revolution (see Mayntz in this volume).

39 This evaluation is documented in a volume containing self-portrayals and conceptual considerations of all the institutes (AdW 1990).

seem to have differed considerably. As a former employee of one research department reported (interview dhw031293), the department of mathematics and informatics very soon gave up all efforts to coordinate the activities of its institutes, while the physics department managed to keep up regular meetings of the directors of the institutes until September 1990.

Thus, while the institutes within some disciplines received limited support from the management of their research department, in general the institutes were left to their own devices. Finally, with the date of the unification, the upper levels of the AdW (except the society of scholars) were dissolved. From then on, even the hypothetical opportunity of support and coordination from above was dropped – but the evaluation was still in process. Virtually all my interview partners from the institutes stated that during the crucial phase of the restructuring they did not obtain any support worth mentioning from the higher levels of the AdW.

3.2 Different Organizational Fates – Three Case Studies

As I argued above, the starting positions and the environmental constraints differed considerably among the institutes. I will try to illustrate this point and describe actual coping activities by examining three institutes devoted to the natural sciences.[40]

3.2.1 Case Study 1: A Comparatively Unproblematic Transformation

Institute 1 was a chemical research laboratory founded in 1949. It was one of the traditional institutes of the academy, with 40 years of research experience in a subfield of organic chemistry and physics. With approximately 400 employees, it was medium-sized compared to other institutes of the AdW; according to a member of the expert evaluation group, it was not as heterogeneous as the largest institutes. Its research tended to be application-oriented,

40 The actual names of these institutes are not relevant to the purpose of this article. The information contained in this section stems from published and unpublished documents and from interviews with members of the institutes and external actors (Science Council, agencies at the federal and *Länder* level). To preserve the respondents' anonymity, no reference to single interviews will be given.

with about 50% of the research capacity being linked to industrial clients. Nevertheless, Institute 1 had also acquired a considerable reputation in certain fields of basic research. In one of its principal research topics it even possessed expertise which – as members of the expert group stated – was hardly available in the Federal Republic. During most of its 40 years, Institute 1 was led by highly reputable scientists. Thus, it had also acquired a certain degree of renown in Western countries.

For all these reasons, one can assume that the "trouble situation" for Institute 1 was relatively moderate from the very beginning. However, its organizational past did include some more problematic points. In the second half of the 1980s, political pressure on the institute increased. The appointment of the scientist who directed the institute from 1985 until 1990 was exclusively politically motivated,[41] as were the appointments of other leading scientists and administrators.

As soon as the opportunity arose after the political changes in 1989, however, members of Institute 1 began to redress the results of that politicization. Already in November, a committee was founded and charged with exploring the appointment policy within the institute during the preceding ten years. A senior scientist who had directed the institute for more than ten years until the early 1980s was nominated as chairman of this committee. He had an excellent professional reputation and quite a number of international contacts, including many in Western countries. As he himself stated, these contacts had led to conflicts with his superiors, who succeeded in forcing him to retire in 1985.

In December 1989 votes of confidence were held in the institute. No members of the directorate were confirmed; they all had to resign from their posts. In a democratic election, a scientific council *(Wissenschaftlicher Rat)* was nominated. The senior scientist mentioned above was elected its chairman; shortly thereafter, he was appointed to be the new director of the institute. In February 1990, he was confirmed as acting director by the Presidium of the AdW. By spring of 1990, most of the other managerial positions were newly filled.

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41 At least this was the judgment of a member of the expert group, who claimed quite drastically that the appointed director did not know anything about the research topic of the institute.

Thus, Institute 1 managed to complete its internal reorganization in a comparatively short time. While this could not be achieved without intra-organizational conflicts, they were not as severe as those in other institutes; centrifugal tendencies and egoistic policies of different research groups remained within limits. This rendered the institute capable of organized action at a time when many other institutes were still struggling with internal trouble.

It seems that the reinstatement of the former director was particularly helpful for the further development of the institute. He combined scientific eminence with a reputation for having been "persona non grata" in the eyes of the old political system. As members of the expert group stressed, the advantageous effect of such a personality supporting an institute should not be underestimated. It is very likely to enhance an organization's prestige – thus the choice of the new (and former) director can be considered an element of Institute 1's successful impression management. Moreover, since the institute was able to profit from the director's experience and established relations to West German actors, it had an advantage in niche selection and networking, too.

In February 1990, several working teams began to develop the future scientific strategy of Institute 1. By April, a proposal had been drafted. It is not surprising that the strategy regarding the future organizational form of the institute was not extremely precise at this time. However, the paper included an explicit reference to deficits in West Germany in a subfield of chemistry in which Institute 1 was specialized. Thus it is evident that, although rapid unification was not yet certain at that time, members of the institute recognized the need to find niches within the research landscape of the unified country.

Immediately after the opening of the frontier, Institute 1 began to intensify its contacts with Western actors. Among other things, it invited several of the leading West German chemists working in its field to colloquiums and took part in conferences organized by the East and West German research ministries and by the professional association of chemists. It also launched several joint research projects with West German research institutions. Furthermore, a process of personnel reduction began. Between summer of 1990 and September 1991 the number of employees declined by 25%. Although this was one of the highest decline rates within the chemistry department of the AdW, several affected persons confirmed that the social costs of the cutback had in fact been kept low (by means of early retirement, etc.).

A first important step concerning the organizational future of Institute 1 was taken by the FhG. "Rather spontaneously," as a leading member of the institute said, the FhG made contact with the institute. By July 1990, the FhG and Institute 1 agreed that the integration of parts of the institute into the FhG was conceivable. Institute 1 also tried to get in touch with the MPG, but at that time prospects for a partial takeover of Institute 1 by the MPG did not look as positive. A member of the institute's directorate reported that the MPG took a rather negative stance at first because one of its established institutes in West Germany was specialized in the same field as Institute 1.

Roughly, this was the state of affairs when the institute was asked by the Science Council to respond to its questionnaire. It is evident from that response that the institute's plans regarding its future organizational form still remained rather indefinite. Like 30% of the total population of institutes, Institute 1's most preferred option was continuation within a *Forschungsgemeinschaft* of the AdW. *In retrospect,* this option seems as difficult to implement as the second option the institute specified, namely its conversion as a whole into a research laboratory of the *Land* in which it was located. On the other hand, the institute gave as a third alternative its disintegration.[42] It reported the FhG's interest in taking over about a quarter of its employees. Institute 1 also reported another plan for a large share of its personnel: becoming a Max Planck institute. It stated that this plan was to be worked out in September in cooperation with the Max Planck institute in West Germany specialized in the same field.

This coordination actually took place. In the late summer of 1990, a first draft of a proposal for a new Max Planck institute was composed.[43] The proposal explicitly conceptualized the new institute as a complement to the West German one, with a different research orientation than had originally been suggested in April. Thus, Institute 1 received active support in the process of niche selection (support that came from a potential competitor for resources!). At the same time, the plans of the FhG became more definite; now an autonomous institute plus a small branch of a West German Fraunhofer institute were planned. Moreover, a West German Big Science Center

42 Obviously, Institute 1 doubted its ability to survive as an organizational unit.

43 Though research groups from other institutes took part in this process, the most prominent role was played by the director of Institute 1 in cooperation with the director of the West German Max Planck institute.

also expressed its interest in taking over a part of Institute 1. Different organizational solutions had begun to emerge for the individual research groups.

Thus, by the time the expert group of the Science Council visited the institute in December, the prospects of the institute had changed considerably. I consider it an important advantage for the institute that various feasible solutions were on the table at the time of the on-site inspection. On the one hand, this made the task of the evaluators easier. They were not forced to spend too much energy on thinking about practicable institutional options, but met quite a number of scientists whose future was already traced out. On the other hand, the promising state of affairs had a positive effect on the self-esteem of the scientists being evaluated.[44] One can assume that the task of impression management was easier to fulfill under these circumstances.

Members of the institute stated they were satisfied with the course of the inspection. Half a year went by before the Science Council finally decided on its recommendations with regard to the chemical research institutes. During that time period, a kind of "interplay"[45] between the expert group (particularly its chairman) and Institute 1 took place. The proposals for the successor organizations became more and more substantial. The FhG and the Big Science Center made binding positive decisions regarding the respective takeovers even before the Science Council gave its recommendation. Shortly before the final debate in the Science Council, Institute 1 submitted an additional proposal for a small research institute financed by the *Land* (the chairman of the expert group had even encouraged the institute to do so).

In its recommendation, the Science Council supported this proposal along with those for a Max Planck institute, the two laboratories of the FhG and the branch of the Big Science Center. As a result of the recommendation, not a single employee of Institute 1 had to be dismissed into unemployment (with the exception of a few persons who left the institute because of earlier political involvement).[46]

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44 This was pointed out by members of the expert group. It seems conceivable that positive or negative judgments by the Science Council or other scientific peers in the course of the evaluation created positive or negative feedback loops, mutually amplifying external judgment and internal self-esteem.

45 This was stated by a member of the institute.

46 This was confirmed by a leading member of the institute.

The implementation of the recommendations was not altogether smooth. The MPG did not make the official decision to found the new institute until late November 1991 (shortly before the end of the "moratorium"). The research laboratory of the *Land* government has not come into being because of resistance from the *Land*'s ministry of finance. By and large, however, the outcome of the transformation of Institute 1 can be considered very positive. The goal of organizational survival was not achieved for the institute as a whole, but to a high degree on the level of its subunits. Moreover, the interest of a high share of the individual employees in securing their future regular income was satisfied. Finally, as a member of the institute assured me, the most important research fields can also be continued in the new institutions. All in all, the case of Institute 1 can be classified as an example of successful coping. The strategies of impression management, niche selection and networking were aptly pursued by the institute.

3.2.2 Case Study 2: A More Problematic Case with a – Comparatively – Happy Ending

Institute 2 was founded in the early 1980s as a part of the East German program for the promotion of microelectronics research. Similar in size to Institute 1, it was highly application-oriented, comparable to some extent to an industrial research laboratory. For a time, basic research only made up 15% of its activities. Two thirds of its employees were technical and clerical staff (thus, Institute 2 was one of the institutes of the AdW with the lowest share of scientists). Most of its research was performed in close cooperation with a local semiconductor manufacturer. Due to the high economic priority placed on microelectronics, Institute 2 received comparatively large subsidies from the state; its investment funds were extraordinarily large. On the other hand, it was rather secluded from international research in the discipline. As the institute conceded in its self-portrait for the *Forschungsgemeinschaft*, it was mainly working to reproduce the international R&D standard.[47] However, it pointed to a few original research contributions acknowledged by the international scientific community, including a biennial international conference on problems of semiconductor technology it organized from 1985 on.

47 The insufficient participation of the physics research institutes of the AdW in international research (with some exceptions) is admitted in AdW (1990: 20).

Since its founding, Institute 2 had been directed by the same professor, who, according to one of the institute's leading scientists, did not have much of an international reputation and lacked contacts outside the GDR. Evidently, it was the other scientists who were to be credited with managing to establish the international contacts mentioned above. All in all, it can be stated that the "trouble situation" for Institute 2 was more severe than for Institute 1. Institute 2 worked in a subfield in which the isolation of the GDR from the international development was particularly disadvantageous and the competition among R&D institutions particularly strong. The institute was not able to build on widely uncontested prestige within the scientific community.

Moreover, the internal situation of Institute 2 was rather problematic. Much more than Institute 1, Institute 2 was divided into subgroups with conflicting interests. The necessary process of reorganization was hindered by this situation. Several times the director was voted out of office, only to be reappointed soon after. Eventually, he remained in charge until the end of existence of the institute. As one member of the institute put it, the different subgroups maintained a certain loyalty to the director, seeing him as a figurehead who was not, in fact, able to take decisions binding on them. The election of a scientific council did not take place until spring of 1990.

During the first half of 1990, the search for the future scientific and organizational orientation of Institute 2 proceeded rather slowly. A member of the institute reported that in this period the institute possessed little information about the West German research system and could hardly figure out the differences between research organizations like the FhG or MPG. In spite of its low share of basic research, for a short period Institute 2 even thought about going in the direction of becoming a Max Planck institute.

The most serious efforts at that time, however, concerned the FhG. The director of Institute 2 had several talks with the director of a Fraunhofer institute in the same field. At first, the FhG gave hopeful signals as to its willingness to integrate Institute 2, but a definite promise was never made. Nevertheless, the inclusion of Institute 2 into the FhG was described as the institute's aspiration in the academy's report compiled in June 1990 (AdW 1990). Other activities of the institute in that period included the reduction of personnel by separating out several subunits involved in the manufacturing of equipment and putting them into private ownership.

According to one of its members, Institute 2 did not prepare very carefully and systematically for the evaluation by the Science Council. The response to the questionnaire was compiled and written mainly by an employee of the institute's administration. The institute's scientific council did not take part in a systematic way, and the director even went on vacation while the report was being prepared. Upon reading Institute 2's response to the questionnaire, members of the physics department of the AdW had the impression that the proposal for future development would not be successful. Not only its content, but also its style and form were criticized. The response did not reach the Science Council until just before the deadline.

Institute 2's statement regarding its future was one of the most indefinite among all the answers submitted to the Science Council. It proposed splitting the institute into three units; this was an indication of the rift between Institute 2's subunits. Two of the suggested laboratories were supposed to cover certain scientific topics, the third one was supposed to become part of a nearby university. Beyond that, the text simply stated that the question of the future status and institutional affiliation of the institute was to be treated separately.

In November 1990, the expert group of the Science Council visited Institute 2. The result of the inspection was ambiguous. On the one hand, the expert group found that the kind of fundamental research the institute had suggested would be impossible due to technical insufficiencies, and that its conflicting subgroups did not "speak a common language" (as a member of the institute put it). Moreover, a member of the expert group heavily criticized the presentation of the institute given by its director. On the other hand, the expert group pointed to the stock of expert knowledge available there. With reference to economic considerations, it concluded that the preservation of this knowledge was desirable, but that a practicable way to achieve this was yet to be found. As one of the experts reported, regional policy was discussed as another reason to preserve Institute 2 (it was the only institute of the AdW in that region). Thus, in spite of the unsolved problems of its future organizational affiliation, the selection of an appropriate research program and the criticism of the available infrastructure, it was evident that the expert group intended to deal with these issues and to grant the institute some support.

After the inspection, the members of Institute 2 perceived their situation rather pessimistically. One member reported that at that time he received information that only a small group of scientists would be able to "survive." Thus, it can be stated that at that point in time the trouble situation of Institute 2 was still quite serious and that its coping strategies were not working very well.

Nevertheless, at about the same time a group of scientists became more actively engaged for the future of Institute 2. From January 1991 on, this group – without the director! – compiled another report which they sent to the Science Council on 17 January. This statement contained a rather self-critical account of the suggestions the institute had made in its first report to the Science Council. It also presented a modified research proposal and new organizational solutions, particularly the foundation of an institute tied to the local university. It reiterated, however, the suggestion to split Institute 2 into three units. On 15 January, the director had sent his own revised report to the Science Council which was less critical and less detailed with regard to the suggested research orientation.

A rift between a passive or even obstructive director and a more active group of scientists within Institute 2 was becoming apparent. The group also began to intensify contacts with scientists and industrial corporations in West Germany and abroad, asking them for their point of view with respect to the future of Institute 2. They received some supportive reactions which they passed on to the BMFT, thus demonstrating that important actors perceived considerable demand for the activities of Institute 2. What seems particularly important is that they got in contact with an outstanding Western scientist (Institute 2 had already cooperated with him before 1989). They offered to recommend him as the founding director of a possible successor institute emerging out of Institute 2. They were able to gain his interest, so that from then on he actively supported the institute in its process of reorientation. Evidently, the strong support of such an eminent member of the community of scientific peers (as an additional expert, he also took part in the evaluation by the Science Council) was very beneficial in the further development.

In February, the same group of scientists sent another report to the Science Council. The proposed research topic was further elaborated upon; the issue of organizational affiliation, however, still remained rather uncertain. The three more recent reports to the Science Council no longer mentioned the FhG. By the end of 1990 at the latest, the FhG had decided not to take over Institute 2 or parts of it (apparently mainly because of the insufficient state of its technical devices and because the FhG already had several laboratories working on similar research topics).

From May on (i.e. in the late phase of the evaluation period[48]), there was important progress in the process of niche selection for Institute 2. It chose a new specialty upon which it would concentrate (a combination of semiconductor materials that was not frequently treated in German laboratories). The Western scientist mentioned above – who, in fact, was later appointed to be the founding director of the successor institute – played a major part in conceiving this research program; he did so, among other things, in talks with the BMFT where he found support for this project. This special research program was not explicitly mentioned in the recommendation of the Science Council. However, its development (about which the Science Council was well informed) very likely had a positive effect on the judgment of the evaluators. According to the opinion of a member of Institute 2, it also partially reduced the thematic similarity and hence competition with laboratories in the West German (and even European) field of microelectronics. Without that reorientation, the Science Council probably would have recommended a much smaller successor institute.

Since spring of 1991, a Blue List institute was the favorite organizational solution for Institute 2. The institute stated its preference for this option in a talk with the chairman of the expert group of the Science Council. Eventually, in the decisive meetings in June and July 1991, the relevant committees of the Science Council opted for founding a Blue List institute based on Institute 2. The proposed institute was to have roughly half as many employees as Institute 2 had at the end of 1991. Since it included a markedly higher percentage of scientists, however, a comparatively large share of scientific employees of Institute 2 was given the opportunity to continue their work. A member of Institute 2 stated that the institute's most important scientists found their way into the newly founded institute. All in all, he ranked his institute as belonging to the most successful third of the physics institutes of the AdW, although he had ranked it as being in the most problematic third during the first phase of transformation.

Although the new institute differs from the old one in size and many other organizational aspects, one can assume the continuance of the original or-

48 Thus, the time factor is obviously very important. While a crucial and beneficial change in the development of Institute 2 (and there are other similar examples) took place, the recommendations for roughly one third of all institutes of the AdW had already been passed.

ganization. The interest of organizational survival, therefore, was preserved, even though the institute did not pursue it during most of the transformation period.[49] However, the interest of the individual employees in continued employment was met to a lower degree than in Institute 1.

Case Study 2 demonstrates that the evaluation process included the chance to revise a temporarily unfavorable course of development. Though Institute 2 did not pursue a very convincing coping strategy in the first phase, it acted more appropriately in the second one, when the means of niche selection and networking were more actively brought to bear. There is evidence that these activities had a positive impact on the decision of the Science Council, and that they contributed to a correction of the ambiguous first impression Institute 2 had made. What seems equally important is that – as had happened in the case of Institute 1 – a beneficial interplay emerged between the Science Council, the evaluated institute, and the government agencies which later promoted the successor laboratory.

3.2.3 Case Study 3: A Particularly Problematic Case

Founded in 1954, Institute 3 was directly subordinate to the Ministry of Heavy Industry until 1970, when it was transferred to the physics department of the AdW; in 1973 it switched to the chemistry department. As a member of the expert group stated, Institute 3 had always been a kind of alien element within the academy. Mainly performing tasks in the science of engineering, it did not fit easily into the pattern of disciplines. Like Institute 2, it was highly application-oriented; it had a high share of non-scientist personnel and a percentage of basic research that did not exceed 30% of its activities. Its tasks were determined to a great extent by requirements typical of the GDR: the institute concentrated on optimizing the exploitation of raw materials available on the East German territory. According to the report the institute submitted to the Science Council, it protested several times – fruitlessly – against the imposition of this kind of task from above during the last years of the GDR. In terms of personnel, Institute 3 was comparable in size to

49 A member of the expert group stated that the proposal of a disintegration made by Institute 2 was the most foolish strategy the institute could pursue. It sacrificed one strong point of the institute, namely the combination of application-oriented activities and basic research.

Institute 1 and 2. Housed in a huge building, it had a great number of technical devices, allowing it to work up large quantities of material. As the institute itself admitted in the report, its level of participation in international research cooperation was relatively low. It was not well known in the international scientific community, nor did it have many contacts to foreign laboratories, especially in the West. This is partly explained by the fact that, many years ago, most Western research institutes discontinued their engagement in the rather traditional kind of research performed by Institute 3.

Because of its tight inclusion into the GDR's policy aimed at national autarky and its lack of prestige as a scientific institution, Institute 3 was faced with severe trouble when the unification process began. The only assets it could be said to possess were the technical facilities and the – mainly technological – expertise of its personnel.

As had been the case in Institute 2, the first period after the revolution in 1989 was marked by serious internal conflicts. Constructive action for internal reforms did not get under way before spring of 1990. In May, confidence votes were held. While the director received support (he retained his position until the winding-up of Institute 3), one of his deputies and roughly half of the heads of the departments and sections were not confirmed. However, not all of them immediately resigned from their positions.

In the first evaluation report for the *Forschungsgemeinschaft* (June 1990), Institute 3 outlined a reorientation of its activities toward environmental research, conceding, however, that since most of these topics would be new territory for the scientists, it would take some time before they could perform this research with optimal efficiency.

The preparation for the evaluation by the Science Council was mainly organized by the established top personnel. The response to the questionnaire makes a rather indeterminate impression. No profound change of the research orientation was intended, except for an intensification of environmental research and a strengthening of basic research. Preference for maintaining the institute – in the form of a Big Science Center – was expressed. In retrospect, this option must seem rather impracticable even to the members of the institute. A leading scientist of Institute 3 admitted that the institute as a whole would not have fitted into the emerging research landscape of unified Germany. A second choice described in the report was splitting the institute into three units, each of which would have a different institutional affiliation. Moreover, plans for separating out parts of the institute and transforming them into private corporations were presented. Specific steps toward integrating Institute 3 or its subunits into the West German research system had not been taken up to that point.

A shift to a more active reform orientation took place rather late in the transformation process; this, too, is similar to developments at Institute 2. The personnel council and the scientific council of the institute jointly initiated what they called a "fundamental restructuring and reorganization" of the institute at the end of September 1990. Two large research departments – one of which was called "environmental process engineering" – were formed to which the research tasks of the various sections were assigned. In-house applications for the positions of the directors of these departments were requested. When these positions were filled, the newly appointed department heads in turn requested in-house applications for the positions of the directors of all the sections. Compared to what occurred in many of the other institutes of the AdW, this internal reform can be considered quite far-reaching. Only now did the influence of all the former leading members who had not been confirmed in the confidence vote begin to diminish. Thus, similarly to Institute 2, Institute 3 became more active at this time owing to a bottom-up process originating at the level of the rank-and-file personnel. In contrast to Institute 2, however, there was no open conflict with the director. According to a member of the institute, the new organizational structure and the new leading scientists made it possible for the process of reorientation to be pursued more by consent than by conflict.

As of October 1990, Institute 3 began to have talks with organizations that might potentially take over parts of it. Several talks with the FhG bore no fruit in the end. The FhG made clear that Institute 3 would not be among the research groups it would take over, at least in the short term. Institute 3 also got in contact with a local university. For some time, the project of creating a publicly financed institute associated with that university and staffed by personnel from Institute 3 was discussed. Ultimately, however, the university refused to take over Institute 3 as a whole or any large parts of it. However, it signaled its willingness to integrate some small research groups into its chemistry department.

The situation was aggravated by the fact that quite a number of scientists left the institute at that time. Though the decline in personnel did not turn out to be as large in Institute 3[50] as it was in Institutes 1 and 2, the institute itself complained in a report to the Science Council that the departure of 16 scientists had left a noticeable gap with respect to the fulfillment of future research tasks. As members of the institute reported, an outstanding scientist had already left the GDR before the opening of the border. Some members felt that had he remained, this very scientist could have become director and given the institute a new research profile. So there is some indication that – in comparison to the developments in Institute 1 and 2 – Institute 3 suffered more disadvantageous effects from the interplay between the level of individual scientists and the organizational level. However, interviewees from Institute 3 stressed that, even with the help of the scientists mentioned above, the negative outcome could not have been prevented.

At the end of January 1991, Institute 3 drew up an additional report for the Science Council. The institute stressed that a critical examination of the situation in East Germany had made it clear that a stronger concentration of the institute on environmental research was required and proposed a corresponding research program. Regarding the institute's organizational future, a split-up in various units was now planned. Still, the future affiliation remained rather unclear. Among other things, an institute of the Blue List and a laboratory of the FhG were mentioned. Institute 3 also intensified its efforts to acquire external funds from private or public institutions. However, these activities met with only limited success. In particular, the institute did not succeed in getting significant support from industrial corporations.[51]

The revised report to the Science Council was completed only three days before the inspection date in February; this was one of the last inspections conducted in the AdW.[52] The evaluators were apparently impressed by Institute 3's technical facilities, and after the inspection the outlook within the institute was rather optimistic.

50 In its first evaluation report for the *Forschungsgemeinschaft*, Institute 3 stressed that a reduction of its R&D personnel was not required.

51 Shortly before the decisive meeting of the Science Council, Institute 3 had to admit that the number of projects for which external partners had granted their support was still too small.

52 Again, one may speculate about the importance of the time factor. Since the inspection of Institute 3 took place very late, the stretch of time remaining up to the final recommendation was comparatively short. Conceivably, this rendered the search for appropriate institutional solutions more difficult.

Actually, the expert group found it necessary to consult additional experts to evaluate the scientific value of Institute 3's research.[53] The four expert opinions submitted were rather negative. The expert group came to the conclusion that the technical devices of Institute 3 were oversized and that there was no perceivable demand for the kind of work it performed. Nevertheless, since Institute 3 was principally considered to be a valuable establishment (mainly because of the technical facilities and expertise available there), the expert group launched some efforts to find suitable solutions for its future.

Between the date of the inspection and the passing of the recommendation in July, Institute 3 started several new initiatives. A small research group joined other institutes of the AdW in drawing up a proposal for a new Max Planck institute – in fact, it was later integrated into this very institute. More importantly, Institute 3 drew up a plan for integrating one of its two departments (with more than 100 employees) into a new Big Science Center for environmental research which was soon to be founded in East Germany.

The expert group, on its part, had worked out a plan in which Institute 3 would become a soil decontamination facility which would be funded by the BMFT or the Federal Ministry for the Environment. In fact, when the recommendation was finally published, this project was the only proposal that included the chance of further support for a significant number of employees of Institute 3, whose dissolution was recommended. Beyond that, the Science Council only expressed its support for the integration of the group mentioned above into the Max Planck institute and of some small research groups into the university. The option of integrating parts of the institute into the new Big Science Center was not even mentioned.[54] What was even worse for Institute 3, the Science Council did not give an unconditional recommendation for the project of an establishment for decontamination. Rather, it formulated this idea as a mere suggestion to the appropriate ministries, pursuing it with much less vigor than the usual, unconditional recommendations.

53 This relates to the special character of the institute which obviously presented a problem to the expert group.

54 One reason may be that this option was introduced in the second additional report to the Science Council, which was written at the end of June 1991 just two weeks before the decisive meeting of the Science Council. At that point, it was obviously too late to give serious consideration to a project of such significant dimensions.

Thus, things turned out rather negatively for Institute 3 in the end. The establishment for the decontamination of soil has not come into existence (and probably will not). Though there may have been a certain willingness on the part of the BMFT to realize the project (actually, the ministry supports the former institute to a significant degree with project grants), several serious problems surfaced. The existing demand in East Germany for this kind of work had obviously been overestimated; doubts emerged as to the appropriateness of the institute – situated in the midst of a residential district – for treating large amounts of highly toxic material; moreover, there was a discussion as to whether such activities should be financed by public authorities at all or rather be performed by private enterprises. The (former) members of Institute 3 were forced to make the best of their situation, and they were moderately successful. Some groups were able to survive on project grants, others tried to hold their own as business corporations. For many others, receiving funds from the federal labor office's job-creation program (ABM) was the only way to continue their work, at least temporarily. Including the Max Planck and the university groups, approximately half of the former personnel of Institute 3 receive further funding.[55] But most of their positions are highly insecure. Thus, neither the interest of organizational survival could be satisfied, nor could a high share of employees realize their interest in continued employment.

Not surprisingly, members of the institute expressed their strong dissatisfaction with this outcome. Some of them stressed in particular that the Science Council, ignoring the reorientation that had taken place, continued to focus on the first proposal written before the internal reform. They expressed the opinion that all the coping activities of their institute had virtually no impact on its fate. Moreover, most of the interviewees explicitly blamed the evaluators for having pursued some kind of self-interest as competitors for R&D resources. They called the decision to "wind up" their institute a "political" one.

External actors from the Science Council and authorities involved in the institute's evaluation drew a different picture. They described the activities of Institute 3 as being far removed from any conceivable demand once unification had taken place. They criticized the reorientation of the institute toward

55 Thus, individual coping to some extent compensated for the failure of corporate coping (see the similar example described by Schimank in this volume).

environmental research as being an obvious break in its tradition which could not be bridged successfully.

To summarize, in the case of Institute 3 none of the coping efforts sufficed to prevent the negative outcome. Networking did not function well; only a very limited number of external supporters could be won. Impression management apparently worked in only one respect: The expert group was indeed impressed by the technical facilities available in the institute, and this seems to be a major reason why they made an effort to find a suitable solution at all. The endeavor of Institute 3 to find an appropriate niche by redirecting its activities toward environmental research was not successful because it represented a break with the organization's past which was considered to be too abrupt.

4 Conclusion

A variety of factors determining the fate of the institutes of the AdW during the process of German unification was described in the preceding sections. All the institutes were equally affected by the *general trouble* of German unification, i.e. by the abrupt change of their general environment described in Section 2. But each of them also found itself in a *specific trouble situation.* The particular organizational history of each institute, its research orientation, its organizational prestige, the dynamics of its personnel's exit decisions, and the particular actor constellation in its scientific field are elements of this specific trouble. Each institute followed its own strategy of *coping* with both kinds of trouble. Three types of strategies were described: niche selection, networking and impression management. General trouble, specific trouble and coping in combination determined the organizational success of the institutes.

As the case studies demonstrated, the elements of trouble *and* coping can be easily found in the development of the single institutes. However, the case studies have also shown how difficult it is to strictly separate specific trouble and coping with respect to their impact on the final outcome. For instance, it seems virtually impossible to determine the extent to which the organizational success of an institute can be attributed to the sophistication of its coping behavior, the moderate amount of trouble it faced, or a combination thereof. Although I tried to delimit the impact of these factors by considering how they were evaluated by different observers from within and outside the institutes, an exact weighing of these closely interwoven factors is not practicable. Given these reservations, the three cases can be thus summarized:

- The specific trouble situation of Institute 1 was relatively moderate. Among other things, it could capitalize on its experience in a subfield of chemistry which was considered a desideratum in West Germany. Thus, the likelihood that large parts of Institute 1 would survive the trouble of unification was rather high from the start. Nevertheless, Institute 1 also proved to be outstandingly skillful in coping. It can be assumed that the institute was thereby able to increase the share of its personnel which was to receive further promotion.
- Institute 2 found itself in a more severe trouble situation. It worked in a field where the technological lag of the GDR behind the Western industrial nations was particularly disadvantageous. Its problematic internal state also seriously affected its coping efforts. However, rather late in the transformation process, it succeeded in overcoming these difficulties (at least to a certain extent) and started some activities which turned out to be helpful in the end. Perhaps the most important among these activities was that the institute managed to win an outstanding Western scientist as a coalition partner. As in the case of Institute 1, it seems very likely that coping contributed substantially to organizational success.
- Institute 3 had to cope with the most serious specific trouble. Its activities were particularly closely determined by conditions typical of the vanishing GDR. As was the case in Institute 2, it did not display very active coping behavior for a long time. It also became more active in a comparatively late stage of the process. There is some indication that, even then, its coping behavior was not particularly adroit. Among other things, Institute 3 did not finish the two additional reports to the Science Council until just before the inspection and the final meeting respectively. Therefore, it is understandable that the Science Council could not thoroughly analyze these documents. However, it appears plausible that in the case of Institute 3, the trouble of unification was so overwhelming that even the most skillful coping behavior would not have changed the final outcome.

Summing up, how can the transformation of the institutes of the AdW during the unification process be described? Since a certain scope of action on the part of the institutes as well as a certain impact of their coping behavior on the final outcome could be demonstrated,[56] the notion of "bulldozing" (which equates the process with an ineluctable catastrophe) does not do justice to the process in general. Instead, I want to employ another metaphor: Kaufman (1991: 67) likens the organizational environment to a "perpetually varying net or screen sweeping continuously through the total aggregation of interlocked organizations that form in the human population." Kaufman describes how organizations continuously try to change their shape in order to fit through the holes of the net. And – be it by deliberate planning or by accident – in some cases they actually succeed in fitting themselves through. But there are three other possible cases: Sometimes the shape of the organization and the shape of the holes differ so much that the organization cannot succeed in adapting its form, no matter how undaunted its efforts. Sometimes the holes in the net are so large compared to the organization that the latter will fit through regardless of its adaptation efforts. A fourth case is conceivable: an organization may at first be shaped appropriately to fit through the net, but its very efforts to maintain or improve this fit may lead to the unintended consequence of a shape that no longer matches the holes. Though such fatal maladaptation may have occurred in the transformation of the institutes of the AdW, I do not know of any specific example. However, Institute 1 came very close to pursuing a strategy which probably would have turned out very disadvantageously. In the first months after the revolution in the GDR, members of the institute discussed the possibility of separating out the institute from the AdW and transforming it into a private enterprise. Had it done so, the institute would not have profited from the temporal protection of the "moratorium" and would have faced the very difficult situation of East German private R&D establishments during recent years.

The process of German unification (or: the trouble it produced) can be equated with one pass of the environmental net through the population of East German institutions, among them the institutes of the AdW. The institutes

56 Additional support can be drawn from a poll (Bigl 1991) which was answered by 26 institutes of the natural science department of the AdW (including two institutes of the Academy of Agricultural Research). 55% stated they were justly evaluated by the Science Council, 40% agreed with qualifications, only 5% answered in the negative. 50% reported that the recommendations of the Science Council were mainly based upon their own suggestions; 40% agreed to this with modifications, and only 10% negated this statement. These results indicate that many institutes perceived noticeable impact of their actions on the outcome of the evaluation.

that did not fit through the holes of the net have been swept away, the others "survived." True, Kaufman (1991: 80) points to the limits of this metaphor: "I do not mean to portray organizations and their environment as separate, independent forces, one active and the other passive." According to his approach, "the properties of organizations themselves are important determinants of the environment and of adjustments to it." In the case considered here, these limits are obvious. The institutes of the AdW were not compelled to adapt to identical holes; the character of the net (i.e. the gravity of the environmental change) varied from one institute to the next. This is what I referred to as specific trouble: The net had holes of different shapes and sizes at different positions.

For some of the institutes, the holes in the net at their particular position were so large that they would have fitted through them in any case, regardless of their own actions. Institute 1 can be placed in this category.[57] Some other institutes – like Institute 3 – were shaped so differently from the holes that they would have been swept away by the environmental change no matter what they did. In a third group of institutes – among them Institute 2 – the difference between the shape of the organization and the shape of the hole was so small that it could be made up for by the coping behavior of the institute. Only in this subgroup was coping essential for organizational survival.

Since the case studies have demonstrated how difficult it is to evaluate the impact of the different factors on the final outcome, it does not seem practicable to distribute the 60 institutes of the AdW among these three categories. Nevertheless, it can be stated that a significant number of institutes falls in the second group, in which coping made a difference. This holds true all the more if we depart from the binary distinction between organizational survival and death, which was adopted above from ecological organization theory, and take into account the gradual changes successful coping could achieve (for instance an increase in the share of personnel which was to be integrated into successor organizations).

All in all, the "Coping with Trouble" approach can be useful in the particular case considered here, but it must be employed in a differentiating way. *All* the institutes were in trouble, and *all* of them coped with it in some way, but since their specific trouble varied to a high degree, coping behavior mat-

57 At least if we disregard the potential maladaptation described above.

tered significantly only for a subgroup of them with respect to their organizational fate.

If all that is true, however, the result of this article appears rather promising for the concept of "Coping with Trouble." After all, the discussion in Section 2 stressed the singular and particularly far-reaching character of the trouble of German unification from the perspective of the institutes of the AdW. If effective coping strategies can be discerned in such an exceptional trouble situation, this analytical concept must be all the more useful in more common situations of "normal trouble." Thus, further investigation in this direction seems worthwhile.

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German National Research Centers under Political Pressure: Interference between Different Levels of Actors

Andreas Stucke

1 Introduction

The dissolution of a whole political system (socialism) and its integration into another system (capitalism) is a very extreme form of social change and, obviously, a very drastic cause for severe political turbulence. It is often overlooked that these integration processes have caused significant changes not only in East Germany, but also in West German society and its differentiated subsystems. Undoubtedly, the transformation proceeding the fall of the Berlin wall in 1989 has threatened both the stability and the legitimation of the West German research system more than any other event since World War II. For not only has this process opened up many new opportunities for the focal actors, which include the federal government, the federal states and the German research organizations such as the Max Planck Society, the Fraunhofer Society or the National Research Centers[1] – it has also produced social threats. Now we can see that the opportunities these research organizations had to expand in the East went hand in hand with financial cutbacks and the delegitimation of their previous research programs. This is especially true for the National Research Centers.

But there is another reason why the National Research Centers in Germany represent a very instructive case of politically induced trouble. As op-

1 We prefer the term "National Research Centers" rather than "Big Science Centers" because it corresponds more accurately with the official terminology of the centers themselves and the ministry to which they belong. Nevertheless, the historical identity and organizational development of the centers is based on "Big Science" in the field of nuclear energy.

posed to other research organizations, they were "caught" by German unification in a "double trouble" situation. The National Research Centers had been suffering from political trouble since the mid-1970s and were just recovering when the unforseen event of German unification threw them into a new, turbulent situation. It is ironic that the National Research Centers were just beginning to cope successfully with their existing trouble when they encountered the challenge of German unification requiring additional new coping efforts. This type of coincidental[2] trouble situation resulting from the convergence of two different sources of trouble makes the National Research Centers an interesting case study of coping with trouble.

From a theoretical point of view, the National Research Centers provide a good example for the interplay and the interdependence of different levels of actors. Analytically, we can distinguish four levels of action: the individual researcher, the project group or institute as a part of a National Research Center, the entire National Research Center and, finally, the Association of National Research Centers. One of our main goals is to answer the question (from a strategic as well as theoretical point of view) of how coping activities on one of these levels cause – sometimes intended, sometimes unintended – effects on other action levels.

By examining national research in Germany from this particular perspective, we hope to gain greater insight into how scientific actors respond to serious and sudden changes in their political environment. Taking for granted that the growing dependence of the scientific subsystem on the political subsystem is an irreversible result of evolution during this century,[3] we must systematically question the strategic capacity of science in modern societies. Therefore, I would like to contribute to an institutional perspective on science using multilevel actor constellations as an analytical focus from which to study strategic action problems of science.

Regarding the research methods employed, it is important to note that the case of the National Research Centers in Western Germany represents an ongoing social process which became dynamic in mid-1991. In contrast to many case studies in this book dealing with "histories" which can be considered complete, the events connected with this case can still be observed. We

2 The term "coincidental" means the "convergence of two independent causal series" as Boudon (1986: 175) puts it. Cf. also Schimank (1988).

3 Cf. the introduction by Schimank and Stucke in this volume.

can collect data from the past and the present, and guess how the future will develop.[4] Therefore, we can only offer limited observations of real coping activities now, but think it is justified, and might even be fruitful, to speculate scientifically about the future coping of national research actors and the side effects it may have.

Such observations will help to answer these questions in the following sections. 2: Why has German Unification not only offered good opportunities but actually caused trouble for the West German research system? 3: Why are the National Research Centers in West Germany much more susceptible to this form of trouble than other types of research organizations? 4: What are – or might be – the coping efforts of the different actors at the different levels within the National Research sector? And what are the (mostly unintended) effects of the interplay between different levels of coping activities?

2 German Unification: Good Opportunities and Increasing Trouble for the West German Research System

It was not obvious that German unification would create a troublesome situation for the well-established West German research system. International observers, in particular, might think that the term "political trouble" more aptly describes the total restructuring of the *East* German science system, which lost its institutional basis and half of its personnel through the unification process.[5] Nevertheless, it is not just a peevish complaint when Western actors consider their own situation to be troublesome within the framework of German unification. To understand this, we must distinguish between two main stages of the unification process: a *first stage* of basic decision making, which lasted from early spring of 1990 to the date of unification, 3 October 1990 and a *second stage* of implementation – i.e. making unification actually work – which is still going on. Unquestionably, the actors of the West Ger-

4 My empirical data consists of official and unofficial documents, and interviews with the managing directors of some National Research Centers and with officials from the research ministries at the federal and state level. In order to guarantee the anonymity of the experts, I can only name the organizations from which the information originates.

5 For more detail, see the contributions by Mayntz and by Wolf in this volume.

man research system prevailed from a legal point of view in the first stage, having succeeded in upholding their position that nothing should be changed in the West after unification, and that East German science would have to adapt to the established West German structures (cf. Mayntz 1991; Stucke 1992). But they failed to anticipate that this policy aimed at preserving the institutional status quo could *not* avert massive and substantial repercussions in the West German science organizations during the second stage, which led, in turn, to a new and unforeseen troublesome situation for themselves.

In the first stage, in the winter of 1989/1990, some of the science organizations were motivated by special interests, and some were simply guided by organizational indifference to the processes in East Germany. It is not surprising that the market-oriented Fraunhofer Society[6] was the first German science organization to present a concept for cooperation with East German institutes and to establish joint ventures with East German scientists. Neither is it surprising, however, that the National Research Centers in Germany reacted to the incentives of the Federal Ministry for Research and Technology (*Bundesministerium für Forschung und Technologie*, BMFT) to expand in East Germany. Having gone through a difficult period in which it had to suffer with criticism from many sides,[7] "Big Science" viewed unification as a unique opportunity to enhance its scientific reputation. Various types of cooperation with East German scientists and institutes were built up, especially by National Research Centers working in the fields of high-energy physics, cancer therapy, biotechnology and informatics. Despite the existence of individual contacts between individual scientists, there was not as much cooperation in the classic fields of Big Science, nuclear energy and space technology. The German Research Foundation (the largest science-promoting agency in Germany) and the Max Planck Society, on the other hand, reacted cautiously to the expectations regarding cooperation with East German science organizations. Both organizations hesitated because of their limited budgets and because they did not have enough information about the scientific and technological quality of the East German institutes.

6 The Fraunhofer Society conducts applied research in cooperation with state organizations and industry. Most of its institutes are self-financing, drawing on their own profits from contract research for firms and for the government.

7 For more details see Section 3 below, and Hohn/ Schimank (1990: 233-297).

This period of cooperation ended in March 1990 when Lothar de Maizière (a Christian Democrat supported by the West German Christian Democratic Union) won the general election in East Germany and an immediate unification of both states dominated the political agenda. In the early spring of 1990, the immediate and direct unification of the two German states became more and more probable. At this time, the BMFT, which finances 90% of the institutional promotion of the centers, stopped the cooperation activities of the National Research Centers in the East, fearing that the expansion of an expensive type of research organization would have repercussions in its own budget. Gradually, the National Research Centers began to see the process of unification in a completely different light. The phase in which changes had seemed to offer good opportunities gave way to one in which defense of the status quo took priority over all else on the political agenda of West German science organizations.

In April and May of 1990 at the latest, the actors of the West German research system had to start preparing for German unification. Two questions became increasingly important: What kind of institutional science structures would be desirable in a united Germany? What legal and administrative procedures would be needed to establish these all-German institutional structures? At this time, all the western science organizations were interested in preventing the emergence of a separate science structure in East Germany after unification. Despite many diverging interests, they converged at a shared first-order goal: the defense of their respective domains. The federal government, the federal states and the science organizations arrived at the consensus that the institutional status quo should be protected. Specifically, this meant that the West German actors agreed to maintain the federal structure in an all-German science system. The decisive question of what consequences the establishment of five new states in a united Germany would have for the future of cooperative federalism within the area of science policy was completely neglected at this time. The federal government, the federal states and the science organizations were content to emphasize federalism and the joint promotion of science as a formal principle.

The formal organizational structure of the all-German science system was a more critical question between the three actor groups, but they finally managed to agree on this, too. In June and July of 1990, the BMFT adopted the position of the West German science organizations completely. The establishment of new GDR structures and the consolidation of the old ones was to be avoided, and there was to be "only *one* German Research Foundation, *one* Max Planck Society and *one* Fraunhofer Society." The West German actors neglected to consider the substantial consequences of integrating the former GDR institutes and scientists into the existing West German science system, being satisfied once again to proclaim that nothing was to be changed. To implement the transition of the East German science system, the West German Science Council (*Wissenschaftsrat*) was commissioned to evaluate all extra-university institutes in East Germany with regard to their scientific performance (cf. Krull 1992; Simon 1992).[8] Those which were evaluated positively were to be integrated into the appropriate West German science organizations, while those evaluated negatively were to be completely dissolved.

Hence, the strategy of the major West German actors with regard to the formal negotiations on the unification treaty became clear:

1. The East German Academy of Sciences would only be temporarily maintained.
2. All East German institutes which were evaluated positively would be integrated into the West German science structure.
3. The new East German states would join the West German "Agreement on the Promotion of Science" (*Rahmenvereinbarung Forschungsförderung*) between the federal government and the states.

This interest in preserving the status quo was sanctioned, finally, by Article 38 of the German Unification Treaty (*Einigungsvertrag*), which went into effect on the date of German unification, 3 October 1990 (Einigungsvertrag 1990: 902). By the end of 1990, it seemed that a definite winner in the unification process had emerged: the West German science system. It had successfully preserved its institutional structures, while the East German science system, on the other hand, had been completely dissolved.[9]

During 1991, it became increasingly evident that the unification process in science would be more than a mere formal integration of some East German scientists into the well-known West German structure. In mid-1991, the

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8 The corresponding extrauniversity science organization in East Germany was the Academy of Sciences with 24,000 employees (cf. Gläser 1992).

9 The mere transfer of institutions from West to East is one typical pattern of German unification in all policy areas (cf. Lehmbruch 1993).

Science Council finished its evaluations and recommendations on East German science, so that new centers, institutes or project groups could be established in the five new states in the East. In that context, three new National Research Centers were founded in former East Germany in 1992: the Geoscientific Research Center Potsdam, the Max Delbrück Center for Molecular Medicine in Berlin-Buch and the Center for Environmental Research Leipzig-Halle. In fact, German unification opened up good opportunities for some East German scientists and institutes that had repercussions within the West German science system.

As early as 1991, however, the federal government was pressed by a growing national debt resulting from German unification. Consequently, the government proclaimed a political principle of "establishing and developing [institutions] in the East before expanding in the West," so that a new, unanticipated trouble situation for the Western science system emerged. Three interrelated dimensions of this trouble can be distinguished: a financial, a social and a programmatic one. The financial dimension is illustrated by the fact that the institutional basis of the extrauniversity sector in Germany after unification increased by about 20%, while the budget of the BMFT increased only about 10% (Meske 1992). This forced the BMFT to "rob the West to pay the East." Now we have a zero-sum game in Germany in which new institutes and groups in East Germany are financed mainly by cutbacks in the West.

The scarcity of public resources has led in the social dimension to increasing competition between institutes in the East and the West, and among the Western science organizations themselves. Every science organization in the West is trying to use its strategic power and reputation to avoid financial losses and, consequently, to keep open its options for realizing its own scientific programs in the future. According to the "Matthew effect," the institutes and research organizations with good reputations are better off in such times of redistribution, while those with image problems – like the National Research Centers, which had been the subject of criticism for 15 years – are at a relative disadvantage.

Social competition calls forth not only competition for scarce resources, but also competition to preserve the established program areas an institute wishes to maintain. Hence we can expect competition between East German and West German institutes struggling to be more successful in certain research areas in the future. Since political actors will undoubtedly try to reduce redundant capacities in specific program areas, West German institutes will be forced either to face a process of substantial reorientation, or to vanish from the research scene.[10] Therefore, we may expect increasing problems of acceptance and legitimation for the West German research organizations.

All West German science organizations are confronted with varying degrees of these three types of trouble. But the National Research Centers are in a specific dilemma which has to do with their precarious status in the past.

3 National Science Centers under Political Pressure

The National Research Centers were "caught" by German unification during an important stage of programmatic reorientation. This reorientation was necessary after years of debates on the future of Big Science in Germany and strong pressure from the political actors on the centers to orient themselves toward new and challenging areas of science and technology. To understand this extreme political pressure on the thirteen National Research Centers in West Germany, one must know that the two largest ones (*Kernforschungsanlage Jülich* and *Kernforschungszentrum Karlsruhe*) were founded between 1955 and 1960 for the purpose of conducting research in the field of nuclear energy.[11] When some of the basic research programs of these centers came to an end, the federal government had a problem: Closing the centers was impossible because of the interests of the federal states in "their" National Research Centers. Moreover, the personnel could not be substituted by a younger team of scientists with the know-how necessary for programmatic reorientation, because most of the positions in National Research Centers are permanent, leaving the management little flexibility regarding personnel mat-

10 The foundation of the Center for Environmental Research Leipzig-Halle, for example, might be seen by several of the western National Research Centers as a direct threat to their own projects in the field of environmental research.

11 It must also be emphasized that these centers were "creations" of the former Federal Ministry for Atomic Energy. This ministry was able to considerably enhance its own political importance in the field of research policy by founding the centers (cf. Stucke 1993: 141-161). 90% of the institutional promotion is provided by the federal government, 10% by the respective federal states in which the centers are located.

ters. Nevertheless, considerable political pressure was brought to bear on the National Research Centers during the seventies. They were urged to look for new application-oriented research programs which would provide an adequate basis for cooperation with other sectors, especially with industry (Hohn/ Schimank 1990: 233-297). This concept of technology transfer failed partly because the centers were not willing, partly because they were not able to fulfill these political expectations. This resulted in a stage of discontent and disappointment among the political actors at the beginning and then, in the 1980s, in a reaction which can be described as reduced expectations (Hohn/ Schimank 1990: 282-297). A political redefinition of Big Science took place; the political actors, especially the BMFT, no longer expected a technology transfer to industry and a strong orientation to application. Instead, they now projected the future of the centers in the area of long-term programs in the fields of health, environment and bioengineering. The National Research Centers supported this conceptual reorientation because it would enable them to conduct more basic research and to plan more reliably in the long term. Within this general political frame, the National Research Centers made their medium- and long-term plans during the 1980s. It is rather ironic that these plans were induced by political pressure from the BMFT and are now experiencing trouble as a result of policy measures from the same ministry.

The specific directive issued by the BMFT to the National Research Centers in the "old" (i.e. western) federal states in 1991 was to reduce their personnel by 12% to 15% by 1994 (BMFT 1991). The BMFT enforced these reductions by freezing the budgets of the National Research Centers until 1994. These budget cuts were accompanied by a catalog of additional political measures such as: an examination of the programmatic priorities of all centers, the elimination of redundancies in the research programs, and the encouragement of more flexibility within the centers by a reduction in institutional promotion, by project funding, and by transforming vacant permanent positions into nonpermanent ones. It is quite obvious that the BMFT saw an opportunity to reduce costs in a research area it considered to be overfinanced in terms of institutional promotion. Why were the National Research Centers one of the main targets of budget cuts? As a group, the thirteen West German National Research Centers receive far more federal funding than any other branch of extrauniversity research. More than three billion DM are spent annually by these research organizations – three times the amount the Max Planck Society receives from government sources. The National Research

Centers receive 90% of their funding from the BMFT; some 30% percent of that ministry's total budget is appropriated to their funding (BMFT 1991).

This trouble does not affect all National Research Centers equally. Represented by the Association of National Research Centers (*Arbeitsgemeinschaft Großforschung*, AGF), the centers themselves demanded individual consideration concerning budget cuts (KFA 090992; DLR 180892). From the beginning, the AGF took the stance that proportional budget cuts for all centers without a careful assessment of their performance would harm the idea of National Research Centers in general (AGF 070593). This argument was then willingly adopted by the BMFT, which finally drew a distinction between three classes of National Research Centers to which different respective budget restrictions would be applied (BMFT 1992). Four centers (three of which had been founded to conduct nuclear energy research) had to face real cutbacks, five (e.g. the *Deutsche Forschungsanstalt für Luft- und Raumfahrt*, DLR, the space science center) would have to do without any growth over the next years, and only four, devoted to life sciences, to the study of the earth, the oceans and the atmosphere, and to cancer research could expect any growth rates at all (e.g. the *Alfred-Wegener-Institut für Polar- und Meeresforschung* and the *Deutsches Krebsforschungszentrum*). By giving the BMFT the opportunity and legitimation for this strategy to "divide and conquer," the National Research Centers – unintentionally – provoked a detailed and substantial evaluation by the BMFT which might cause new trouble or will aggravate the situation for some of the centers.

Under these preconditions, it is quite evident that collective and solidaristic coping reactions of all centers are improbable. But coping will be difficult not only because the individual centers are affected differently by these policy measures. It will also be difficult because "trouble" does not mean the same thing at the different institutional levels of action. In the case of the National Research Centers, we have to distinguish between at least four levels of action, including different types of actors with various basic interests:

a. the *individual* level, comprising the individual researchers in the institutes concerned. Here, we have to consider about 6,100 scientists (in 1989) in thirteen West German National Research Centers, most of them in permanent positions.
b. the *group* level, consisting of parts of institutes or of independent project groups. The size and degree of organization of these working units signifi-

cant enough to be classified as "groups" vary substantially, so that it is difficult to delineate the exact difference between this level and the corporate level (described below) just by looking at an organizational chart.

c. the *corporate* level, made up of the institutes and the individual National Research Centers, is characterized by formal organization and hierarchical representation to the environment. The heads of the institutes are particularly important at this level, as are the boards of directors and the management representing an entire National Research Center.
d. the *association* level, which has one actor, the Association of National Research Centers (*Arbeitsgemeinschaft Großforschung*, AGF). The AGF has an office and a manager in Bonn, and a chairman elected by all of the National Research Centers. It is the explicit aim of the AGF to "represent the collective interests of the members externally" (AGF 1991: 9; translation by the author).

While these levels of Big Science are all, of course, interrelated, each is affected very differently by political trouble. At the individual level, the individual researcher has to deal primarily with a *threat to his career* when his research area is limited and resources are reduced. At the level of institutes and project groups, such trouble is primarily viewed as a *threat to the groups' competitive position* with regard to other institutes and project groups outside the center. Here, the scientific community is the main reference group. This is not the decisive point at the corporate level, because a National Research Center integrates many institutes and many research programs. Rather, management faces the *threat of a loss of integration and reputation* with regard to other relevant actors (universities, industrial firms, ministries, etc). At this level, therefore, the main interest of a director is that the departments of his research center be considered "successful" (AGF 07051993) by the ministry and by industry. Finally, on the association level, the interests of the individual National Research Centers play a less important role; here, the actors are interested in defending the general idea of Big Science. Cutbacks and criticism from the political actors always represent a *threat that the model of Big Science will be questioned* in general.

With regard to these four levels of action, we can already observe different coping strategies which are sometimes complementary and sometimes contradictory to each other. But it must be emphasized that these different ways of coping according to the respective levels of the actors are not only the result of certain "objective" problem situations demanding different reactions from the actors; they are also caused by the strategic position of the actors, which predetermines specific perceptions and ways of information processing as well as potential means of coping. If we keep this in mind, we can understand why the corporate level predominates during the coping processes. When the political trouble for the National Research Centers began as a result of actions by the BMFT, the individual National Research Center (and its management) was called on stage as the relevant corporate actor. Indeed, the budget cuts were first addressed by the BMFT to the top executives of the centers in May 1991 and it was seen as the task of the managing directors to inform their institutes and researchers.

There is another structural reason why the corporate level played a pivotal role and why it reacted promptly: Since specific formal positions determine action only at the top executive level of the organization, the institutes and researchers expect their management to be the first ones to cope with external trouble. This is also the case with regard to the association level, which is expected to maintain permanent contacts to the political actors. Hence, both the association and the corporate levels represent typical buffering functions, while institutes, groups and researchers make up the "technological core" (Thompson 1967: 19-24) of Big Science. Only later might the heads of institutes and individual researchers react, possibly to what they perceive as the "failure" of their management. When we compare the two buffering levels, we will observe a predominance of the corporate level over the association level. Empirically, the individual centers were the main recipients of information about the political measures; as we will see, arriving at a collective interest representation of all centers is a new step requiring several preconditions.[12]

4 The Interplay between Coping Activities

As the top *management of the centers* were the first to receive information about imminent budget cuts by the BMFT in May of 1991, coping efforts

12 Concerning the improbability of collective coping reactions in general, cf. the concluding chapter by Schimank and Stucke in this volume.

could first be observed at this level. But it is striking that in this early period of trouble the centers made no serious attempt to exercise their influence on the BMFT in order to *prevent* political interference. Considering the severe budget problems of the federal government and their own weak position, which still had to do with the nuclear energy image of Big Science in Germany, the top executives of the centers saw no real chance to avoid budget cuts. The managing directors of the centers, moreover, had no powerful allies: The other science organizations were their competitors for scarce resources, and the federal states with their 10% share of the financing of the centers had no real opportunity to influence the budget policy of the 90% financial backer (DLR 180892). Furthermore the federal states are always in an ambivalent position: On the one hand, they feel obliged to protect "their" centers; on the other, they see the necessity for a reform and even a reduction of the National Research Centers. It is this permanent conflict between science policy as research policy and science policy as regional policy which dominates the position of the federal states. In the case of Big Science, the states argue that a restructuring of this sector seems indeed desirable, but not as a result of an executive budget order by the federal government (WiMi 17071992). They joined the AGF in opposing the financial cutbacks, but their efforts were not very successful.

For that reason, with regard to the inner structure of his center, each managing director only tried to minimize the undesirable consequences for his entire center as a corporate actor. On the one hand, the directors aimed to satisfy the political actors and to foster the integration and research identity of their center by preparing concepts to reduce personnel, to reorient programs and, sometimes, to restructure organization. On the other hand, they informed their institutes and employees early in mid-1991 about the approaching changes in order to prevent inner conflicts and to gain acceptance for internal restructuring. At first, some of the centers considered it advantageous that the BMFT was only interested in achieving a general reduction in spending rather than at gaining substantial control over the implementation process. Surprisingly, however, they then decided that letting themselves each be subjected to the same rate of reduction was unjustifiable. They thus opened "Pandora's box," eliciting a flurry of political control activities from the BMFT. It was the centers themselves that called upon the BMFT to provide a detailed concept for its budget cuts which would take the specific needs and research outputs of the several centers into account. In autumn of 1991, the BMFT responded to this demand by developing a differentiated concept for the future of Big Science which distinguished between three classes of centers. In this way, as one managing director of a center put it (KFA 090992), the centers themselves supplied the BMFT with the "sharpened knife" for (possible) further cuts in the future which will lead to an unnecessary "exposure" and increasing competition between the centers.

But the unintended effect of the extended political control by the BMFT is not only due to the myopic, particularistic strategy of the individual centers trying to minimize their own losses at the expense of the other centers. This is reinforced by the strategic behavior on the *associative level* as well which demonstrates the peculiar structural position and role of the AGF. The AGF, founded in 1970 as a collective interest representation of the centers toward the political actors, could hardly fulfill this role over the last twenty years. This weakness is mainly due to the particularistic orientation of the various centers. Unlike the Max Planck Society or the Fraunhofer Society, the AGF represents heterogenous and, moreover, relatively autonomous research centers (with their own budgets). Therefore, we can expect particularistic coping efforts on the part of the centers to predominate rather than laborious attempts to come to a collective solution on the level of the AGF. At the association level, there is no executive authorized to pass binding decisions effective for the lower level of the individual research centers. The chair of the AGF is a member of the top management of one institute (at the moment the Managing Director of the DLR) who is elected for two years by representatives of all National Research Centers. In view of the "divide-and-conquer" concept of the BMFT, we cannot expect the AGF to find a common solution and formulate joint action with regard to these budget measures. What the AGF really did was to complain in general about the policy of the BMFT and to emphasize the value of the Big Science *model* for the future (AGF 11031992); it did not act collectively on behalf of the self-interests of one particular center. The AGF is not designed to fight, but rather to coordinate, as one managing director put it (KFA 090992). Consequently, the AGF supported a differentiated political treatment of the centers on the one hand, but denied the BMFT's request to take part in the discussion about the programmatic and organizational reorientation of the centers (AGF 07051993) on the other. Since there are, in fact, influential voices calling for the "burial" of the model of Big Science (FAZ 1992), the aim to defend the *raison d'être* of this type of research organization appears to be an important coping effort on the association level.

The reference point for the *institute or project group* is its competitive position within the scientific community: The group concentrates on acquiring and maintaining the infrastructure and financial resources necessary for continuing innovative work. Its goals are sometimes undermined by the coping activities of the managing directors of the centers. Coping strategies of the National Research Centers (e.g. decisions executed by their top management) signaling to the political actors that the centers would be willing to implement internal reforms in certain program areas in order to survive as a whole definitely meant trouble for certain project groups. It is inevitable that the management of a National Research Center will sometimes be forced to hurt the interests of several of its institutes in order to maintain the strategic capacity of the whole organization.

One coping strategy at the group level was to oppose the internal redistribution of resources if at all possible. The success of such a strategy depends on the relationship between the heads of the individual institutes and the management (Board of Directors) of the center. Since the Chairman of the Board of Directors of a National Research Center is always appointed by the BMFT (a political decision from the top down), the influence of an institute on executive decisions will be probably low. Therefore, the only way the institutes can cope with internal cutbacks is to look for allies outside of the center (such as project groups at universities or other research organizations) or cooperative partners with whom they can share resources and equipment. The situation for the institutes is aggravated by the fact that they cannot receive any project grants from the BMFT (with the argument that this ministry is the main institutional promoter of Big Science) and that they receive only very limited project funds from the German Research Foundation, whose main task is supporting university research. The same holds true for the European community funds, which are relatively scarce in many program areas with respect to the number of applicants. Finally, the institutes are referred to research contracts in industry. Since the research conducted by many of the centers is not oriented closely enough to the research demands of industry, however, this coping strategy is also limited.

At the level of *individual researchers*, who are mainly interested in improving their career options, we found three different coping patterns (GMD 220792). The first one is exit, meaning that the researcher leaves the institute and goes to another scientific organization or to industry. This option can be used only by very few researchers who are young and flexible enough to be attractive for other research areas. Another individual option is "exit" in the sense that the researchers of a certain institute or project group leave their team and change to another project group or institute within the same National Research Center. This option was used mainly in one National Research Center where the intraorganizational structure was totally changed.[13] A third individual strategy is an example for "defensive coping," that is, waiting for better times, which means continuing with one's own work and hoping that political priorities will change again in the near future.

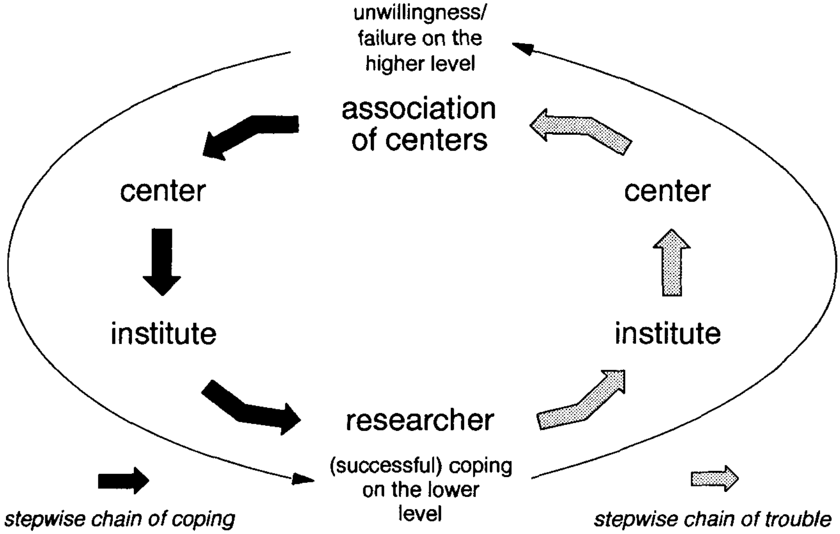
These multilevel coping constellations now become even more complex if we consider the unintended side effects the action on one specific level will have on the other levels of action (possibly causing a new type of trouble). To give two examples of such interdependencies: The exit option at the level of the individual researchers, for example, leads inevitably to a brain drain in an institute or a project group, and in turn affects the respective coping strategies at this respective institutional level negatively. The institutes or project groups therefore lose their micropolitical power vis-à-vis the management of the National Research Center or are no longer attractive cooperation partners for groups outside the center. On the other hand, these side effects may be regarded positively by a particular center because it may support the policy of the top management to reorganize or even close certain constituent institutes.

Another example may serve to illustrate this point. The policy of adaptation on the part of a National Research Center (i.e. willingness of the center's top management to fulfill some of the BMFT's expectations in order to survive as a whole) will inevitably lead to the delegitimation of some research areas, institutes and project groups. In effect, these areas will be "sacrificed" by top management. Obviously, this coping strategy and its side effect, the delegitimation, will have negative consequences for the coping strategies of the institutes or project groups within the center: If they are delegitimated by their own management, it is hardly conceivable that they will still find

13 Under the condition that none of the new institutes will acquire any additional positions for scientists it is rational for them to accept all applications from members of those institutes which are subject to cuts by political measures.

cooperation partners outside the center. Their means of coping are therefore restricted by the coping activities of the higher level.

Figure 1: Interplay Between Coping and Trouble on Different Levels of Action



So far, on the basis of empirical observation, we recognize the first symptoms of trouble, particularly at the individual and at the institute level caused by coping activities at the corporate level. But these symptoms might indicate a general pattern and a specific dynamic of coping and trouble throughout the different levels of action in the future (cf. Figure 1). From a higher to a lower level of action (association, center, institute, researcher), we can observe a stepwise chain of coping which activates the coping efforts at the lower level because of the failure or unwillingness of the next-higher level. Since the AGF fails to represent the collective interests of all members, each center chooses a particularistic coping strategy. But if this corporate coping is not successful or adequate from the point of view of the next-lower level of the institutes or project groups, the actors there have to launch their own coping efforts in order to guarantee the continuation of their research. Finally, if the institute's coping cannot guarantee the career interests of its researchers, these researchers will react individually to the threat to their professional future.[14]

If these different coping reactions at the different levels do, in fact, take place, we have an interesting example for a stepwise chain of trouble in the upward direction. For if there are some successful coping activities at the individual level (e.g. "exit" of the best scientists to other institutes), this will reinforce the trouble at the next-higher level because the institutes lose the very personnel necessary for the success of coping strategies such as arranging for cooperation with prestigious institutes or applying for project grants. But if, however, some institutes are successful – for example in buffering the reorganization plans of the management of the center – this may cause new trouble on the next-higher corporate level because the center management is demonstrating a lack of authority in implementing reforms and thereby satisfying the political actors. Finally, the centers which succeed with their particularistic strategy of minimizing their own losses at the expense of the other centers directly produce a delegitimation of the AGF and, in this way, a new (reactive) trouble which harms the scope of action on the association level.

In conclusion, we see that multilevel constellations play a pivotal role in the institutional perspective on science. Up to now, we often had analyses and theoretical concepts which concentrated on one of these levels. In the future, it might be fruitful to pay more attention to the interdependence of these different levels combined with a dynamic perspective on science, to show how actions and the interference of actions will *cause* structural effects, in turn causing new intentions and actions. In this way, we may also gain more insight into the scope and limits of political action in science.[15]

14 That means that there is sequential *logic* of coping actions. This does not exclude coping activities which occur *simultaneously* on the different levels of action, possibly because actors on a lower level anticipate the failure of the next higher level. But often we can assume that lower-level actors expect coping reactions primarily from the higher-level actors, who are thought to have greater organizational capacities to act strategically.

15 As Uwe Schimank and I have attempted to do; cf. the concluding chapter in this volume.

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Biotechnological Research in Germany: Problems of Political Regulation and Public Acceptance

Raimund Hasse and Bernhard Gill

Introduction: Scientific Trouble in a Risk Society?

The concept of "Coping with Trouble" deals on the one hand with trouble originating from resource cutbacks or from the political instrumentalization of research. These two kinds of trouble permit sharp distinctions to be drawn between the research system and its societal environment: the latter causes trouble by specific nonscientific influences which the former, in order to protect its functional autonomy, tries to cope with by buffering its technological core. This case study, on the other hand, looks at the extrascientific restrictions of the research system that are characteristic for research areas whose societal impacts are assessed as being inherently risky. These restrictions intervene in the form of political regulations or a lack of public acceptance directly into the technological core of the respective research areas. The methodical research strategies themselves are affected by this kind of trouble. While the lack of acceptance by the lay public and the restrictions of political, legal, and administrative institutions can affect the research system deeply, research actors themselves can affect each of these factors profoundly, too. Both the level of acceptance and the extent of regulation are primarily based on scientific participation: Politicians and government employees drafting formal regulations require expert advice to get information, legitimate their rules and achieve satisfying levels of implementation (Smith/ Wynne 1989; Jasanoff 1990). Scientific dissent, deviating points of view and whistle blowing influence public opinion and media reports substantially (Wiedemann 1990: 346; Rohrmann 1990: 330).

As far as genetic engineering is concerned, Germany seems to be the paradigm case of the tendency to react to possible risks of new technologies with stringent legal restrictions. In some scientists' opinion, these regulations are already endangering national research standards. From their perspective, both science and industry are being forced either to emigrate or give up their work in the field of genetic engineering. To avoid negative consequences for the economy and society, these scientists and representatives of the industry call for a lowering of the safety standards and the elimination of red tape (Max-Planck-Gesellschaft 1992, Büchel 1992). The deep frustration about the legal framework within which genetic research is now being conducted is surprising, since this framework grew out of a cooperative policy-making process incorporating a great deal of scientific expertise.[1] When the Genetic Engineering Law (*Gentechnik-Gesetz*, GenTG; see GenTG 1990) was enacted in West Germany, the extensive regulation of genetic engineering was seen as an acceptable modus vivendi. Scientists expected regulations to absorb any potential social and legal uncertainties (Brauer/ Stadler 1992; Winnacker 1992). But even though research actors have been continually involved in the process of drafting and implementing the regulations, the actual enforcement of the laws is perceived as being unexpectedly difficult to handle. Scientists have even publicized their resulting frustration, their protests and their conflicts with administrative agencies in the print media.[2]

Characteristics of the technology itself have often been used to explain precautionary, anxiety-laden regulative procedures of risk management in biotechnology (for example: Tait/ Levidow 1992: 220). This position will be contrasted here with a social and cultural explanation. We assume that the trouble with the implementation of the GenTG is grounded in the awareness of an extreme sensitivity on the part of the public which forces politicians and administrative agencies to react in a more formal and bureaucratic manner than usual, leaving little leeway for flexibility and good will. It has been

1 Many scientists were members of committees convened by the parliament, both as the regulations were being drafted and, later, when they were implemented. They were involved in the Study Commission of the German Parliament on the Opportunities and Risks of Genetic Engineering *(Enquete-Kommission des Deutschen Bundestages zu Chancen und Risiken der Gentechnologie)*, whose recommendations had the greatest cognitive (and legitimatory) influence on the Genetic Engineering Law and went on, in fact, to dominate the Central Commission for Biological Safety (*Zentrale Kommission für Biologische Sicherheit*, ZKBS).

2 See, for example, Korbmann's (1992) very impressive survey of many important molecular biologists' experiences with and assessments of the GenTG.

shown elsewhere that administrative rationality is not usually able to adjust totally to scientific rationality, because the former is oriented towards consistency with prior policies, while the latter insists on validation and ultimate adjustment to the "state of the art" of the sciences (Jasanoff 1989). In the field of biotechnology, these discrepancies in rationality have been amplified by the awareness that a very sensitive area is being explored. Therefore, it is rather improbable that an ideal process of mutual defining and shared understanding will emerge between technology promoters and regulatory agencies. We assume that this disparity is the reason for the risk aversion evident in the decisions taken by technology regulators.[3]

Section 1 deals with the legal regulation of scientific research as a specific kind of coping that is transformed into the trouble of restrictive and bureaucratic implementation by government officials. Measures taken, such as delays of certain kinds of research, the development of commissional ethics, or the scientific investigation of areas of uncertainty are interpreted as strategies aimed at guaranteeing a realistic maximum of autonomy. And even intermediate activities – such as voluntary adaptation to administrative recommendations, the participation in the process of drafting government regulations, and the representation of scientific expertise in the implementation procedure – may be analyzed as being directed towards this goal. In the case of molecular

3 This kind of cultural explanation may be distinguished from the most elaborate concept of a sociology of risk because it does not adapt to Luhmann's (1993) widely accepted thesis, which draws a sharp distinction between attitudes towards technological innovation manifested by risk-seeking decision makers and attitudes of the endangered lay public, whose concern is to minimize its exposure to danger. Such a fundamental distinction between risk and danger can hardly take into account the conflicts *within* the coalition of risk-taking decision makers – between science and industry, and between politicians and administrative officials – which have dominated the genetic engineering controversy. Furthermore, instead of reducing the effects of regulation within the policy-making process (merely) to different modes of incorporating technical expertise, a cultural approach takes further variables into account. The outcome of the regulative procedure may be influenced not only by the institutional structure surrounding it – which can be considered contextually determined (Jasanoff 1986) – but also, quite directly, by norms, values and attitudes connected with new technologies. Hence, by focussing on levels of acceptance, our explanatory shift also differs from political-science approaches focussing on the regulative practice as a function of institutional arrangement (Brickman/ Jasanoff/ Ilgen 1985; Vogel 1986) and on formal agenda setting (Otway/ Winterfeldt 1992).

biology, however, trouble emerges when modes of implementation are experienced as being restrictive and bureaucratic. It will be shown that the actions of government licensing and inspection agencies and the specific rationality behind them are difficult for scientists to comprehend. Furthermore, though molecular biology is regulated intensively and biotechnological catastrophes have not yet occurred, the field has not been successful in buffering scientific research from external trouble.

We assume biotechnology's low acceptance – rather than its difficulties with the legal framework in which it is embedded – to be the underlying reason for its trouble with government agencies. Therefore, we will deal in Section 2 with the normative structure of public awareness, its impact on active pulls of biotechnology, and its support by multiple-actor configurations including whistle blowing within the sciences themselves. Concerning acceptance, three shifts towards complexity can be observed:

- from technical risk to social and ethical impacts, rendering regulations – which have to be linked closely to scientific "Risk Assessment" – an inadequate means for achieving aspired levels of acceptance
- from public acceptance to promoter acceptance, which requires enthusiasm in order to assure resource acquisition, and
- towards the perception of the heterogeneous actor configuration of opponents.

Institutional forms of coping with this kind of trouble are analyzed in Section 3. On the *research community level*, we will examine the impacts of the closure of the risk debate within the sciences and the corresponding mode of risk communication as an educational program designed to convince the lay public and inform via mass media. We will compare the situation in Germany with what happened in the United States, pointing out discrepancies with regard to professionalization and procedural rationality which coincide with different levels of acceptance. On the *organizational level*, we will condense intranational variances into two analytically distinguishable paths of handling the problems of public acceptance: that of the hardliners and that of the more moderate actors. The far-reaching consequences of the moderates' path are interpreted as the most innovative mode of coping because they encompass a modification of the corporate identity of the scientific institutions who were willing to consider ethical and social criteria. Delayed release and even partial sacrifice in order to acquire legitimation are parts of this strategy.

In a deductive search for strategic considerations in the conclusion, the trouble of a lack of acceptance and dominating modes of risk communication will be examined in light of studies on technology acceptance. On the one hand we will show that the trouble faced by biotechnology can be explained by institutional and cultural factors whose significance is generally acknowledged. On the other hand, we will focus on an aspect illustrated by our case study which has not yet been considered with analytical rigor: the social preconditions of successful innovations lying beyond the scope of passive toleration and acceptable risk.

1 From Coping to Trouble: Whistle Blowing, Regulation and Implementation

1.1 "Normal Regulation"

By trying to prevent trouble, molecular biologists, in fact, produced it. Both the technical risk of recombinant DNA (rDNA) research – i.e. the feared danger to nature and human beings (including the scientists themselves) – and its repercussion as social trouble in terms of inadequate support and increasing legal restrictions were anticipated by the research actors and led them to actions which unintendedly initiated trouble. The conference that took place at Asilomar in California in 1975, at which collective whistle blowing played an important role, is a good example of scientists' anticipation of impending trouble. It demonstrated a kind of professional responsibility oriented towards two interconnected problems: fear about ecological and human health risks of research on rDNA, and serious concern about possible social consequences of these risks on the sciences (Krimsky 1982; Wright 1986; Lewin 1991). Thus it may be useful to investigate the sciences' contribution to the putative "cycle of anxiety, sterile debate, and bureaucratic regulation" (Davis 1984), though the attempts to regulate hypothetical risks by law were criticized by scientists from the very beginning of the debate (van den Daele 1990: 20).

A call for regulation first came from scientists conducting rDNA research themselves: Addressing his colleagues in the journals *Science* and *Nature*, the Nobel laureate Paul Berg suggested a moratorium on rDNA research. At the Asilomar conference, safety guidelines for DNA experiments were proposed. This initiative was institutionalized by the establishment of the Recombinant DNA Advisory Committee (RAC) at the US National Institutes of Health (NIH), which in 1976 issued the first *Guidelines for Research Involving Recombinant DNA Molecules.* Compliance with these guidelines was compulsory for projects receiving grants from the NIH, and privately funded research projects complied with them "voluntarily." From a formal point of view, the US safety institutions were imitated in the FRG: After Asilomar, the German Research Society *(Deutsche Forschungsgemeinschaft)* established the Senate Commission on the Safety of Recombining DNA *(Senatskommission für Sicherheitsfragen bei der Neukombination von Genen).* The commission proposed to the Federal Ministry for Research and Technology (*Bundesministerium für Forschung und Technologie,* BMFT) that a number of safety precautions be enacted. In 1978, the BMFT coopted the Central Commission for Biological Safety (*Zentrale Kommission für Biologische Sicherheit,* ZKBS) – comparable to the RAC in the US – and enacted the first version of the Guidelines for Protection from the Dangers of In-Vitro Recombinant DNA (*Richtlinien zum Schutz vor Gefahren durch in-vitro neukombinierte Nukleinsäuren,* ZKBS Guidelines), which were in large part a direct translation of the NIH Guidelines.[4] In each country, a majority of the members of the most relevant commissions – the RAC in USA and the ZKBS in the FRG – were biotechnologists whose task it was to monitor themselves and their colleagues; the safety guidelines were based on their proposals. But in contrast to those in the FRG, the self-regulative procedures in the USA were supported by participatory elements: Critical scientists from other disciplines were integrated into the RAC almost from the beginning; the RAC meetings were open to the public, all citizens had the right to introduce proposals or subjects for discussion; the records of the meetings were published, etc. Thus, the RAC could react flexibly to changing topics and demands, and the discussion within

4 It must be noted that attempts to pass federal legislation based on the guidelines failed, at first, in both countries. In the USA, the scientific community succeeded in thwarting the passage of such legislation in Congress and in several state legislatures (Herbig 1978). In the FRG, the BMFT planned to pass a bill based on the ZKBS Guidelines in 1979. This initiative was dropped by the BMFT after science and industry intervened. The situation changed as soon as industry called for regulatory legislation in order to clear up remaining legal uncertainties.

the RAC and the scientific community could be more easily transmitted to the public. The regulatory process in the FRG was completely different because the regulations were expected to be so effective as to eliminate any possible controversies over biotechnologies.

To clarify our argument – that neither regulations as such nor the actual scope of risk determine the extent of trouble encountered in implementation – it is useful to compare the GenTG with the execution of long-established, less politicized laws with a similar potential for restricting research practice. The Federal Infectious Disease Law (*Bundesseuchengesetz*, BSeuchG; see BSeuchG 1980), for example, was enacted in Germany in 1962, but its principal regulations do not differ greatly from the Reich Infectious Disease Law *(Reichsseuchengesetz)* of 1900. The BSeuchG spells out how public health authorities are to deal with infectious diseases. The safety requirements for laboratory work with infectious microorganisms are defined in Sections 19-29, which were originally formulated in 1917 for the Regulations Regarding Handling and Transporting Infectious Microorganisms *(Vorschriften über das Arbeiten und den Verkehr mit Krankheitserregern,* RGBl. 1917: 1069-1079). In terms of the objects to be regulated and the problems these objects can cause, these sections of the BSeuchG are very similar to the safety requirements for laboratories specified in the GenTG, but from our perspective they differ in one important respect: They are far less detailed and use a greater proportion of broad legal terms[5] that have to be interpreted in each individual case. Even though working with conventional infectious microorganisms is not known to be any less hazardous than working with rDNA,[6] the BSeuchG is not very strictly enforced. This may be due to the fact that authorities and scientists are not well enough informed about the law;[7] or maybe the authori-

5 in German *unbestimmte Rechtsbegriffe.*

6 More than 6,000 laboratory accidents with infectious microorganisms of natural origin were reported worldwide until the 1970s; 173 deaths were documented (Pike 1979). The genesis or introduction of new human pathogens since the 1970s also poses a threat in connection with experiments which, by definition, are not included in the category of "genetic engineering" (Kiper 1992). If we take into account the most harmful laboratory accident reported to date, which is thought to have caused cancer in seven researchers at the Pasteur Institute in France (Ravetz/ Brown 1989), it seems to be inappropriate to state that rDNA research harbors risks humans have never been subjected to before by any other kind of scientific research.

7 For example, interviews with public health officers responsible for the enforcement of

ties take too great advantage of their discretionary powers.[8] In any case, very few federal cases deal with violations of Sections 19-29 of the BSeuchG. And even these exceptions are not criminal cases, but disputes under administrative law as to which professional qualifications are necessary to get "approval for working with infectious microorganisms" (BSeuchG, Section 22).

In contrast to the BSeuchG, however, the GenTG is very formalized. Even at a first glance, the amount of detail in the GenTG's statutory provisions is remarkable. Because the topic of biotechnology was very politicized and the federal states *(Bundesländer)* had no experience with regulating biotechnology, there was much uncertainty, distrust, and suspicion. Pressed by the scientific associations, the lawmakers wanted to restrict the discretionary powers of the states and ensure uniform enforcement by state authorities throughout the country. In contrast even to the similarly contentious Nuclear Law *(Atomgesetz)*, the authorities are not permitted to use any discretionary powers in deciding whether to approve or reject a laboratory. Apparently this strategy was partly due to the fact that the federal government, a conservative coalition, feared that the "red-green"-governed states[9] would try to obstruct approval of genetic engineering. But the state authorities, too, contributed to the formalization. To implement the GenTG, biotechnology departments had to be established at the offices of 24 licensing authorities and 61 enforcement authorities within a few months. It is not surprising that the authorities, who had never been confronted with this subject before, were often not up to the new task. They had to employ many new specialists who, in turn, had little

the BSeuchG in Berlin showed that most of them had only a vague idea of the statutes relating to safety requirements for laboratories and, hence, how to monitor compliance with them (Gill 1989: 68-69).

8 For example, a laboratory which not only conducted research involving genetic engineering but also investigated pathogenic viruses was completely unaware that it was required to make a legal declaration of its activities; failing to do so is a punishable offence. The necessary approval was given to the head of the laboratory retroactively in 1989, when it was already publicly known that the corresponding experiments had begun three years earlier. In a parliamentary inquiry, the local health authorities called this a borderline case, "because even among experts it is controversial whether a certain strain requires government approval in accordance with BSeuchG Section 19 or not" (AH-KA 11/273, Berliner Abgeordnetenhaus, 11. Wahlperiode, Antwort auf die kleine Anfrage Nr. 273, Seite 7).

9 Red-green refers to a coalition between the SPD (the Social Democratic Party, associated with the color red) and the Greens (an ecologically oriented party).

or no administrative experience. On the other hand, the officials responsible for licensing and enforcing the law were only vaguely familiar with the subject of biotechnology and unable to evaluate many technical details. Hence, the detailed regulations of the law were followed by a flood of equally detailed application and evaluation forms. Formalization resulted here from the insecurity of the newly employed specialists within the administration, of the administration in dealing with the applicants, and of the administration when confronted with a suspicious public. Everyone was trying to avoid making a mistake.

Before the GenTG was enacted, science had not been troubled by restrictions such as the BSeuchG because government authorities possessed broad discretionary powers in enforcing these legal restrictions. The laws were interpreted generously and often enforced in a way favoring the free development of science and technology. The corset of formalization which is characteristic of the GenTG, however, makes such interpretations difficult. Even the adaptation of licensing procedures to new scientific findings is endangered.[10] In this context, recent complaints of some of the genetic engineering labs about regulatory decisions they considered arbitrary may produce counterintentional results if they are combined with demands for further formalization.[11] Even if research reveals, for example, that fewer safety precautions are necessary than had originally been thought, authorities will have to continue to enforce the regulations to the letter if stringent formalization exists.

1.2 Implementation and Bureaucracy

Extensive formalization is supposed to ensure that regulations are clear to everyone, and that regulations guarantee the legal certainty of administrative

10 Hence, especially in dynamic fields of technological development, many experts of jurisprudence recommend making regulations more flexible by means of procedural or reflexive law in order to facilitate fast improvements in the details of enforcement (Ladeur 1992).

11 See, for example, the Max Planck Society's call for a restriction of "existing discretionary powers of the authorities which also can be and are used heavily to the disadvantage of scientific inquiry" (Max-Planck-Gesellschaft 1992: 13).

procedures. The complexity of the regulations and, hence, the details of enforcement are supposed to buffer the scientists from a latently suspicious public. But in the social context of controversies, formalization seems to have the unintended effect of causing further trouble because it impedes the discussion of the controversy itself. Formalization can hardly suppress disputes. A hearing on the deliberate releases of genetically modified sugar beets and potatoes that took place in Northeim at the beginning of 1993 can serve to illustrate this point. The chairman of the meeting (who had been commissioned by the licensing authority) tried to tie the contributions of the critics down to the agenda and to involve them in a discussion about the prerequisites for licensing, i.e. about the technical details of the specific cases. Many critics, however, expressed very general ethical and political concerns upon which they based their opposition to biotechnology and deliberate release as such. Moreover, the critics demanded that the order of the agenda be changed, requesting the chairman to state the alleged benefits and, then, the risks of the deliberate releases. In order to limit the length of the discussion, the chairman tried to cut many contributions short by referring to the agenda. Narrowly legalistic disputes arose repeatedly, causing the discussion of individual topics to last longer than expected. When the chairman finally shifted to a more flexible handling of the proceedings, the discussion became more focussed and meaningful.

Skepticism toward biotechnology also tends to influence administrative procedures; elected public officials issue guidelines, for example, or career administrators bring their own private opinion into their work. In the case of less formalized laws, attempts at regulation by the administration rely on material risk assessment and, hence, on scientific expertise usually provided by members of the very scientific community which is to be affected by the planned regulations. In the case of more formalized laws, it is easier for the administration to restrict the researchers' activities on the basis of a trivial formality: the more letters (literally) a law includes, the more likely it is that empirical reality will deviate from the "letters of law." Finally, formalization may provoke displeasure and overreaction among scientists affected by the regulations, leading in turn to formal administrative reactions and further suspicion on the part of the public: Even the biotechnologists who consider the regulations of the GenTG to be reasonable complain about the "flood of forms to be filled out."[12]

1.3 Modes of Arrangement and Confrontation

Biotechnological researchers can deal with the local authorities' attempts to monitor their activities in several ways. For example, they can use methods which are not formally defined as "genetic engineering." The bacteria constructs used in the first field experiment with genetically manipulated organisms in the FRG in 1987 were declared to be "not recombinant" according to the ZKBS Guidelines in force at that time. The ZKBS hence abstained from an assessment. When experiments with the same constructs were carried out in the United Kingdom and in France, they were, however, classified as "recombinant" (Dickman 1987). But because Section 3 of the GenTG provides a relatively precise definition of which procedures are to be classified as "genetic engineering," the possibilities to use ad hoc definitions to evade regulation are restricted here – as opposed to laws like the BSeuchG which operate with broad legal terms to be interpreted case by case.[13]

Unlike the multinational companies, publicly financed research organizations do not have the option to avoid the German safety requirements. Individual researchers have the option to apply for positions in foreign countries, but this only makes sense if they can offer those countries special expertise not otherwise available there. Less developed countries are less appealing because they neither have adequate research facilities, nor do they offer good salaries. Another alternative for German biotechnologists is to try to perform their experiments in foreign research partnerships. But then the reputation

12 When city trade inspectors in Marburg discovered a laboratory that was not licensed, the laboratory staff refused to make any kind of statement. As the head of the Inspector's Office later reported, the "more formal offense" (that a move within the institute had not been declared) was not the reason the public prosecutor's office was called in, but rather the sum of the lab's safety violations and the project head's "very uncooperative behavior" (*Frankfurter Rundschau*, 21 November 1992, p. 22; cf. also *Frankfurter Rundschau*, 20 November 1992, p. 27).

13 Another way to bypass the GenTG is to avoid experiments requiring higher safety standards. However, this might also be due to the fact that specific experiments are now defined as less hazardous than before.

must be shared with the foreign partners, too, and the researchers risk giving the impression that their work is somehow illegitimate if it is all too apparent that the partnership is being used to avoid the German safety regulations.

Safety requirements, however, can also be ignored. It is hard to assess how successful this practice is because it only becomes public when it fails. In the USA and the FRG, several cases of such offenses have come to light. Aside and apart from whether – and to what extent – it is possible for researchers to obscure their own responsibility or to influence the sanctions imposed if they are caught, these offenses justify even tighter controls. The remaining gap between norm and reality will probably depend to some extent on the public acceptance of genetic engineering and its risks. Since the confrontational stance which biotechnologists presently take in attacking the GenTG is probably counterproductive as far as the direct contact with government authorities is concerned (see Kahn 1992), the more pragmatic strategy for the researchers is to cooperate somewhat with the authorities. At a larger research institute, for example, the employee charged with ensuring compliance with the safety requirements advises his colleagues on how to fill out the application forms and evaluation sheets. It can be assumed that he not only does the task assigned to him, but passes on his experiences regarding what type of information will facilitate the "smoothest" communication with the licensing and inspection authorities. All in all, many of these officials seem to respond by being very cooperative and generous.[14]

In summary, it can be stated that the GenTG as it is presently implemented does not fulfill one of its functions, which is to buffer research from public criticism. Bureaucratic rationality and continued observation by politicians, the media and the lay public seem to be mutually stimulating each other in their troublesome effects – at least for the time being. "Usually" the authorities have no incentive to tighten controls of the persons and institutions they are called upon to inspect; hence, they do not discover too many offenses; and, hence, the public and politicians do not become aware of the subject and do not call for tighter controls. But in our case, the public and, then, the

14 Regarding the enforcement of the GenTG, the head of the Berlin Genetic Research Center *(Berliner Genzentrum)* stated at a press conference it held together with the Berlin authorities in 1992 that his experiences with licensing and inspection procedures as conducted by the Berlin authorities were quite favorable (see *Pressemitteilung [des Berliner Genzentrums]*, 19 August 1992).

politicians were alert from the beginning; hence, detailed formal regulations were enacted which came into conflict with the reality of research; hence, these conflicts were publicly discussed and gave cause for further formalization. Instead of the usual spiral of negligence and ignorance, a spiral of distrust and excessive monitoring has emerged. Individual and collective research actors have tried to adapt to the GenTG and its enforcement. But this cannot be seen as successful coping, because the described implementation problems are of secondary importance. The real, more complex trouble – as indicated above – is the underlying lack of societal acceptance which causes the precautious and formal implementation of the GenTG.

2 The Lack of Acceptance as Complex Trouble

2.1 Beyond Regulation: The Research Actors' Responsibility

Because the main material functions of regulation of risk are to attempt to avoid catastrophes and to mitigate undesired consequences of technological innovations, some risky activities are generally forbidden, some have to adhere to certain rules such as monitoring or operating standards, and some have to be framed by further research in order to reduce uncertainty by generating new knowledge (cf. comparison by Morone/ Woodhouse 1986: 14-120). Moreover, as an integral part of modern technological systems, regulations serve to fulfill requirements of public acceptance. In the case of risk, the regulative framing often seems to be an important prerequisite for the acceptability of new technologies. Their regulation can therefore be interpreted as a reaction to two interconnected but different problems the political system is expected to address: the avoidance of accidents and the generation of public acceptance.[15]

15 "Risk regulation is as much about containing fears as preventing environmental harm" (Levidow/ Tait 1991: 271). Similar conclusions have been drawn from regulating pharmaceuticals (Bodewitz/ Buurma/ de Vries 1987) and chemicals (Rip 1990). For a general discussion, see the conclusion of Wynne's profound analysis of hazardous waste management (1987: 356-397).

Risky technologies have to be implemented carefully, as the advisory part of the study by Morone/ Woodhouse (1986: 150-175) suggests. They argue that coping should start with restrictive regulation and the generation of knowledge in the case of assumed high degrees of uncertainty combined with expected potentials for catastrophe. Genetic engineering is treated as an example of the successful introduction of such a risky technology in the USA: The precautionary character of the regulation of rDNA in the 1970s made it possible to lower the standards without endangering the society with accidents. Because of the absence of major accidents, this lowering of safety considerations did not cause public fear, so that at the end of the 1980s, the technological risk of genetic engineering was no longer a subject of political controversy. Following their argumentation, compared with other technologies such as toxic chemicals and nuclear energy, public acceptance in the case of rDNA has been generated as a by-product of stiff regulations which also serve as a symbolic means of publicly demonstrating the regulators' determination, enabling them to gradually lower their standards.

In terms of avoiding accidental biological hazards in the past and present, the regulation of genetic engineering in Germany seems to be as successful as it was in the USA. But many argue that it would have been possible to achieve the same result at much lower cost. What is quite obvious is that worst-case scenarios have not been experienced yet, so that it is legitimate to interpret regulatory activities as successful catastrophe-avoidance measures – in Germany and in the USA. In Germany, however, the risk issue still seems to be on the agenda. Therefore, the attributed second function of risk regulation does not seem to have been fulfilled. The acceptance of genetic engineering in Germany is perceived as still being low, and concerned actors in science and industry blame society as a whole or relevant parts of it for obstructing research on rDNA. As far as their normative expectations are concerned, politics has failed (Brauer/ Stadler 1992: 3-4; Büchel 1992: 7).

The lesson deductively drawn from the analysis of Morone/ Woodhouse (1986; 1989) would be to intensify regulatory activities even more until the technology is accepted. But, considering the situation in rDNA research in Germany today, this conclusion may even be called sarcastic, because science and industry already view many standard regulatory procedures as being overly restrictive. So far in Germany, regulatory restrictions have been perceived as being "fundamentalistic" and detrimental to research in genetic engineering; nevertheless, public acceptance is far beneath the level aspired to by the technology promoters. This indicates that regulation seems to be an inadequate means of generating public acceptance. The failure of regulation to buffer the focussed social trouble directs the analysis away from politics and administration back to the research system as the concerned and responsible institution. But before we turn to the coping of scientific institutions, we have to consider the problem of low acceptance with more analytical rigor and ask why the restrictive, risk-oriented outcomes of the regulative procedure have been unsuccessful in absorbing it.

2.2 Beyond Technical Risk: Social and Moral Assessment

Even if modern science is identified as an institution enjoying great autonomy, it is expected to produce research results that will help to solve societal problems by generating new knowledge. It is now more obvious than ever before that the positive effects of scientific advancement can also harbor technological risks, endangering the health of mankind and causing ecological damage. On the one hand, the transfer and application of scientific knowledge may lead, for example, to risky implementations of new technologies and to abuse. On the other hand, the scientific process itself is often assessed as being dangerous because a total containment of risky experiments seems to be impossible. The controversy indicates that fears about genetic engineering are often directed at the latter type of risk as well.

From the very start, recombinant DNA research techniques were identified as being potentially risky.[16] Fears focussed on unknown consequences of the release of genetically modified organisms. At the beginning, the scientists themselves and the neighborhoods in which the research laboratories were located were most worried about potentially harmful effects. Protests were organized which were supported by local media and by some scientists who were skeptical about the possibility of avoiding catastrophes.[17] Their de-

16 This controversy began in the USA, but it existed in Germany, too – soon its dynamics, no longer contained by national boundaries, affected the subsequent debate everywhere. Thus, it is not useful to draw sharp national distinctions. Especially the effect of Asilomar – legal regulation and an alarmed public – should be considered a transnational phenomenon.

17 See, for example, the case study on Harvard University's proposal to renovate biological

mand was a moratorium on all rDNA activities – at least "now," and especially "here."[18] Social movement organizations began to establish the topic of hazardous consequences of genetic engineering. Reports in newspapers, radio and television concentrated on the potential catastrophes and accidents rDNA technologies could cause.[19] In connection with the diffusion of knowledge about genetic engineering, a shift of attention occurred: Arguments against the diffusion of genetic engineering were based not only on potential accidents. In addition, the consequences of a successful implementation were reflected and assessed in terms of their social and ethical impacts.

Though the narrow risk debate was increasingly overshadowed by fundamental beliefs concerning technological progress and its benefits to mankind, there still was hope of being able to end the debate not only by assuring genetic engineering's safety, but also by referring to its positive impacts.[20] Unfortunately, the more intensively the social and ethical consequences were put on the agenda of public acceptance, the more the established, socially

laboratories in order to conform to the genetic engineering safety standards of the National Institutes of Health (NIH) and the corresponding reactions of the neighborhood public (Krimsky 1986: 14-16).

18 Most often resulting, at least, in delays of research, which were partly recommended by regulative agencies or commissions and partly voluntary. See, for example, the case study on Ice-Minus (Krimsky/ Plough 1986: 75-121).

19 This is not to say that biotechnologies were consistently treated in the media as being harmful. Instead, reports weighing the opportunities and benefits could be observed as well as ones focussing on the risk and negative impact (van den Daele 1990: 40). Nevertheless, media reports have even been blamed for being partly responsible for low degrees of acceptance, demonstrating what Peters/ Hennen (1990) call the *hostile-media effect*. See, for example, Arnold/ Domdey (1989: 16): "In the overwhelming majority of the reports, the emphasis is on illustrating the unintended applications of genetic engineering ... The innumerable examples of positive applications, however, ... are either disregarded completely or mentioned, at best, in an aside. These bad seeds sown by the media have born the desired fruit: genetic engineering has a disastrous image" (translation by the authors).

20 For the USA see, for example, Hanson/ Nelkin's conclusion of their discourse analysis of Congressional hearings about transgenic animals: "While scientists and those who work on technological applications of scientific advances define their projects in terms of technical elegance and economic benefits, groups as diverse as creationists, ecologists, anti-abortionists, and animal rights activists oppose scientific impacts on moral and ethical grounds" (1990: 80).

concerned organizations entered the stage.[21] These collective actors – such as churches, women's organizations and third-world groups – differed in two ways from the original protesters concentrating exclusively on hazardous risks concerning their own well-being. First, the new opponents organized in a way that could hardly be called fragile and ad-hoc; second, they have an almost institutional character and are endowed with the competence and legitimacy to make authoritative assessments of social and ethical issues.[22] As a result of the broad rDNA controversy, it could no longer be said that the protest against genetic engineering technologies was being pushed by a concerned minority. Hence, potential promoters of the technology in the political and economic system also had to worry about the trouble with a lack of acceptance by the lay public. This led to a new problem for the research units of genetic engineering: the loss of a supportive climate in terms of resource acquisition caused by decreasing motivation and commitment of political and economic agencies to push the technology. Political support and market demand lost their momentum because the future success of genetic engineering was now thought to be highly uncertain.[23]

2.3 Beyond the Lay Public: Promoter Acceptance

In order to understand how this second shift caused high degrees of complexity in the acceptance dimension, we must look at the peculiarities of the distribution and potential usage of rDNA technologies. These characteristics of

21 Note that the debate on rDNA in Germany became public at this advanced stage of the controversy. Thus, reflections about ethical and social impacts were involved here from the beginning of the public debate. For a summary of the relevant actors in the German controversy and their specific concerns about rDNA technologies, see van den Daele (1990: 37-38), who argues that the diversity of protest actions refutes the appropriateness of labelling the protest as a coherent social movement.

22 For a third consequence, see Tait/ Brown/ Carr (1991: 52-53): the shift from NIMBY-founded opposition ("not in my backyard") to the more fundamentalist NIABY approach ("not in anyone's backyard"). While the NIMBY faction seems to leave room for negotiations and compromise, the NIABY proponents are quite resistent to such influences.

23 It should be noted that, in addition to low public acceptance, technical problems were encountered in the application of rDNA technology. Nevertheless, they may be related to the high expectation levels of use and benefit which will now be considered.

rDNA technology differ fundamentally from nuclear energy, for example – another technology whose implementation has led to similar kinds of trouble. Because there is a wide range of genetic engineering technologies which can either be part of a product or a process, applications might be useful for many small innovations, but indispensable only for a few actors. Another important aspect is that the material results of rDNA techniques are usually put directly onto consumer markets, so that the degree of immediate acceptance by individual consumers, retailers, wholesalers, and farmers is a much more relevant factor influencing decision makers than it has been in the case of nuclear energy (Tait/ Brown/ Carr 1991: 51). The success, therefore, of genetic engineering is dependent not only on consumers' passive tolerance, but on a kind of positive acceptance encompassing an active demand for products based on genetic engineering (Yoxen 1982: 139). But how do these matters of technological innovation and diffusion affect scientific research?

In the case of genetic engineering, the proximity of basic research to modes of application caused science to become dependent on industry and government support for external funding. Molecular biology has been treated as an ideal example of the changes which can occur in connection with such dependencies (Markle/ Robin 1985; Grobstein 1985; Krimsky/ Ennis/ Weissman 1991). The effect of direct contact with nonscientific institutions on the cognitive structure of the sciences is not known precisely. While some believe that there really is a growing amount of applied research, others observe that research areas are only labelled as having significant applications, being vital to technical innovation or promising future benefits in order to acquire external resources for intended basic research (Blumenthal et al. 1986; Peters 1989; Kleinman/ Kloppenburg 1988). But the – factual or apparent – compatibility of basic research with nonscientific criteria requires a modification of the corporate identity of scientific institutions (Ruscio 1984: 267-268). They develop a need for high levels of public acceptance in order to assure their legitimacy. Merely drawing attention to the acceptability of technical risk will not satisfy these requirements.[24] In addition to minimal standards of public

24 As Ruscio puts it: "The safety issue has passed" (1984: 269). See also Lynn/ Poteat/ Palmer (1988: 111): "The concern of much of the debate about environmental applications of biotechnology has been on the issue on environmental risk or risk to human health. This initial focus on scientific and technical hazards is typical ... However, ... to ignore or dismiss the ethical and political aspects of biotechnology, to focus public

acceptance, the promoters of genetic engineering have to generate enthusiasm for the new technology among actors potentially responsible for the application and diffusion of research products and processes. These pull mechanisms are especially important in order to enlarge the coalition of technology promoters and to generate support in terms of resources (Holtzman 1985). The more basic an innovation is, the more formidable the barriers are that have to be overcome. Thus, promises concerning the advantages of the new technology tend to exaggerate. In the case of genetic engineering, slogans like "revolutionary character," "goal-oriented engineering," and "man-made nature" were used in presentations – characteristics diametrically opposed to those emphasized within the context of the acceptance debate when public legitimacy was the main goal.[25]

The need for support in order to acquire resources and the concern of the lay public about harmful effects cause a dilemma of acceptance: The need for support makes it necessary to represent rDNA technology as being basically new and offering revolutionary applications; the public's concern makes it necessary to downplay the extent of its innovativeness. While the public acceptance debate forces scientists to dispel fears about the biotechnological applications expected to intervene in "nature's plan," the sponsors' acceptance can only be achieved by promising just such incisive innovations. Thus, the search for successful strategies that will ensure promoter acceptance endangers the public's acceptance.[26] Because there are inherent limits to the flexibility with which opposing qualities can be attributed to one and the same technolo-

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policy discussions primarily on the question of risk, is to prolong controversy. To deprive the field of this type of discussion is to miss the opportunity to create the legitimacy which is necessary for biotechnology to proceed both safely and with the public's trust."

25 This discrepancy made it all the more difficult for promoters to establish the technology's revolutionary image. See the conclusion of "a crisis of confidence in the revolutionary nature of biotechnology" (Tait/ Cathaway/ Jones 1990: 297).

26 Meanwhile, this aspect is openly reflected by molecular biologists. See, for example, Gassen's self-criticism (1990: 104): "Many of the false conclusions and misunderstandings that are going around are really our own fault ... We had the audacity to think that some day we could manufacture an 'outer space person,' and we glorified genetic engineering using the expression 'The Eighth Day of Creation.' We wanted to use genetic engineering to cure cancer, eliminate hunger in the Third World and reconcile ecology with economics. It is probably justified that we were accused of being naive or even collectively stupid for exhibiting such shallow positivism" (translation by the authors).

gy, the acceptance debate undermined the research actors' attempts to present rDNA techniques to potential technology promoters in a way that could spark their interest and inspire them to make a confident commitment. In order to cope with the trouble of low acceptance, the image of a revolutionary breakthrough in plant breeding and agriculture via genetic engineering was sacrificed.[27] But as an unintended side effect of more moderate representations, incentives for political and economic actors to join the group of promoter agencies decreased further. As far as the expectations regarding a successful diffusion of innovative technologies are concerned, there has been a decline of interest in pioneering applications of rDNA technologies; now they are not expected to replace, but only to complement traditional methods.[28]

2.4 The Shift Towards Environmental Complexity

Lack of promoter acceptance is a crisis which is not only directly caused by risk debates and deficient lay acceptance. But in a phase of hesitation and inertia, in which the demand that would pull the technology is weak, research actors as well as economic and political actors put the blame for the trouble solely on the public fears. Because of the hesitation of potential promoters, research units are confronted with problems which traditionally have been buffered by the political system. The political system, however, cannot solve the problem in the traditional way by providing technology support. Though there might be a relative consensus among experts to assess the risks as toler-

27 Levidow/ Tait analyze this as a shift from revolutionary to evolutionary metaphors that "can be understood partly as a response to public fears about novel organisms degrading the environment and/or industry controlling human destiny" (1991: 275). Note also the semantic shift in presentation, from genetic-engineering metaphors to the now more frequently used term "genetically modified organisms," which was a response to criticisms of engineering attitudes towards nature that had emerged as rapid advances were made in molecular biology (Herbig/ Hohlfeld 1990).

28 Though the "trend at the moment is such that industry and regulators appear to be paying greater attention than ever before to public attitudes in deciding which new products to develop or to approve" (Tait/ Brown/ Carr 1991: 47), for the industry there are much tighter and more traditional parameters requiring great precaution: in-company constraints, market constraints and – plainly – the lack of profit. For a summary, see Tait/ Cathaway/ Jones (1990: 297-301).

able, ethical and social consequences still stay on the political agenda. Hence, all parts of the "Technical System" (Shrum 1986) of genetic engineering are negatively affected. It is not only the political system's own logic and constraints tending towards restrictive regulation combined with industry's hesitation to support the development of the technology that block acceptance, but also the scruples concerning scientific progress expressed by members of the scientific community (Aretz 1992). For the technology pushers among the scientists, it is difficult to grasp the multifaceted arguments and the heterogeneous actor configurations of opponents making use of them. Their environment seems to be unstructured and diffuse.

When we try to analyze how problems of low public and institutional acceptance affect scientific research, we find different single mechanisms interactively culminating in conditions that are extremely difficult to cope with simultaneously. Research actors are confronted with political and administrative restrictions and with decreasing demands by industry, both of which seem to be grounded in the diffuse hostility of the lay public, whose fears are mainly perceived in terms of critical mass media reports that overestimate potential accidents and amplify moral concern. Furthermore, social movement organizations stage sensational protests in order to make their point of view well known. Last but not least, there is a minority of scientists perceived as whistle blowers as soon as they make use of their expertise to analyze technical risk or even join the public debate about ethical and social consequences. We conclude from our brief analysis that the trouble with acceptance is structured in a diffuse way. There is a heterogeneous mixture of arguments and expectations that makes coping rather difficult. The shift from technical risk to social and ethical consequences means that technical expertise no longer suffices to provide competent assessments of the technology's impact. If the commitment, enthusiasm and expectations needed to guarantee sufficient support from political and economic actors are lacking, strategies are called for that are fundamentally opposed to those appropriate for gaining acceptance of the lay public. So, besides the loss of competence, there is the dilemma of conflicting demands on the way the technology is presented. Not one, consistent rhetorical approach is required, but rather the decoupling of arguments and hypocrisy (Brunsson 1989). But this is limited by the risk of further distrust and loss of acceptance (Wynne 1988). And, thirdly, the actor configuration is structured in such a way that it is incompatible with established procedures of problem solving in science, i.e. participation of the scientific establishment in corporative settings that have been labelled hybrid communities (van den Daele et al. 1979). Their heterogeneity makes an involvement in multiple arenas with different participants desirable. All together, the acceptability of genetic engineering is a rather new and complex kind of trouble.

3 Coping via Professionalization and Organizational Learning

In this section, we will focus exclusively on the coping of scientific institutions: first, on that of the science system organized on the basis of communities and professions and, second, on that of research organizations. Certain processes and structures will be interpreted as reactions to the severe trouble of the lack of acceptance. Though there is no general coping strategy that can be attributed to one collective actor, and though there is probably no one best way to solve the problems mentioned, there are relevant patterns of action which have the effect of coping.[29]

3.1 The Community Level: Closure versus Professionalization

As the Asilomar example indicates, critical reflections within the sciences were the initial stage of the protest against the risks of genetic engineering. In the meantime, there has been a closure of the risk debate inside molecular biology.[30] Since that debate ceased, the majority of natural scientists – espe-

29 While the subsection on professional coping uses statements by biologists as they have been documented in the print media, the subsection on organizational coping is based on some 50 interviews with molecular biologists and plant researchers conducted by one of the authors (Hasse/ Hohlfeld/ Nevers 1992; 1993). Because the transcriptions have not yet been analyzed systematically, these subsections do not claim to contain more than heuristic impressions which must be validated by further empirical research.

30 For a detailed analysis of the technical arguments see Krimsky (1982). For a reconstruction of this process from a political scientist's perspective, see Wright (1986). Note that it is not important for our argument whether the closure of this debate was the product of negotiations drawing heavily on suggestive antiscientific modes of assertion (as indicated by Wright 1986), or rather a result of scientific reasoning based mainly on Mer-

cially the establishment[31] – judge the risks of genetic engineering as being acceptably low. From their perspective, the fears of the public and the social dynamics of the risk debate outside the sciences are beyond the scope of scientific reasoning. But a minority of concerned scientists and whistle blowers is trying to put risk issues back on the agenda and is thus blamed for low acceptance and its consequences.[32] Critical stances are considered to be politically motivated, and expert criticism of genetic engineering is accused of expressing opposition to Western progress in general.[33] This closure of the debate has led to an institutionalization of the controversy within the scientific community characterized by fixed, value-laden positions and lacking in informational exchange.[34] The opposing minority is isolated, and the scientific establishment is confronted with the problems of low acceptance of genetic engineering by the lay public as mentioned above.

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tonian norms (as suggested by Morone/ Woodhouse 1986).

31 For an analytical concept of scientific establishments that incorporates ideas about the stratification of scientific communities, see Elias et al. (1982).

32 See, for example, the molecular biologist Müller-Hill in a discussion between biologists (in Gassen et al. 1989: VIII).: "With your wonderful criticism you have actually managed to keep this branch of industry from succeeding in the Federal Republic. Now, all you are lacking is the second victory – ruining basic research as well" (translation by the authors).

33 Apart from any cognitive considerations, consensus within the scientific community is achieved by isolating one's opponents and discrediting their motives. See, for example, the conclusion by Arnold/ Domdey (1989: 14, translation by the authors) from the Munich Genetic Center *(Genzentrum München)*, following their considerations about biotechnological risk and the demand to reverse the burden of proof: "Since we assume that a majority of the critics is quite familiar with the results above, we must assume that ... the main goal of a large number of them is to do away with free society altogether, and that they view genetic engineering right now as a handy instrument with which to achieve this goal." In a similar vein, the chairman of the ZKBS, Peter Starlinger, stated: "Those who question the basic right to conduct research and to obtain and diffuse knowledge today will want to censor the press and outlaw demonstrations tomorrow" (*Ethik und Sozialwissenschaften* 1991: 582, translation by the authors).

34 See, for example, the biologists' controversy about recombinant DNA in Gassen et al. (1989); see also the biologists' comments on Starlinger's provocative essay in *Ethik und Sozialwissenschaften* (1991: 583-635), which can easily be divided into total agreement on the part of the highly reputed members of the scientific establishment (Mohr, Schell) and total disagreement on the part of "critical" scientists who, in the meantime, have disappeared from experimental biological research (Hohlfeld, Kollek, Albrecht).

In discourses between actors of the scientific establishment and the lay public, the establishment conveys the zero-risk assumption with scientific authority. This kind of coping is part of an attempt to find teaching methods adequate for educating the public in order to change its attitude. The simple message to the public is: "I am the expert; you have to believe me; there is no risk (worth mentioning) you have to worry about." As an unintended effect of such educational coping, the risk debate outside the closed community can develop in a way which cannot be controlled by a simple proclamation of zero risk. Concerned scientists and whistle blowers become extremely influential. In their capacity as scientific experts, they argue that the claim that genetic engineering presents no risks can be disproved. As a consequence, public fears increase because the technology promoters do not seem to be trustworthy. The dynamics of this process resemble a vicious circle. Problems of acceptance provoke scientists to make very far-reaching statements about the technology's safety. But precisely because they are so expansive, these statements become easy targets for public scrutiny (Wynne 1988). It may be concluded that the risk debate probably cannot be absorbed by scientists who are considered by many observers to be neither impartial nor trustworthy.

This situation has something to do with the fact that institutionally separate autonomous communities devoted to risk analysis and assessment are lacking in Germany. A comparison with the situation in the United States indicates that coping potentials used there affected the acceptance problem in general and the risk debate in particular in such a way that genetic engineering has evidently become a much less prominent issue. In the USA there was an institutionalization of a separated risk assessment community in the 1980s, resulting partly from negotiations during the implementation processes, partly as an element of regulation itself. In effect, risk issues were dealt with scientifically by a community extending beyond molecular biology.[35] In order to formalize the analysis process, special concepts of Risk Assessment were developed in the mid-1980s.

- The Cornell University Ecosystems Research Center and the Institute for Comparative and Environmental Toxicology could base their work on advanced models of chemical risk assessment (Gillett et al. 1986).

35 For the history of the Risk Assessment of rDNA technologies and their release, see Strauss (1991: 298-307); Levin/ Strauss (1991: 13-16).

- The Massachusetts Institute of Technology took an interdisciplinary approach based on input from natural scientists, engineers, lawyers and political scientists (Strauss et al. 1986a; 1986b).
- The National Science Foundation funded the Risk Assessment approach developed by Covello and Fiksel (Fiksel/ Covello 1986), who later even tried to make use of knowledge-based systems of artificial intelligence (Fiksel 1991).
- Further risk assessment approaches for biotechnology were developed based on mathematics (Gintzberg 1991) and ecology (Mooney/ Bernardi 1990).

If we consider this formalized Risk Assessment in the USA while looking at the different acceptance levels of rDNA technology in the USA and Germany, we see an indication that there are social and cognitive reasons why a strategy is likely to fail that deals with risk issues only within the boundaries of the scientific specialty that is expected to cause them. We can conclude that if risk debates are to be conducted at a professional level, they must be based on assessment by independent – in Mertonian terms: disinterested – scholars from related fields who are able to claim both technical expertise and moral trustworthiness (Barber 1983; Abbott 1991).

But the risk dimension is not the only factor in the acceptance debate. As already mentioned, there is also serious concern about ethical and social impacts. A strategy of professionalization addressing these issues would have to incorporate the expertise of other disciplines, too. A short-term consequence of autonomous interdisciplinary research into the ethical and social impacts of technology would be that many scientists would be forced to give these impacts very serious thought.[36] Nevertheless, in the long run, both the chances of achieving consensus among different experts and the acceptance of genetic engineering by the lay public and the media might increase. However, just as in the risk debate, the scientification of impact assessments

36 In the short term, it is likely that biologists will object to such scholarly interference. See, for example, Rabino (1991: 84): "As to who should participate in regulatory decision making, there is a strong feeling (68%) against greater involvement by social scientists, political scientists, and ethics/religion scholars. Eighty-three percent of those who view public attention to recombinant DNA research as harmful reject the idea that other scholars should participate."

would probably be less oriented at fixed results such as the acceptance of genetic engineering or a fast and uncomplicated implementation of specific technologies but, rather, at procedural rationality ensuring legitimacy and credibility via scientific professionalization.[37]

In Germany, the degree of professionalization of issues relevant to the acceptance debate is rather low. Besides the scientific actors directly affected by the trouble of low acceptance, there are few experts claiming to be competent to deal with problems of genetic engineering. As a consequence, dissent cannot be incorporated, modified, and absorbed. The pros and cons of genetic engineering coexist and keep discrediting each other with technical and moral arguments. Because this deep conflict is not treated at the level of the scientific community in general, it is always waiting to materialize every time the technology is about to be introduced somewhere at the lower level of research organizations – for example when releases of genetically modified organisms are planned. These research institutes are then confronted with protest against genetic engineering in general, with the whole complexity of the acceptance problem. Thus, the failure of the scientific community to cope with low acceptance overloads research institutes with trouble. Still, they must try to cope with it somehow if they want to continue their research work.

3.2 Two Organizational Strategies: Indifference and Reflection

The first observable coping practice of research institutes can be labelled "indifference." In general, indifference might be grounded on cognitive factors – if, for example, the trouble with low acceptance and its consequences are not perceived at all. But in this particular case, the extent of trouble experienced with public acceptance makes such ignorance highly unlikely. As argued above, the manifest protest against genetic engineering forces the research institutions to reflect on its impact because the trouble is absorbed neither by political institutions nor by professional handling on the community level. Grounded on a scientific ethos, indifference may be legitimized to be

37 Ethics commissions fulfill these requirements only in a rather limited way. They most often proceed according to incremental logics which do not disclose the reasons for recommendations and avoid dealing with general principles that might entail conflict. For empirical investigations see van den Daele/ Müller-Salamon (1990); Mendeloff (1985).

an element of the corporate identity of modern science as an autonomous institution charged with generating beneficial knowledge without succumbing to social pressure.[38] The problem with such Mertonian norms is that modern research conditions require science organizations to search for resources and legitimation, which prompts them to incorporate norms beyond those of their narrow scientific field and to develop transscientific identities (Ruscio 1984). Hence, reflections on the behavioral pattern of indifference must be accompanied by a careful consideration of its consequences.

The greater the public's distrust is, the less likely it is that indifference will succeed as a strategy. On the one hand, the first institute that dares to face an alarmed, distrusting public is also the one that stands to enhance its scientific reputation by being the first to make an important discovery. But, on the other hand, this actor will surely take a beating[39] – and cannot be sure whether it will be able to overcome the public resistance. Anticipating this, many research actors will follow the "second winner's" strategy of waiting for better times. The second winner's pattern of dealing with public protest of planned releases is rather defensive because it is adaptive: it might sacrifice releases for reasons of acceptability – at least temporarily. The search for alternatives might theoretically lead to intensified theoretical research or to research contained in high-security greenhouses. Thus, such strategies do not have to imply changes in general research strategies, because it may be expected that planned releases will be accepted at some point in the future. For single research units such a moratorium might be quite rational because it can be assumed that its implementation is much easier for the second and

38 For an illustration of this, see Baltimore (1978). This ideology of a pure scientific ethos may even support expectations that legal and political institutions can buffer the consequences of such conflicts between scientific research and public concern (but it is likely that they would seldom be fulfilled). Its character as a norm could serve as an immunization from changing expectations and behavior. In reality, however, accusations of deviance and demands that the responsible institutions be sanctioned arise.

39 Note that the payoff of this strategy would change if different institutions released genetically modified organisms simultaneously. On the aggregate level, this strategy would lead to a short, massive confrontation; but for the individual institutions, the confrontation would be smaller because protest would be fragmented. However, in this case there might be an increase of the total amount of protest that would lead to a quasi referendum on the technology as described by Douglas/ Wildavsky (1982).

following releases. But, of course, if every unit adopted this strategy, total inertia would set in.[40]

So far we have introduced two opposing strategies: indifference which is oriented towards prompt releases without considering the trouble of public acceptance, and the second winner's strategy that takes protest and image aspects into account and tries to avoid confrontation by a quasi moratorium. Both strategies are based on the acknowledgement of given standards of public acceptance. But can the time lag be used in order to campaign for a change in the troublesome lack of acceptance, and how may the trouble be influenced and arranged in order to solve the problem? The traditional pattern insists on the responsibility of politics to buffer scientific research from disturbances. From this perspective, corporate strategies – either in a closed manner of silent lobbyism or, via controversial pressure, group-like activities that keep contact to the public – try to influence the political system. In addition, however, there are activities intended to influence acceptance by means of information and education. It can be supposed, though, that this strategy has its own repercussions internally: The more the participation in the public controversy is rooted in an interactive discourse structure, the more feedback has to be considered. Lessons from other sensitive areas indicate that such considerations might lead to far-reaching consequences, especially to the partial sacrifice of intended implementations in order to ensure trustworthiness.[41] Even if such sacrifices are intended as symbolic acts, they may develop their own dynamics that can be difficult to contain. In the long run,

40 From this point of view, the planned release of genetically modified organisms has the structure of a classical collective good, and free-rider problems as treated in theories of Rational Choice (Olson 1965). Fortunately or not, organizations are able to decouple their action rationality from these kinds of decision rationality. Action rationality, especially if supported by a strong ideology, avoids paralysis in favor of necessary courses of action (Brunsson 1985). Thus, for example, planned releases will probably be implemented even if there is no interorganizational coordination or special incentive structure.

41 Such a "rule of sacrifice" – Wildavsky's (1988) term for ensuring safety in the long run and on the aggregate level under conditions of uncertainty that makes prevention rather difficult and costly – is used quite often in the field of medical innovation. There, the regulation of pharmaceuticals is both rather strict and, at the same time, widely accepted by the technology promoters. Besides safety levels, many inherently social consequences have been considered, especially in terms of trust and public acceptance. See Bodewitz/ Buurma/ de Vries (1987).

this kind of adaptation leads to a consideration of nonscientific aspects in order to guarantee a realistic maximum number and frequency of intended releases. Thus, established research logics may be undermined by "symbolic politics."

For structural reasons there is a close link between indifference which ignores potential conflicts with the media, the lay public and social movement organizations on the one hand and the discrediting of internal critics on the other. In contrast, interactive discourse structures at the organizational interface with their social environment require the scientists to conform to cultural standards which they cannot turn around and dismiss when it comes to discussing criticism internally. Therefore, corresponding with the two opposing strategies of implementation, there are two analytically distinguishable patterns of "home politics" – i.e. organizational cultures – inside research organizations. The first discredits opponents and tries to maintain the closure of internal consensus by differentiating fundamentally between participants in technological progress and the advancement of science, and the illegitimate critics of these processes. The second course of action tries to avoid total confrontation by taking counterarguments and opposing views into account. Interestingly, this mode of coping is even able to make use of internal critics as early warning devices. It does not try to suppress or to discredit these critics and thus comes into close contact with the cognitive and normative structure of the counterarguments. It enables the technology promoters to anticipate trouble with external agencies and to check out opportunities for convincing opponents by intraorganizational mediation.

This response can be interpreted as a kind of coping that fulfills the strict requirements of the concept of organizational learning: the search for adequate action patterns that go far beyond standard operating procedures and the development of new ideologies (Hedberg 1981; Beyer 1981; Barber 1983). This kind of coping may even include an open-ended controversy between the pros and cons of rDNA technologies. Scientific, ethical and social criteria have to be taken into account. The research organization will look for external support, both to obtain information itself and to guarantee legitimacy for the course of action that is to be taken, and will be highly sensitive for extra-organizational influences and nonscientific criteria.[42]

42 The incorporation of plural rationalities such as professional and procedural standards (Meyer 1983) is an expression of open-mindedness in an organization that indicates it

3.3 Learning to Cope?

The multiplicity of strategies and structures that comes into play when a plurality of research institutes tries to cope with low acceptance indicates that a stable, universal pattern has not yet been established for dealing with this problem. So far, the institutes are in a phase of crisis and learning. Analytically, the courses of action that are taken can be divided into two paths. The first we have called indifference, which tries to ignore the trouble by expecting the political system to buffer the sciences from these disturbances and by interpreting protests as irrational reactions of the lay public which can be overcome by scientific education. Scientific opponents are blamed and discredited: Risks are denied, as is the legitimacy of reflecting upon the ethical and social consequences within the sciences. This corporate and professional identity corresponds to repressive organizational cultures which try to suppress internal protests that do not correspond to narrowly defined scientific issues. The second path encompasses more moderate reflective behavioral patterns. Interactional discourse structures at the scientific organizations' interfaces coincide with an internal culture that makes use of conflicts for anticipation and for informational exchange. This pattern is able to use the present standstill – for example with regard to planned releases of genetically modified organisms – in order to learn and to develop a new identity. It includes the consideration of nonscientific criteria and of scientific reservation even in an early phase of the research process. Looking beyond the mode of implementation and the public representation of the scientific enterprises, actors tending towards this approach even go so far as to consider sacrificing certain kinds of research in order to ensure legitimacy. Counterarguments are not discredited in general but acknowledged, at least to a certain extent, as being appropriate.

The variety of coping strategies dealing with the trouble of low acceptance can be interpreted as organizational and professional learning. The interdependence of isolated strategies is the most obvious barrier for the institutionaliza-

is in a crisis regarding its legitimation. Thus, organizational revolution is probable, and it may fundamentally change the corporate identity (Brunsson 1985). These dynamics are difficult to contain, so that the outcome cannot be anticipated in detail – a fact that is systematically recognized in the concept of "permanently failing organizations" (Meyer/ Zucker 1989).

tion of a conclusive coping strategy of research actors. The hardliners' strategy of discrediting and suppressing opposing views is undermined by those research actors preferring a rather moderate course of action. The conflict-oriented behavior of the hardliners, in turn, makes cooperation and trustworthiness rather difficult to establish, lowering the moderates' chances for success as well. Because of the interaction between the effects caused by these coexisting patterns, a consensus on strategic cause–effect relations can hardly develop. Hardliners blame moderates and vice versa for the trouble they are both experiencing, which makes learning even more difficult than analysis (Morone/ Woodhouse 1986; March/ Olsen 1975). This constellation of coping strategies leads to unintended – and even perverse – effects. Therefore, it is impossible to anticipate which variants of coping will be selected and which coping strategies will be institutionalized.

4 Conclusion: The Social Limits of Scientific Coping

Molecular biologists experience the implementation of the GenTG as being troublesome. Though the legal regulation itself is evaluated as being far too restrictive and bureaucratic, low levels of public acceptance seem to be the most important cause of this phenomenon. The traditional coping pattern of the research actors is to focus upon safety issues in order to convince opponents and thereby change their attitudes. But these educational measures have not achieved satisfying results in this case, and it can be assumed that stepping up such activities – as more of the same – will not suffice.

Partly, this can be related to familiar explanations of acceptance. An education of the public via scientific assessments of narrowly defined risks had indeed been expected to be an appropriate response to worries of the lay public. It had been supposed that, in the long run, the technologies would be accepted whose objective risk – in terms of probability and harm, i.e. death rates or economic value-of-life statistics – is beneath the level of many technologies which are already accepted (Starr 1969). In this scenario, no coping besides information and patience would have been necessary. But – unfortunately or not – such expectations soon were disappointed. Psychological studies offered an explanation for people's ignorance of objective risk levels by disclosing so-called heuristic and perceptual biases of the lay public (Tversky/

Kahneman 1974; Slovic/ Fischhoff/ Lichtenstein 1980). Further experimental research designs verified the significance of qualitative attributes of technological risk – such as involuntariness, lack of familiarity, dread of worst-case scenarios, etc. Sophisticated models were developed that took increasing numbers of variables into account; these in turn made it clear that the assessment of a technological risk is primarily based upon heuristic evaluations of the respective context surrounding the risk.

This line of research finally culminated in the discovery that acceptance is a function of "trust" (for a summary see Renn/ Levine 1988).[43] These studies indicate that, in order to ensure trustworthiness, effective modes of information may not be decoupled from risk regulation. Coping strategies that try to decouple the communication of risk from its management might cause further trouble of distrust.

If public acceptance is a function of trust in the institutional context of biotechnological research and development, technical risk communication alone will not be an appropriate coping strategy. Following this course of argumentation, the responsibility to generate acceptance may even go beyond the scope of molecular biology, because the research actors are intertwined with political and economic actors who are expected to apply and diffuse the promised technologies.[44] Because it indicates both the relevance of social

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43 Similar results have been developed by sociological and anthropological research. Wynne (1987), for example, defends the significance of both the institutional context of the technology and the cultural context of its assessment by the lay public (see also Short 1986). Douglas (1986; 1990) points to the social function of risk debates for rethinking the relationship between the dynamics of technological innovation and its effects on values and norms of different cultures. Giddens (1990), drawing heavily on Luhmann (1988), says that the "disembeddedness of expert knowledge" is the most distinguishing phenomenon of modern society, and that expert knowledge is therefore in permanent need of trust, confidence, and faith (see also Barber 1983; Lewis/ Weigert 1985).

44 The problem with recombinant DNA technologies might be that they are and will be used in areas whose innovations are not judged without ambiguity: primarily in medicine, the chemical industry, and modern agriculture. Thus, the protest against research on rDNA may be grounded in the expectation that biotechnologies will amplify tendencies which are not only assessed as being beneficial but also as partially causing ecological, social, and ethical harm. In this context of application, biotechnologies serve as a symbolic means of dynamics whose origin and energy are only rarely influenced by (and far beyond the control of) biological research. Hence, it may be deduced that low acceptance is only very partially based on perceptual biases of the lay public, which can be easily

environments which only indirectly influence the resource support for science and the legal constraints placed upon research, the trouble discussed here is a context which blends the sciences and their traditional environments of economic and political agencies together into a "community of fate" (Heimer 1985), whose trouble is internally attributed, transmitted, and amplified. Hence, the coping of the economic and political agencies may cause further trouble for science (and vice versa). Strategies aimed at overcoming the trouble of low acceptance are bound to the constraints of legitimation and have to be scientifically, politically and economically successful. Thus, coordination and collective action are even more difficult to achieve.

But the case study of biotechnology also directs attention to aspects that have not yet been considered by concepts which associate acceptance exclusively with the lay public, whose deficits primarily materialize in the form of public opinion or in quasi-political activities of protest. The case demonstrates that the lay public is only one of the societal groups whose acceptance is needed, and that passive toleration is not sufficient for a successful technological innovation. Hence, coping strategies have to consider that because research actors have to prove their legitimacy and, at the same time, acquire financial support, they require the acceptance of promoters which pull and push this particular technology. The mode of acceptance of the promoters, however, requires more than passive toleration; it has to lead to an active demand based on high levels of expectation, commitment and enthusiasm.[45]

As long as nuclear energy was the paradigm of a risky technology, these sharpened requirements of acceptance could be neglected because nuclear energy represented the exceptional case of an innovation which needed very little nonpolitical promoter acceptance. Its application was primarily dependent on governmental and administrative promotion; especially in the beginning, the great magnitude of expectations, commitment and motivation made it able to compensate for all the other shortcomings of acceptance. From the case

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overcome by educational programs downplaying ecological damage and health risks.

45 However, in the case of biotechnology, the technology promoters of the medical system, of chemistry, and in agriculture have to consider not only the material function of biotechnology, but also its symbolic meaning, because both are inseparably intertwined. Thus, the incorporation of rDNA technologies might entail further trouble for the technology promoters than has already been experienced. As far as economic institutions are concerned, a shortage of public acceptance could plainly materialize quite conventionally: in the lack of market demand that is far beyond pure opinion and political activism.

of biotechnology, however, it can be concluded that technological innovations also require expectations of use and benefit which can only be determined by the political system in a very limited way – and, as may be deduced, even scientific considerations of formal Technology Assessment or efforts to achieve commissional consent will not suffice to create this kind of acceptance.

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If People Become Afraid of Your Research Methods: Conflicts over Research Reactors in Berlin and Munich

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A scientific project as big as the FRM II ought to be supported by a broad majority of political forces and by an equally broad majority of the public. If this is not the case, it will prove to be anything but a blessing for science. We feel that the project should only be carried out if a high degree of consensus can be established (*Information Leaflet on the New Munich Research Reactor Project (FRM II) at the Garching Research Center,* 19 February 1992).

1 Introduction

In the not too distant past, decisions on the location of large scientific instruments were regarded by the public as a purely scientific concern. For some time now, however, society has "discovered" research. Discussions on science now go far beyond the traditional concern with the "curses and blessings" brought by the application of scientific findings outside science. The new criticism of science directly addresses the way research is carried out. The first ethical issue the debate focusses on is the research object: Under what circumstances are clinical experiments on human beings (van den Daele 1990, Gill 1991) or experiments on animals legitimate? The second ethical issue is the choice of research methods. Society is becoming increasingly aware that *research activity itself,* when it involves certain research methods and instruments, can generate the same sort of risk as the results of research when

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they are applied in industry. The most significant examples of this are genetic engineering (see Hasse/ Gill in this volume) and the use of nuclear reactors to produce neutrons for research purposes.

As social awareness of this new type of problem with research has grown, society has become increasingly interested in the processes by which decisions on research methods are reached. Political actors and affected parties have increasingly attempted to promote their interests in the decision-making processes on research. At the same time they have even sought to alter the course of these processes, that is to say to establish new modes of decision-making.

From the point of view of science, these efforts represent the attempt to attack the very heart of scientific autonomy, the free choice of research topics and methods. This means trouble, and it means trouble of a very particular sort. Whereas cuts in funding and political orientation toward social applications allow the scientist a certain margin of liberty in making scientific decisions, political decision-making on research methods directly addresses the issue of *whether* and *how* certain research topics are to be tackled.

A special type of trouble ought to provoke specific perceptions and reactions on the part of the scientists concerned. This is the subject of our paper. With regard to the decision-making processes on upgrading or constructing the research reactors in Berlin (BER II) and Munich (FRM II) we wish to discuss the relation between the special type of trouble and the coping strategies developed. The following general hypotheses can be stated:

1. The specific processes by which internal scientific decisions and science policy decisions are made shield research to a large extent from extra-scientific influences that could threaten its autonomy.
2. Where such influence is nevertheless successfully exerted, it poses a vital threat to scientists and research groups because it deprives them of their working basis. The very existence of a research institution is threatened if someone tries to negatively influence an element of its work that is essential for the identity of the organization.[1]
3. Scientists have little room for manoeuvre in coping with this kind of trouble. The scientists' opportunities to ensure the continuation of research by changing their research object or their methods and techniques are circumscribed by the range of objective options (i.e. the laws of nature

1 See Stucke (1991: 33-43) on organizational identity.

and the state of the art) and subjective options (the abilities and preferences of the researchers) open to them.

We have used the "Coping with trouble" approach for the design of our empirical analysis. To justify classifying situations and activities within this coping concept, we tried to develop definitions of the basic concepts related to empirically identifiable characteristics of actors' situations and actions. Important distinctions with regard to these characteristics are; firstly, the difference between trouble which is caused by a single event or by a limited number of events, on the one hand, and trouble as a result of a complex and gradual worsening of an actor's situation on the other and, secondly, the difference between the factual conditions of actions and the way actors perceive these conditions.

Coping with trouble we understand to be a special type of action cycle realized by actors who strive to transform their situation to correspond to their goals.[2] The situation of an actor consists of the internal and external conditions for action as evaluated by that actor. Internal conditions for action are those which are under the exclusive control of the respective actor, while external conditions exist relatively independently from him and can be influenced by him, at best, partially. Research actors' actions can be classified as either research action or scientific action. Research action is the action of researchers aimed at producing knowledge, and scientific action is the action by researchers or research institutes aimed at influencing their environment in order to ensure the continuation of research.[3]

A *troublesome situation* develops when the actor's conditions for action can no longer ensure the continuation of research action and, thus, the attainment of the actor's goal. Such a change in the conditions for action occurs not only when a goal actually proves impossible to attain, but also when the possibility arises that its attainment will be obstructed, i.e. in the case of a threat.

In relation to individual actors, i.e. scientists, this means that the attainment of their current research goals, and possibly even the perpetuation of

2 See Gläser et al. (1993) for a more comprehensive treatment.

3 In German *Forschungshandeln* and *Wissenschaftshandeln;* see Krohn/ Küppers (1989: 28-31, 71) Cf. an English-language description by the same authors (Krohn/ Küppers 1990).

the way in which research is carried out, cannot be ensured. Research institutions find themselves in troublesome situations when the action taken by the organization to influence its environment is no longer able to ensure the survival of the technical core, that is to say the research action (Thompson 1967, Stucke 1991: 29). The troublesome situation for the organization is thus always a consequence of a troublesome situation for at least some scientists or research groups.

In tune with the editors in the introductory chapter of this volume, we define *trouble* as a specific perception of the conditions for action by an actor. A factually troublesome situation can be perceived in two ways by an actor. Either he directly recognizes the specific threat to goal attainment, or he does not perceive the trouble, but does perceive a problem which demands to be attacked by scientific action. The latter way of perceiving the situation, which is not explicitly mentioned in the introductory chapter, is in a certain sense a milder form of trouble, since the range of options available for action are still regarded by the actor as adequate for problem solving. This fact gives rise to considerable methodological problems, since it implies that not every modification of scientific action can be attributed to trouble as defined above.

As a reaction to the perception of trouble, we expect specific scientific action, namely coping. Similar to the editors in the introductory chapter, we understand *coping* to be the choice or development of strategies for scientific action with which the actor beleives he can obtain relief from trouble. Because this also includes simply waiting for better times, we will show that the distinction the editors make in the introductory chapter between coping and fatalistic suffering must be revised. The various forms of coping will be treated systematically when we compare the cases under investigation in Section 3 of this chapter.

2 The Genesis of Trouble and the Limits of Coping: Two Case Studies

2.1 General Aspects of Research with Neutrons in Germany

In the past two decades, measurement with neutron beams has become a standard research method in various scientific disciplines. A large number of research problems in biology, chemistry, physics, metallurgy, materials science, and other fields are tackled with the aid of neutron beams, either alone or in connection with other methods (Gutachterausschuß 1981: 14; Kommission Grundlagenforschung 1992: 64). The users of neutron beams are members of these various scientific communities and can be categorized in terms of the function neutron beams have in their research activities:

1. Some users could substitute other methods for measurement with neutron beams.
2. For other groups, access to an external neutron source is sufficient. "These are people who want there to be a hole in the wall with neutrons coming out" (interview quote).
3. Another group of users needs physical proximity to a neutron source (for producing unstable isotopes by neutron irradiation, or for measurements of meta-stable objects).
4. Still another group of users requires direct availability of a neutron source of its own without application formalities or narrow restrictions on beam time (this applies, for example, to the development of neutron beam measuring techniques).

Parallel to these differing demands, the failure of projects to provide new neutron sources has very different consequences for the users, both in terms of the genesis of trouble and the prospects of coping with it.

A certain institutionalization of neutron beam users took place when the "Committee on Research with Neutrons" was founded in 1987. The core of the group of neutron beam users is a scientific community that consists primarily of the operators of research reactors, who are also to the fore in developing measurement methods and technology. This community forms a network with the external commissions and advisory committees, with the relevant departments of the state *(Land)* science ministries and with the Federal Ministry for Research and Technology (*Bundesministerium für Forschung und Technologie,* BMFT). These actors constitute a "hybrid community" (van den Daele/ Krohn/ Weingart 1979: 24-31). As they forward and exchange ideas on the upgrading or construction of individual neutron sources, advocates of this technology in the fields of science and science policy form special networks which can be viewed as "vertical technical fraternities" (Wagener 1979).

The research reactors constructed in Germany in the fifties and sixties in the National Research Centers, and the Munich I research reactor at the Technical University of Munich (FRM I) were able to meet the demand for neutron beams for a long time, although they were not especially designed for such applications. In the mid-1970s, however, there were indications of a reduction in the supply of neutron beam time. Jointly operated by France, Germany and Great Britain, the high-flux reactor had started operation in Grenoble in 1971, but many German researchers felt that the demand in Germany exceeded that reactor's capacity. They saw a need for a powerful national neutron source to serve as a "home base" for preparing experiments to be conducted in Grenoble and to meet the demand for medium-intensity neutron irradiation. Since the mid-1970s, various projects have been under discussion within the scientific community, including the building of a new reactor, the construction of a spallation neutron source[4] and, as an "interim measure," the extension of the Berlin experimental reactor BER II (see 2.2). In 1981, the Advisory Committee on Large-Scale Projects in Basic Research made the following recommendations (Gutachterausschuß 1981: 109):

- to carry on with project studies for the spallation source and to make a decision in the mid-1980s on realization of the project;
- not to construct the medium-flux reactor, since it could be completed only shortly before the spallation source;
- to begin upgrading the Berlin reactor BER II without delay, the advisory committee assuming that work could be completed within reliably predictable time and financial limits (Gutachterausschuß 1981: 31).

4 A spallation source is an alternative possibility for producing neutron beams. Whereas the research reactor provides neutrons generated in a chain reaction, in a spallation source neutrons are produced by bombarding a target (a small lead or uranium plate) with high-energy beams (from an accelerator). The advantages of the spallation source are the greater overall adaptability of the technology, the possibility of a higher neutron flux, and greater safety (no uncontrolled reactions can occur, since there is no chain reaction; there is a considerable reduction in the amount of waste produced, which is nonetheless highly radioactive). The disadvantages are the greater expense of construction and operation (the energy consumption alone would be around 30 megawatts). For most applications the spallation source offers a possible alternative to the research reactor. But due to the different characteristics of the neutron beam produced, there are certain experiments for which only one of the sources is suitable.

The advisory committee also assumed that German scientists would have to make greater use of foreign sources in future.

In June 1985 the BMFT decided not to construct the spallation source in Jülich. The reasons given were the continuing development risk and excessive costs. The extraordinarily high costs (estimated at the time at DM 1.4 billion) and the development risk could be attributed to the dimensions envisaged for the spallation source.[5] The spallation source having been written off, the scientific community turned its attention to the Munich FRM II project (see 2.3). Otherwise little happened for a long time, as the Science Council (*Wissenschaftsrat*, a national board advising the government on science policy affairs) remarked critically in 1989: "Since then nothing further has occurred in this field, while other industrial countries have made efforts to develop, construct, and commission new neutron sources or new types of neutron sources ..." (Wissenschaftsrat 1989: 241-242).

2.2 Decision-Making Processes Concerning the BER II in Berlin: The Pure Case of Coping with Trouble

Remarking on the occasion of the commencement of routine operation of its BER II research reactor in 1975, the Hahn-Meitner Institute (HMI) noted in its Annual Report that an increase in performance was already urgently required (HMI 1975). This alluded to HMI's plans to alter the profile of its research activities. The reactor was no longer intended to be used exclusively for nuclear chemical investigations (for which it had been designed, and for which it was perfectly adequate), but also as a neutron source. Establishing research with neutrons at the BER II required modification that included an increase in performance.

At the HMI there was a variety of attitudes toward upgrading the BER II. The rejection of the project expressed by a number of interested parties was motivated by the changes in profile that were intended to accompany it. In the long term the plans appeared to involve adverse consequences for a number of other lines of research or even to threaten their very existence. But the HMI scientific management, having first discussed the project in 1975 and

5 A neutron source with a medium flux corresponding to that of modern research reactors was planned, which was far in advance of the state of the art for accelerators at that time.

being responsible for the final decision, was not swayed by this internal debate. The controversy did not become public; at that time the public could not be expected to show interest in a discussion on research equipment or an institution's research profile.

The decision-making processes that began with the resolution taken by the scientific management of the HMI were characterized by the clarity of the actor constellation. The HMI as a big science center receives 90% of its financing from the BMFT and 10% from the state of Berlin in which it is located. The parties whose approval was needed for the project were thus quite easy to identify. A first positive decision was made in 1977 when the supervisory board of the HMI approved the project. The BMFT declared its approval in 1978. It set up an Advisory Committee for Large-Scale Projects in Basic Research in 1981 and an Expert Group on Natural Science Basic Research in 1985, which both retroactively supported the decision to upgrade the BER II. In 1983 the HMI supervisory board passed the financing plan for the upgrading procedure. The phase of science policy making was thus completed without attracting public attention and without provoking controversial discussion within the scientific community.

Early on, before the science policy phase had ended, the phase of environmental policy making began. On 10 October 1979, the HMI filed an application with the authorities responsible for granting the building and operating permits (referred to below as the licensing authority) requesting permission to upgrade the BER II and, subsequently, to operate it in the upgraded form. At that time the licensing authority was the Department of Economics and Labor of the Berlin Senate.[6] Thus the formal and informal activities involved in such approval procedures began. A citizens' action committee founded in 1979 to oppose upgrading of the BER II constituted an additional actor.

When news of the HMI's application was published in autumn of 1982, more than 200 objections were filed by concerned citizens. As a result of the public hearing on these objections in January 1983, the HMI and the licensing authority declared their willingness to carry out analyses of potential accidents to determine the effects of external influences on the reactor; this safety aspect had been neglected up to this point. They further agreed to have these

6 The Senate of Berlin is the governing body of the city-state. Because of its special Allied status, Berlin did not have formal state *(Land)* status until unification on 3 October 1990, but its political institutions were similar to those in the West German *Länder*.

analyses evaluated. It took two years to complete these analyses. Parallel to this formal procedure and the work necessary to fulfill its requirements, the HMI filed a total of four supplementary applications, the last of which requested that the building permit be separated from the operating permit. Shortly after granting this separation, the Senate Department issued the first partial building permit in August 1985. On the same day, the BER II was shut down for upgrading.

As soon as the first partial building permit was issued, the litigation began. A citizen filed suit against this permit (and all subsequent ones). Although each of these law suits has been dismissed thus far,[7] the litigation has delayed the project. As a result of the suits, construction on part of the facility was temporarily halted and a separate permit procedure was established. Construction was thus interrupted for about two years. However, after the new formal requirements had been fulfilled by the HMI (applying for a second partial permit) and the licensing authority (granting the second partial permit in October 1988), upgrading work could proceed.

The Chernobyl reactor disaster in 1986 caused only a brief – and mild – disruption. Neither markedly increased resistance from the neighboring residents nor political action initiated in the Berlin House of Representatives by the Alternative Ticket (*Alternative Liste*, AL)[8] resulted in the project being interrupted, let alone put in jeopardy; and the suggestion by the president of the Berlin Higher Administrative Court to defer construction work on the BER II for a while to allow for a "pause for thought" was not heeded. The HMI and the licensing authority declared that the construction under way at the time involved non-nuclear facilities, and that a halt to construction was therefore unnecessary. Moreover, they claimed that the BER II was safe and that an accident like Chernobyl was impossible.

Although the superficial impression was that the project was jeopardized, this was not the case. The objections filed by citizens, delays in the approval procedure, and litigation are "commonplace" in the construction and licensing of nuclear facilities. Numerous uncertainties and delays arose in connection with the approval procedure and the litigation. While the HMI and the project

7 A decision on the suit against the operating permit has yet to be made.

8 The Alternative Ticket was a local party in Berlin sharing the aims of the (ecologically oriented) Green Party in West Germany; it amalgamated with the Greens after unification in 1990.

group perceived these influences as being obstructive, they were also aware that these were the everyday problems any nuclear facility going through the approval procedure has to face. These problems included defining the project more precisely and providing additional proof that adequate safety precautions were being taken, both of which were required by the licensing authority in response to the citizens' objections. The HMI did not equate fulfilling these requirements with averting impending danger; the scientists and management did not doubt that the project would be completed successfully. Since the necessary supplementary applications and the alterations in the project design were always worked out in close, informal cooperation with the licensing authority, their approval was ultimately a foregone conclusion.[9] For the scientists engaged in research with neutrons at the HMI, the delays did not constitute trouble because the continuation of their current research was never in doubt. Research relating to the reactor or dependent on neutron beams proceeded in low gear. There was a cooperation agreement to use the research reactor at Risø (Denmark). Other groups were concerned with developing measurement technology for the periphery of the new reactor.

Shortly before the environmental policy phase ended, at a point in time when the upgrading construction work was almost finished, a decisive change in the actor constellation occurred, triggering an almost two-year phase of trouble. The January 1989 election to the Berlin House of Representatives produced a majority for a government coalition between the Social Democratic Party (SPD) and the AL. With its strong emphasis on ecological issues, the AL was staunchly opposed to nuclear power stations and also to permitting the upgraded BER II research reactor to go into operation. This can be pinpointed as the juncture at which troublesome structures began to emerge, harboring a threat to the very existence of the BER II and, consequently, to the future of the HMI. Overt or implicit threats by political actors were always constituent elements of this threat.

Three phases can be distinguished in the history of the HMI's coping with trouble. The first phase involved adapting to the new actor constellation evolving from the shift in political power: in the new senate, responsibilities were being redistributed and political actors were busy establishing new spheres of influence. The second was marked by conflicts over obtaining a decision from the licensing authority. In the third phase, the actors on all sides

9 See Section 3.1 on the problem of this sort of informal administrative action.

were involved in disputes over the decision that had been reached: the licensing authority had refused to grant permission for the BER II to go into operation. It was a constitutive feature of all these troublesome situations that the endangerment consisted of both threats that were voiced and threats that were carried out.

Phase I

The scientists at the HMI did not immediately perceive the election result as a threat to the reactor project. The HMI management, however, was already aware of possible consequences on election night, and immediately tried to intervene in the coalition negotiations. Fearing that the SPD could underestimate the significance of the approval procedure, the HMI management provided the party with comprehensive information on the research objectives of the institute and the importance of the BER II for the HMI. This intervention was aimed at preventing the AL from obtaining responsibility for the licensing of BER II, because if that occurred, licensing seemed rather unlikely. The HMI did not succeed, however. The coalition agreement contained a compromise which not only turned the responsibility for licensing nuclear facilities over to the Department of Urban Development and Environmental Protection, but gave this department to the AL. The AL was to reciprocate by completing the pending licensing procedure for the BER II "in accordance with the law."

This decision having been made, the HMI's next concern was to ensure that the officials that had been responsible until then for processing the application at the state environmental agency continued to be responsible for it. Here, too, they were unsuccessful. The Senator for the Environment assigned new staff to handle the approval procedure.

With each of these attempts, the HMI was trying to prevent trouble. It tried to forestall menacing changes in conditions for action, which in this case were the transfer of the authority to license nuclear facilities from one department to another and the surrender of this department to a party whose platform forbid it to sponsor the project. When these efforts bore no fruit, a situation ensued in which de facto agreements (following comprehensive informal preliminary negotiations) made between the former licensing authority together with the staff of the Department of Urban Development and Environmental Protection on the one hand and the HMI on the other became essentially invalid. This kind of de facto agreement typically results from informal administrative action (see 3.1): Virtually all aspects of an application procedure that might be relevant for approval are settled by informal negotiations between the applicant and the licensing authority before the application is actually submitted. The licensing authority's breach of the de facto agreement became inevitable when political responsibility for the authority was transferred to the AL, and when the AL's Senator assigned new officials to process the application. The BER II project was now in jeopardy primarily because one of the declared aims of the AL was to prevent the BER II from going into operation. Since, moreover, the Berlin state constitution allows the Senator for Urban Development and Environmental Protection complete autonomy,[10] the HMI could only hope that the governing coalition between the SPD and the AL would collapse prematurely; that was the only conceivable way to break down this impasse before new elections took place.

Phase II

In July 1989, when the agreement based on the coalition negotiations went into force, i.e. when the threat voiced in Phase I was carried out, a new phase of coping with trouble began for the HMI. On the technical side, the BER II was almost ready to go into operation by summer of 1989; upgrading work on the BER II was completed in August. As a result of informal negotiations with the former licensing authority and in accordance with the two partial building permits, a variety of additional safety aspects had been taken into account; this involved both providing additional evidence on the safety of the facility and modifying the project to improve its inherent safety altogether. The demand for containment of the reactor (i.e. providing a shell to make it impervious to airplane crashes) had not been followed up, since such safety measures were considered to be too expensive. The only technical question still unanswered at this point was that of how to dispose of the radioactive waste. The general problem of the disposal of radioactive materials[11] and

10 In contrast to the constitutions of many German federal states which ensure that general competence for establishing policy in all ministries is left to the chief executive, the Berlin constitution grants each senator final competence for policy making in his or her ministry.

11 There is still no satisfactory solution to the problem of disposing of radioactive material. A study carried out by the Worldwatch Institute (Lennsson 1991) in 1990 indicated that

the special problems of the highly enriched uranium[12] initially necessary for operating the BER II increased the difficulty of finding a solution to the *disposal* of the HMI's spent fuel elements. However, under the Atomic Energy Act, proof that a solution has been found is a necessary precondition for granting an operating permit. The operating costs of the upgraded reactor take up a third of the HMI budget. Now that the reactor had been completed, considerable maintenance costs would accrue regardless of whether it was in operation or not. However, funding for neutron research at the HMI had been reduced and was intended to be fully available only after the BER II was permitted to go into operation.

Under these cognitive-technological and economic conditions for action, a period began for the HMI in which the Senator for Urban Development and Environmental Protection refused to grant the operating permit despite growing pressure from various sources. The reasons given for the postponement of the decision on the application, which lasted until the summer of 1990, were the HMI's failure to fulfil the requirements of the second partial building permit and, later on, the lack of a solution to the disposal problem. In January 1990 the HMI had been able to submit a contract between the German firm NUKEM and the British atomic authority UKAEA as evidence that there was a solution to the waste problem. The contract provided for interim storage and possible reprocessing of spent fuel elements from the HMI at the Dounreay (Scotland) reprocessing plant, and committed Germany to take back the entire radioactive material after 25 years at the latest. The Federal Minister for the Environment declared that this contract constituted adequate proof of the HMI's compliance with the law regarding the disposal of nuclear waste. The Senator for the Environment in Berlin, however, was of the opinion that this evidence failed to meet the requirements of the Atomic Energy Act.

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at that time there was not a single country in the world providing sufficiently safe storage for radioactive waste. Reprocessing, which entails additional technological risks, cannot offer a solution to the problem, since it also produces radioactive waste.

12 The 93% uranium used as fuel for the BER II can be used to produce nuclear weapons. For this reason the United States tried for a long time to limit its international circulation. For technical and ecological reasons, however, the United States has not been taking back spent fuel elements since 1988. An international program to reduce enrichment in research reactors created the preconditions for research reactors to convert to lower enriched uranium. This is also planned for the BER II, but has yet to be put into effect.

In spring of 1990, the GDR citizens residing in the immediate vicinity of the HMI were invited to participate in informal consultations; the decision on whether to issue the operating permit was once again delayed until they were concluded.

The delays in the approval procedure and the fate of the BER II project were the object of lively public debate and the cause of polarization in state politics, especially within the SPD. In the media, criticism of the BER II project was predominant. This can be attributed both to numerous activities initiated by the project's opponents and to a series of events which provided effective material for exploitation by the media and thereby fuelled speculation on the possible risks presented by the project: Unexploded wartime munitions discovered on the grounds of the HMI, and an anonymous tip-off on the illegal storage of nuclear fuel elements led to the public prosecutor having the HMI premises searched.

There were marked differences in attitude towards the BER II project among HMI staff. About one third of employees was dependent on the reactor and unambiguously supported the project. Other scientists saw their own research endangered by the new profile of the HMI in relation to the BER II; still others were opponents of nuclear facilities in general. Each of these two groups rejected the BER II. The increasingly critical attitude of the public also lent indirect support to the opponents of the reactor within the HMI. A stalemate arose, during which the project was no longer discussed within the HMI. The advocates of the project were reluctant to take stand because of the predominantly critical public attitude to the project, and the opponents of the project avoided expressing their opinion because they did not wish to be held responsible for the project failing (which seemed quite possible at that time). Thus, the project became taboo within the HMI itself.

However, the opponents of the BER II project within the HMI not only voiced their opposition anonymously in the press, but also leaked inside information to political actors who were opposed to the project. Although these activities were never coordinated, a reciprocal stabilization emerged between the opponents within the Institute and the opponents among the political actors at the state level.

The situation we have described contained a number of troublesome features simultaneously. The threat from the preceding phase (transfer of the responsibility for licensing the BER II to the Department of Urban Development and Environmental Protection controlled by the AL) had been put into effect. It soon became evident that actual operation of the (already operable) BER II would be impossible for an indefinite period, because the licensing authority put off making a decision. And even if they had made a decision, it would most likely have been a refusal. This threat manifested itself both in the general political attitude of the AL and, for example, in specific statements made by the Senator for the Environment about the evidence supplied by the HMI concerning waste-disposal facilities.

This situation directly affected the profile of the HMI. Without the BER II, the HMI could not fulfill its institutional mission. The BER II was intended to be used primarily by (outside) university researchers, with only one third of its capacity reserved for the HMI's own research. It had been only under this condition that the BMFT had approved the project. Under the prevailing circumstances, the HMI would be unable to fulfil this function for an indefinite period; indeed, it seemed quite unlikely that it would ever be able to do so. This meant that an important domain of the HMI was endangered. Research work dependent on the BER II was also postponed for an indefinite period. A difficult time had begun for the HMI. One indication of this was the fact that important posts within the Institute could not be filled. An additional factor aggravating this situation was the general political pressure on big science centers, whose legitimation had come into doubt especially from the point of view of finance policy (see Stucke in this volume).

The trouble perceived by the HMI management and at least some of the scientists threatened not only the success of the BER II project and the neutron research, but the very existence of the HMI as a whole, since the research reactor was a constitutive element of the HMI's identity. In such situations, actors are generally expected to mobilize all resources for action to bring about a change. Surprisingly, during this period of uncertainty about the operating permit, HMI was relatively inactive. In other words, *it waited.* There are two conceivable reasons for an actor to wait in such a situation. Waiting can firstly be the result of a real or imagined inability to act. This kind of "helpless waiting" is very characteristic of HMI's coping. The conditions for action were interpreted to mean that practically no options for action were open. This perception was quite correct, for there were a number of factors restricting the range of activities open to the HMI:

- The upgrading of the BER II having been completed in accordance with the permits granted, alterations of the project were no longer possible.

- The disposal of waste produced by research reactors continued to be an unresolved problem. Hence, there was no likelihood that technological or political measures would improve arrangements for waste disposal.
- The chances of expediting the procedure by taking legal action seemed poor. In order to do so, the HMI would have had to prove that the licensing authority was delaying the procedure for reasons not related to the content of the application, which would have been almost impossible to substantiate.

Secondly, waiting can also be part of a conscious strategy. "Strategic waiting" can be grounded in the anticipation of action by other actors apt to eliminate the trouble, or in the assumption that the trouble is temporary and will thus disappear in time.

Action aimed merely at triggering activities by other actors is on the borderline between "strategic waiting" and purposive action. Such an option for action was still open, so the HMI took advantage of it. In the period of uncertainty about the decision on the operating permit, the HMI initiated letters that were then sent by various scientific associations and societies and by the big scientific organizations (the Max Planck Society, the German Research Foundation, the Fraunhofer Society, and the University Rectors' Conference) to the Governing Mayor of Berlin, drawing his attention to the adverse effects the current situation would have on research in Germany and calling for a rapid decision on the operating permit.

Without the HMI having to take the initiative, the network of advocates of the project was activated by the indefinite postponement of the decision on the operating permit for the BER II. One of the project's most prominent proponents at the state level was the Department of Science and Research, which had shared responsibility for the science policy decision and had provided general funding for the HMI and special funding for the BER II project. The department, which in the new government had been allocated to the SPD, supported the project unconditionally in the state government, in the House of Representatives and vis à vis the public. However, its opportunities to intervene in this situation were quite limited: Since it was not directly involved in the decision-making process, it could only try to influence public opinion and the state government. While the Governing Mayor of Berlin, a further advocate, took the same stance as the Department of Science and Research, he, too, was unable to intervene effectively in the decision-making process because the Senator for Urban Development and Environmental Protection had sole responsibility for the decisions taken within her portfolio.

For advocates of BER II at the federal level, the possibilities to intervene were also limited. One of the most effective instruments would have been a federal directive from the Federal Minister for the Environment issued to the state licensing authority, but such a directive would not have applied in Berlin because of its special status under the Allied forces. The BMFT could exercise pressure only indirectly via its funding of research institutions and projects in Berlin – which is precisely what it did, blocking DM 18.5 million in project financing for solar research at the HMI. This decision put the state government of Berlin under considerable pressure. The governing mayor assured the BMFT that a decision on the BER II operating permit would be forthcoming by May, 1990, and at the same time called for the disposal problem to be resolved by the same date. Upon this assurance from the Governing Mayor, the blocked funds were released in December 1989.

The only coping option still open to the HMI was to try to publicly counter criticism of the project. By attempting to show that its organizational survival was dependent on the success of the BER II, the HMI tried to refute the AL's argument that the institute could continue to exist as a National Research Center without the new reactor. At the same time it sought to convey the image of an institute supporting the reactor to a man. This did not, however, fully succeed due to the informal contacts between internal opponents of the BER II and the press mentioned above.

Phase III

On 10 August 1990, the Berlin Senator for the Environment rejected the HMI's application for an operating permit for the BER II. In the preceding weeks, the Berlin Senate had put considerable pressure on her, culminating in a Senate resolution (carried by the SPD senators) calling for her to grant the operating permit by 21 August. As we have already noted, it was not, however, possible to enforce such a resolution since the Berlin state constitution guaranteed the Senator for the Environment sole responsibility for her department's policy. The Senate's only possibility would have been to strip her of her authority to issue a license in this particular case. This would have precipitated the collapse of the coalition, however. At that time it was unlikely that such a procedure would have found a majority among the SPD.

The BMFT declared that it could see no future for the HMI as a big science center, and scarcely two weeks after rejection of the partial permit, it began negotiating with the state of Berlin on transferring responsibility for the HMI entirely to the state of Berlin and on reimbursement of the investment outlays for the BER II. Without the BMFT's financial contribution, the state of Berlin would have to finance the HMI by itself. This was virtually impossible since Berlin, as already mentioned, paid only 10% of the HMI's budget.

The HMI's conditions for action once again evidenced several troublesome aspects. First, the threat looming in the preceding phase had become reality, the application for an operating permit for the BER II had been refused, and the BER II could not go into operation in the foreseeable future. This decision by the Senator for the Environment could not be reversed as long as the SPD and the AL formed the governing coalition. This constituted a very definite threat to all scientists whose research required neutron beams, and for the personnel operating the reactor. At the organizational level, the identity of the HMI was in danger. A research orientation determining the profile of the institute could not be developed, which was equivalent to the loss of an important domain. Moreover, the HMI was unable to fulfil its intended function of providing neutron beams for a wide circle of external users, resulting in the loss of a further domain. This affected personnel planning, too: It continued to be impossible to fill leading positions at the HMI.

A further troublesome aspect thus emerged as a new threat. After the HMI changed its profile in the eighties, it viewed the BER II as the large instrument constitutive to its identity and, thus, vital for the Institute's future. Since big science centers were subject to considerable political pressure at that time to justify their existence (Stucke in this volume), and since cutbacks in financing and personnel in this sector were planned, the BMFT's threat to withdraw from the HMI – if the issuance of the operating permit were deferred indefinitely or if the project were actually stopped completely – could be considered quite serious. Since the state of Berlin would not have been able to bear the financial burden alone, this would have meant the HMI suffering a considerable reduction in size. Ultimately, the HMI would have ceased to exist as a National Research Center with a profile of its own.

Although BER II had been technically ready to go into operation for over a year, its licensing had been delayed all that time; the time factor began to loom large among the troublesome conditions for action. Theoretically, there were three ways they could disappear. First, the SPD/AL coalition could fail. If the AL was no longer one of the governing parties, it would be very likely that the operating permit would be granted. Second, since it was expected that the special Allied status of Berlin would be abolished when Germany was united, the federal Minister for the Environment would have the right to issue federal supervisory directives. Third, legal action by the HMI contesting the refusal by the Berlin Department of Environment to grant the operating permit (see below) could have succeeded. However, all these possibilities involved great uncertainty with regard to the time factor. The only variant of a rapid and lasting reduction of troublesome conditions for action was considered to be the emergence of a new majority in the election to the House of Representatives scheduled for early December 1990. However, until shortly before election day there were widely divergent prognoses on the outcome, and a renewed coalition between the SPD and the AL did not appear to be excluded.

The other possibilities were recognized by the HMI, but in view of the necessity of creating the preconditions for continued support of the HMI by the BMFT, they were judged to be too uncertain. Consultations with the Federal Ministry for the Environment did not appear to indicate that the minister would immediately exercise his right to issue the relevant directive. Moreover, any directive issued by him might, as on other occasions, lead to litigation of indefinite duration. The indefinite duration of legal disputes also allowed no hope of the trouble being eliminated with sufficient rapidity by any legal action initiated by the HMI itself.

The conditions for action we have described were perceived as trouble at all actor levels. The HMI scientists dependent on neutron scattering had no prospect of beginning the work that had been planned with the new research reactor. In the group directly involved in constructing the reactor, the only persons whose work was not affected were those involved in equipping the reactor with the various measuring instruments needed later for the experiments, a process referred to as instrumentation; they thus perceived no trouble arising from the changed situation.

The management of the project group initially believed that the project was doomed. The HMI management furthermore felt that the existence of the HMI as a National Research Center facility was acutely jeopardized, fearing a drastic reduction in size or even the closing down of the HMI as plausible scenarios.

The new situation offered hardly any new possibilities for successful coping. The limitations from the preceding phase persisted; the reasons for the trouble could not be eliminated. The predominant reaction was once again waiting, whereby the helplessness characteristic of this period of waiting was augmented by elements of strategic waiting for the possible disappearance of the trouble after the election. The strategic aspects of this waiting were expressed, for example, in an attempt to persuade the BMFT to defer its decision on withdrawal from the financing partnership until after the election to the House of Representatives in December 1990. This is once again indicative of the complexity of the troublesome situation and the activities of the actors involved. In regard to one troublesome event (refusal to issue the partial building permit), HMI's action can be classified as strategic waiting; in regard to another (BMFT's threat to withdraw from the HMI), as prevention.

Public relations work increased, but remained qualitatively the same. The heterogeneous attitudes of HMI personnel to the BER II and the strong public opposition to the reactor prevented effective collective public relations activities. Thus, the attempt to organize a demonstration by staff in front of city hall failed because only a few members of the HMI were willing to participate, even among those from the most directly affected sections. A "common denominator" was found, however: The HMI staff sent the Governing Mayor an open letter which was also published in the large Berlin dailies. A meeting of the Committee on Research with Neutrons at the HMI was also taken advantage of by the HMI management to initiate an open letter to the Governing Mayor.

When the third partial building permit was officially refused, the last resort open to the HMI was to challenge the decision before the Berlin Higher Administrative Court. More than anything else, this was a symbolic gesture on the part of the HMI demonstrating its resistance to the licensing authority's decision; it was clear that litigation would be very time-consuming and hence unsuitable as a means of actually eliminating the trouble in time to save the reactor.

In addition to coping with the external conditions for action, HMI also took preventive measures to alter the internal conditions for action. The HMI management tried to defer filling vacant posts and to spread investments in order to keep a number of options for action open in the event that the BER II's operation continued to be obstructed for a long time.

It was not until this third phase of trouble that the scientists directly involved in neutron research at the HMI actually began to react to the uncertainty regarding this research method's future at the institute. Many scientists began to look more intensively for employment outside the HMI; there was a drain of scientists and technicians. Among those who stayed, some made attempts to develop alternative methods which could be substituted for neutron scattering.

The trouble ended abruptly when the SPD and the AL lost the election to the House of Representatives on 2 December 1990 to the CDU. It had already begun to recede somewhat earlier, however; the SPD-AL coalition had collapsed, and an SPD senator revoked the refusal to permit further construction on the BER II. Soon after the change of government, the operating permit was issued. The BER II reactor started operating in 1991, 16 years after the initial idea for upgrading and 12 years after the HMI had filed the application. The disappearance of trouble could not be attributed to action by the HMI, however. Instead, the hopes vested in (at first helpless and then strategic) waiting for the trouble to disappear were fulfilled.

2.3 The Decision-Making Processes Relating to the FRM II in Munich: Coping with Trouble?

In Munich, too, the desire to increase performance of the research reactor FRM I arose in the second half of the 1970s. Various ideas on how the neutron flux of research reactors could be raised led to a distinct technological concept which incorporated the results of the research program on enrichment reduction[13]. Initially, the scientists only planned to upgrade the FRM I, but later they pursued plans for upgrading and new construction simultaneously. As a result of informal preliminary negotiations with the licensing authority, in this case the Department of State Development and Environmental Protection of the State of Bavaria, it was finally decided to build a new reactor (*Forschungsreaktor München II*, FRM II).

13 The results of this research program make it possible to use less highly enriched uranium in research reactors that are already in operation without lowering their performance levels, and to build new reactors which achieve significantly higher performance levels than the old ones while using uranium which is less highly enriched.

In Munich, the science policy phase was "encumbered" from the outset by a particularly complicated actor constellation – especially if compared with the HMI's parallel situation in Berlin. There, one institute had to interact with only two actors: the State of Berlin provided only 10% of the funding, following the decisional lead of the BMFT, which came up with the remaining 90%. This virtually "monogamous" relationship contrasts sharply with the complicated network surrounding the FRM II, whose complexity could be attributed to a different legal framework and a more elaborate financing structure. The network's actors included:

- An institute of the Physics Department at the Technical University of Munich (TUM), which also operated the FRM I, was the institution *actually* responsible for the project (in the sense of performing the scientific groundwork and providing the organizational framework required for construction).
- The Technical University of Munich submitted the grant application and served as the official negotiating partner; hence, to the actors outside the university, it was *legally* responsible for the project,[14]
- The Bavarian State Department of Education and Culture, Science and Art, representing the Bavarian state government, is in turn *politically* responsible for the TUM since the universities fall under the cultural sovereignty of the federal states.
- Commissions within the Science Council were charged with making recommendations on the implementation of the University Construction Act (*Hochschulbau-Förderungsgesetz*), the law ensuring that the federal government would supply a large portion of the financing for construction.
- The BMFT, finally, was also to contribute to the financing of the project; this in turn set off internal decision-making processes requiring consultation among the ministry's experts.

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14 While in the case of the HMI no distinction need be drawn between the project group and the HMI management as far as interest in the project was concerned, it seems appropriate to do so in the case of the TUM. Because the university subdepartments enjoy more autonomy than the sections of the more hierarchically structured HMI, it cannot be ruled out that controversy within the TUM as well as differences of opinion between the university authorities and the project group played a role in the decision-making processes.

The success of the project depended on obtaining and maintaining the approval of all these partners. From the start, each of them has had[15] the power to topple the project or let it "starve" by means of delays.

The project group's activities aimed at obtaining approval from all the necessary political actors began in the early 1980s. In 1984, the BMFT started supporting the project; in 1985 the BMFT decided against building the spallation source in Jülich (see 2.1). The reaction of the scientific community was to favor the FRM II project, which was further along in its planning than other institutes with similar projects.

In 1986, after the Advisory Committee on Basic Research in the Natural Sciences had expressed its approval of FRM II, the BMFT declared its willingness to contribute a fixed sum towards financing the FRM II. In the same year, the Science Council also stated its conditional approval of the project.

A lengthy planning period followed during which implementation of the project proceeded rather haltingly despite the support promised by all parties. In 1987 the project group was still assuming that construction would begin in 1990, and that it would be possible to begin operating the reactor in 1993.

The explanations for the delay are contradictory, some citing technical changes, others disputes over costs and financing, still others the slow processing by the Bavarian state government despite its basic approval. Thus, the first draft of the safety report took four years (1987-1991) to complete.

Despite the complicated actor constellation, the decision-making process continued until the summer of 1991 at a pace that can be considered normal for science policy, the only unusual aspect being the extraordinarily long planning phase of 10 years following submission of the scientific concept.

At that time, there was no perceivable risk to the project; it attracted hardly any public attention, with the exception of a few inquiries in the Bavarian state parliament and a meeting organized by the Greens in Garching to inform the public.[16]

In September 1991, a pamphlet entitled *Alternatives to the Research Reactor Munich II* was published by a group of scientists and students of the Technical University of Munich. Most of the scientists were younger members

15 The science policy decision-making phase is not yet concluded.

16 The FRM I is part of the research complex located in the immediate vicinity of the town of Garching on the northern periphery of Munich. The FRM II is to be built next to the old reactor.

of the physics department to which the FRM II project group also belonged. The pamphlet levelled criticism at the plans for the new research reactor, suggested spallation sources as an alternative solution, compared the two options, and discussed the various forms of institutional control and possible sites for the new neutron source. The pamphlet was sent to scientists, politicians and the media, but did not spark much interest within or outside the physics department at first.

In contrast to what had happened at HMI, scientists opposing the project got together and spoke out against the FRM II project publicly. The fact that this occurred at the TUM rather than the HMI can be primarily attributed to the difference in size between the two institutions, to the greater openness of university structures, and to the far-reaching autonomy of university institutes (the opposition arose in a section of the Physics Department to which the project group FRM II did not belong).

At the very same time, in September 1991, a citizens' action committee opposing the new research reactor formed in Garching. This was the first indication that environmental issues might be raised before the science policy process had been concluded. Neither the publication of the pamphlet nor the founding of the citizens' action committee led to a perceptible change in the situation of the project group.

However, it soon became evident that these two events could mean a distinct deterioration in the conditions for action of the project group when a small incident occurred that, in itself and in retrospect, seemed rather insignificant. In late October it was discovered that tree roots had damaged a drain on the reactor grounds, and that water polluted with tritium had escaped into the surrounding soil. The citizens' action committee opposing the construction of the FRM II seized upon this first opportunity to present its case to the public: As soon as it learned of the accident, it sharply attacked the operators of the old reactor (who were also responsible for construction of the new one). As it turned out, the management of the old reactor had indeed neglected its supervisory duties. In the public discussion that ensued, the citizens' action committee also attacked the planned construction of the FRM II and drew attention to the pamphlet, which thus became known and was quoted in numerous press reports. There was the usual outcry in the media and among politicians, but it died down quickly. It was later discovered that the maximum permissible radiation levels had not been exceeded, and that the contamination had not come from the reactor at all, but from another institute working with radioactive materials.

Criticism of the planned construction of the FRM II and the existence of possible alternatives to this project had, however, become established in public debate. This discussion, unusual for the science policy phase, was triggered by the coincidence of criticism from within the scientific community, organized public protest, and an accident apt to be exploited by the media. Furthermore, the procedures in the science policy phase, which had been informal and discreet until now, suddenly became the subject of public scrutiny. The project group and the physics department were confronted by a barrage of queries from their negotiating partners in the university management and the Bavarian government, all aimed at finding out whether the physics department fully supported the FRM II project. The pamphlet had triggered substantial doubts in this respect; some actors in the project advocate network were quite annoyed with the authors of the pamphlet.

From this time on a gradual deterioration in the project group's conditions for action can be observed. It is hard to say, however, whether or not the situation was becoming troublesome, i.e. whether goal attainment was becoming impossible or merely more difficult. We will begin by describing the origins of the troublesome structures in the conditions for action and how the project group perceived them, and then return to the question of how to characterize the situation.

In Munich, as in Berlin, troublesome features of the conditions for action emerged from a cognitive-technological background. In this case, the background was the existence of a technological alternative for producing neutron beams (spallation source), whose applications partly overlapped with and sometimes complemented those of research reactors (see footnote 4). But there is no spallation source project in the offing in Germany; hence, while this may be a compelling argument supporting the opponents of the FRM II, it does not constitute a viable alternative for research policy makers at this time. The cognitive-technological aspects of the disposal of spent fuel elements that had been predominant in Berlin were not as important in Munich at the beginning, the only problem in this area addressed by FRM II opponents being the proliferation risk involved in using uranium suitable for weapons manufacture. The project also had considerable financial difficulties. The cost estimates upon which the financing negotiations had been based became obsolete and had to be greatly increased. The time factor now became increasingly important because the general inflation rate alone meant that the project could be expected to grow more expensive with each passing year. Since the BMFT's financial commitment had been a fixed sum, negotiations on "index-linking" this amount had to be conducted. The contributions required from other financial backers increased as well. The most significant effect of the pamphlet and of the incipient public discussion was, however, the uncertainty sown within the FRM II advocates' network and among the actors whose approval was legally required. Until autumn of 1991, the project group managed to assuage the doubts of all the backers. Since the project was still in the science policy phase dominated by informal coordination processes, the actors who had just entered on scene had no opportunity to intervene in the decision-making processes. On the other hand, the science policy actors, particularly in Bavaria, were subject to public pressure to justify their positions. The situation of the advocates' network was rendered more complicated by the fact that a public discussion had arisen in which all the arguments typical of the environmental policy phase played a role. Now, science policy makers, at least in Bavaria, had to take these environmental arguments into consideration when making decisions about FRM II. Furthermore, support for the project at the federal level (from the Science Council, for example) was now jeopardized; the sluggish progress was threatening to block other projects.

Undoubtedly, the beginnings of trouble can be found in the situation described above. Since this deterioration in the conditions for action was accompanied by very slow progress in the science policy process, an increasingly ambivalent situation emerged. There were certain indications that the situation was indeed becoming troublesome.

- A public debate and criticism of the project had developed at a time when important actors could still withdraw from the project with relative ease, namely in the science policy phase *before* final financial commitment. The articles published in the media were for the most part critical of the project.
- The discussion on the alternative technology of spallation cast doubt upon the scientific argumentation of the project's advocates. For the lay public, the situation was unclear, but it had become apparent that the FRM II project was not the only solution to the scientific problems. Although no alternative was in the offing, the fact that alternatives did exist had been put on the table.

- Simply ignoring the opposing arguments was impossible, since the project group and the advocates of the project were subject to strong pressure to justify their positions.
- Time seemed to be working against the project because of the growing environmental policy discussion, because of cost increases, and because of the difficulties in sustaining support for the project among all the actors involved.

On the other hand, in light of the progress made by the project group in the science policy process, characterizing the situation as being actually troublesome seems inaccurate. All the political actors mentioned above maintained their support, in some cases explicitly restating their approval (see the statements on the present situation with regard to the policy process at the end of this section).

The project group's perception of the situation also seems to argue against defining it as being troublesome. They regarded the pamphlet as being scientifically insignificant, considering it unfavorable only from a political point of view. Its authors were thought to lack the professional competence necessary to develop a qualified opinion because they were either not directly involved in neutron research, or they were too young and inexperienced, or both; from the project group's point of view, neither undergraduates, graduate students, nor postdoctoral candidates possessed enough knowledge to formulate serious criticism.[17] Making this clear to the network of advocates and the public was one of the coping activities of the project group.

Nevertheless, the project group had to acknowledge that their conditions for action had worsened. The public debate in the media was marked by a negative image of the project, and the political advocates of the project were beginning to have doubts. It is important to note, however, that the changes in the conditions for action were not perceived as endangering goal attainment, but as imposing a shift in scientific action (see Section 1).

The analysis of the conditions for action and of their perception by the project group thus shows a fluid transition towards trouble, the situation being ambivalent and difficult to categorize due to the progress being made in the

17 This assessment culminated in the statement: "They even let cleaning ladies sign!" This is indicative of the general perception pattern and defense mechanism of scientists subjected to criticism which is described by Heine (1992) in relation to chemists in industry.

area of science policy. This shows that not only sudden transitions, but also gradual developments can lead to troublesome situations. It is probable that, in most cases, situations are genuinely perceived as being troublesome when sudden changes occur, whereas a number of modifications in a situation occurring incrementally and obscured by complexity will result in the "mild" perceptions described in Section 1.

There is at least some indication of trouble in the fact that it is possible to describe the project group's various activities aimed at improving the internal conditions for action within the TUM and the external conditions as coping.

Partly on the urging of external project advocates, representatives of the project group and TUM scientists working with neutron beams met with three authors of the pamphlet at the office of the dean of the Physics Department in order to talk the authors into retracting the pamphlet. The pamphlet was not taken out of circulation, however, which can be partially attributed to the fact that the opponents within and outside the department had mutually stabilized each other (much the same as had occurred in Berlin). Nor did the project group insist upon retraction, even though project advocates outside the university had been thinking along these lines. In order to emphasize how completely it supported the project, the Physics Department did, however, hold a vote resulting in a show of unanimous support for the FRM II project.

At the same time the project group had to maintain an intensive dialogue with the actors of the advocates' network in an effort to eliminate the irritations that had arisen when the pamphlet was published. While the pamphlet could not simply be whisked away, these activities did go a long way to soften its negative impact.

The project group's public relations efforts were hampered for a long time by the fact that the university press office, which was responsible for contacts with the media, proved unable to react either adequately or promptly enough to keep pace with the public debate. In retrospect, the representatives of the project group feel that far too little attention was paid to public relations. Public relations activities were expanded as of autumn of 1991. Representatives of the project group spoke at various public events, especially in the town of Garching and the surrounding communities. The scientific director of the FRM I, the head of the project group FRM II and two deans of the University put together an information brochure on the FRM II project, which addressed, among other things, the arguments against the new research reactor spelled out in the opponents' pamphlet. The brochure was targeted mainly at local politicians in the surrounding communities.

When the project group applied for a permit in spring of 1993, numerous critical reports appeared in the press. Thereafter, the group organized and expanded its public relations, designating one of its members to be the group's PR expert. In addition, the project group obtained professional assistance from science journalists.

While all these activities – which can very well be categorized as coping – were taking place, the project itself had not been modified. The activities had been aimed solely at influencing the general conditions within and outside the scientific community. The fact that the FRM II group had a greater variety of moves it could make, when compared with the group in Berlin, was due to the greater openness of the project, the imperatives arising when internal criticism of the project became public, and the larger number of actors involved. With regard to project design, the project group came to the conclusion that there were basically no modifications possible in response to the criticism. Containment against aircraft accidents had already been planned (to prevent possible objections from the public in the course of the approval procedure). Conversion to less highly enriched uranium was rejected by the project group because of the loss of performance. Giving up the project altogether was not within the spectrum of conceivable action.

At present, the situation is characterized by the fact that the environmental policy phase has begun before the science policy phase has been completed. The new estimate of total costs is about 50% higher than the estimate upon which the original financing agreements and commitments were made. In January 1993, after hesitating for quite a while, the Bavarian government declared its continued support for the FRM II project. Negotiations are now under way between the state of Bavaria and the BMFT on increasing the Federal government's share of funding for the project. Because of the ongoing financial negotiations, the contracts with the company which is to build the FRM II have not been signed. The TUM filed an application with the Bavarian Ministry for the Environment on 4 February 1993 for the construction and operation of the new research reactor, and submitted a draft of the safety report. It has also filed an application with the authorities of the region of Upper Bavaria requesting the initiation of a regional planning procedure. However, the local communities have refused all comment on this procedure for the time being because they have not yet been given access to the safety report.

At the end of April 1993, four committees of the Bavarian State Parliament invited experts to take part in a hearing on "The renewal of the high flux neutron source of the Technical University of Munich in Garching." The spallation source as an alternative technology was overtly discussed in the course of this hearing. The line of argument supporting it has since consolidated into an actual alternative decision. The state of Bavaria has been invited to cooperate in developing and using the Austrian spallation source AUSTRON.

The perception of project group scientists with regard to the chances of success for FRM II varies considerably. It ranges from "fifty-fifty" to complete optimism. The main dangers are perceived as being

- financing problems, especially regarding rising costs in the course of construction works or due to additional safety requirements,
- time losses that can lead to (inflation-related) cost increases, and
- a change in government, especially the formation of a coalition between the Social Democrats and the Greens.

The project's situation continues to be complicated, still hovering between everyday routine and trouble.[18] The financing could founder at any time, since all the actors involved are still in a position to withdraw from the project. The fact that the political actors are processing the application so sluggishly, which is still criticized by some project advocates, indicates that there may well be a number of actors with a certain interest in the project's bloodless demise. The environmental policy phase now getting under way modifies the possibilities of the project's opponents to intervene by granting them formal participatory rights. This can cause problems for the project, especially because the science policy phase is not yet over.

If one analyses the present situation, it is clear that the project group's coping activities have enjoyed only partial success. While the group has succeeded in stabilizing the network of project advocates and filing the application to build and operate the reactor, it is also faced with an established network of project opponents who are evaluating the project against the back-

18 The following comments relate to the state of affairs as of 3 May 1993.

ground of a scientific alternative. As was to be expected, the researchers have also not been able to influence the critics whose arguments center on safety aspects. The arsenal of coping activities available to the project group thus appears to be exhausted. The only area where there still seems to be room for maneuver is public relations, which could be increased and improved. The waiting period has begun . . .

3 Some General Reflections: So Much Trouble and So Little Coping

3.1 The Origins of Trouble

In both cases described above, the decision-making processes (which we have frequently referred to as policy phases) began when purely local scientific and science-policy interests were formulated. A neutron source was available that for various reasons no longer met the increasing demands placed on its performance by the group or institution operating it. In both cases the approval of the scientific community and the hybrid community was sought for a project that had been formulated at the local level. At the level of the scientific community, differing (competing) interests were balanced out internally, so that the politicians were in each case presented with only one project for approval.

The approval of the scientific community and the hybrid community are necessary conditions for the realization of projects, since no favorable decision on financing will otherwise be forthcoming. While the informal preliminary decisions and the subsequent formal financial undertakings are not secret, they are arrived at in such a manner that the public hardly becomes aware of them. Formal participation by the public in science policy-making processes does not exist. Environmental concerns and safety precautions are technical aspects not dealt with in this phase.

This focus on the purely scientific aspects of the decision leads to the general isolation of science policy decision-making processes from public attention and, hence, from public debate. Research reactors are, of course, research devices; according to the Atomic Energy Act, they are also nuclear facilities. As policy is developed, they are treated exclusively as research devices at first, and later exclusively as nuclear facilities. The decision as to whether a research device is to be built and what technical specifications it is to fulfill is initially an autonomous one made jointly by the scientific community and the political actors responsible for this field.

In the planning stage, that is to say toward the end of the science policy phase, intensive informal preliminary negotiations take place between the project group and the licensing authority (Pfingsten/ Fietkau 1992: 9-11, Beyerlin 1987). This sort of preliminary negotiation generally leads to an informal bilateral agreement regarding all questions relevant for approval. While it is not legally binding, this constitutes a de facto agreement to which the licensing authority considers itself committed. These preliminary negotiations almost always take place to the exclusion of third parties, presenting the latter in the subsequent administrative procedure with *faits accomplis* and thus to a large extent undermining third party participatory rights (Beyerlin 1987: 2713). The subsequent environmental policy phase is so constrained by the informal preliminary negotiations and the slow, complicated proceedings of the administrative courts that the public – particularly the most directly affected citizens (neighboring residents) – has little real opportunity to intervene, although it is legally entitled to participate.

These are the three principal factors that can avert or inhibit the emergence of troublesome situations: the isolation of the science policy phase both from other policy-making domains and from the public, the informal preliminary negotiations between the project group and the licensing authority, and the constraints framing the environmental policy phase, i.e. the preliminary negotiations and the practice of the administrative courts oriented toward conflicts over nuclear power stations.

How then can troublesome situations develop at all? The decision-making processes in Berlin and Munich indicate two possibilities: the partial overlap between the science policy and the environmental policy phases, and political turbulence during the environmental policy phase.

3.1.1 Policy Phases Overlap

In the science policy phase, the initiators' main goal is to obtain the approval of all relevant political actors, i.e. to bring about a (favorable) decision. In the environmental policy phase, the initiators' main goal is to establish that the fully elaborated project conforms with federal, state and local laws; this is an easy task if informal preliminary negotiations with the licensing authority have taken place. In the formal environmental policy phase that follows, the licensing authority has a certain amount of leeway; hence, slight modifications of the project based on objections raised by third parties may have to be made before a building permit is issued. The two main phases also differ in terms of the options open to the actors and the decision-making criteria they apply. In the science policy phase, the initiators may be competing with other large-scale projects. There may be different options regarding the project design as well, in terms of performance specifications, for example. But this is improbable, since the project proposal is agreed upon by the local group before it is presented to the political actors. Decision-making criteria emerge from assessments of

- the quality of the project and its functions in research,
- the potential impact of the project on individual facilities, on research disciplines, or on the country as a whole in terms of its status as a research promoter, and
- projected costs and possible sources of funding.

A decision in favor of the project always means that an actor or a number of actors have committed funds. This is the most precarious point in the decision-making process, because it commits the actors to supporting the project.

In the environmental policy phase, there are two possible paths. The project can be implemented, perhaps with modifications, or its implementation can be obstructed. Research concerns and science policy considerations are only involved here to the extent that they justify the "public interest" in the project. Here, the debate is dominated by the safety issue. The discussion on how safety requirements are to be met can lead to considerable delay in granting approval, and, if modifications are imposed, to increased costs.

If, as in the case of Munich, issues and decision-making criteria in the environmental policy phase "diffuse" into the science policy phase, the project may be jeopardized, i.e. a troublesome situation may arise. When this happens, the science-policy decision makers are forced to take direct cognizance of environmental considerations and anticipate the debates to be expected in this coming phase. The fundamental decision on financing the project is thus confronted with strong (and generally critical) public interest as well as possible delays, cost increases, and threats to the project posed by the pending licensing procedure. Furthermore, scientific alternatives to the project that offer higher safety levels are judged more favorably, thus reducing the project's prospects for success regardless of how feasible the alternatives may be. This is what happened in Munich, when the science policy phase was prolonged unexpectedly at the same time critical public awareness surfaced. The advent of troublesome events in the decision-making processes in Munich shows how trouble develops when "diffusion" described above occurs, i.e. when one phase interferes with another. Although perception of trouble is frequently triggered by acts of political actors, the advent of troublesome situations cannot be attributed solely to individual actors:

a. Changes in conditions for action occur within the context of a range of possibilities offered by cognitive-technological facts. In each of the decision-making processes, the conditions for action defined by the technology of research reactors provided project opponents with opportunities to act while limiting the opportunities for project advocates to do the same.
b. In the case of situational changes caused by political action, trouble can be triggered in very different ways. Each of the two decision-making processes was affected by outside influences of varying degree. The one extreme was in Berlin, where hitherto favourable conditions for action were suddenly transformed into very unfavourable ones. The other extreme was a marginal change in perception in the Munich case: The conditions for action having been far less than favorable for a long time, the outside influences merely triggered a reevaluation of the situation. The aggregate impact of a large number of interventions can, as was the case in Munich, also induce a shift from everyday conditions for action toward troublesome ones.

3.1.2 Political Turbulence in the Environmental Policy Phase

In the phase in which environmental policy decisions are made, a project is only directly threatened if the licensing authority wants to terminate it, which at the same time presupposes that the latter has not come to a de facto agreement with the project group in the course of informal preliminary negotiations. An indirect threat is always posed by delays and by obligations to myodify the project. Such influences can give rise to a situation in which putting the research reactor into operation is relegated to the indefinite future, or in which the costs increase dramatically because new and stricter safety requirements are imposed. Both scenarios can lead to the sponsors withdrawing from the project. The project in Berlin, for example, was directly threatened. A particular political constellation first led to an unresolved technological problem being used to prohibit the licensing of the research reactor. However, even under normal conditions, i.e. if it had been forced against its will to grant operating permission, the licensing authority would have been in a position to make smooth research operation of the BER II impossible by constant intervention. By now, such a situation has been clearly recognized as a possible risk in Munich as well.

The two types of troublesome situations are characterized by the fact that at the very least the group for whom the research reactor is both the subject matter of their work and a non-substitutable research instrument would be unable to continue their research work. In the case of the HMI there was the additional circumstance that the BER II as a large scientific instrument was constitutive to the organizational identity of the HMI, so that a threat to the project was a direct threat to the existence of the institution.

The research reactors were important for sustaining research – in Munich at the scientific level, and in Berlin at the science-policy level. For these reasons, every autonomous decision taken by the local groups or institutions had to be directed toward carrying on scientifically with the type of research conducted up until then, and carrying on technically with the same type of large instrument. Vice versa, every intervention which contravened this autonomy and was directed against the project on the basis of other criteria had to represent a vital threat.

3.2 The Limits of Coping

Precisely the multifarious coping activities of the scientists and institutions clearly show that the spectrum of action brought into play is very narrow. We will examine it systematically in terms of the subject matter to be coped with.

A *change in goal* in reaction to the project's existence being threatened would have meant abandoning work with a research reactor. In Munich this would have also meant giving up research work dependent on the availability of a research reactor, such as the development of measurement techniques. In Berlin a change in goal of this magnitude would have required developing a new profile for the HMI. Such profound changes in the research goal and profile as a result of external pressure seem to be completely impossible. Changing their goal so completely would have forced the scientists to move into new specialties without being able to build on their previous ones; they would not have even been able to build on earlier ideas. Moreover, this change would have been a response to lay objections. Thus, there are two reasons why a shift in goals could not take place. First, at the cognitive level, research that starts from scratch has almost no chance of succeeding. Second, such a procedure would contradict established norms within the profession as well as the profession's self-image.

A *change in the evaluation of conditions for action* to the effect that these conditions are no longer perceived as troublesome will only succeed in the short term. In our two cases, the extreme character of the troublesome situations makes it unlikely that a change in evaluation really would have helped. Since the situation described in Berlin constituted a threat to the very existence of the project, an adjustment of evaluation of the conditions for action would have been possible only if the research goals were abandoned completely. This, however, was impossible (see above). Regarding Munich, the question of whether a change in the evaluation has occurred – i.e. whether the conditions for action are still considered (barely) adequate for attaining the project's goal even though they are continually deteriorating – cannot be answered right now. The empirical identification of such subtle changes would presuppose accompanying observation as a method of inquiry, which was unfortunately beyond the capacity of our project.

If improving the *conditions for action* is the object of coping, the following strategies can be distinguished from the point of view of *research institutions:*

1. eliminating factors triggering trouble by changes in project design;
2. exerting pressure on the originator of the trouble, especially by mobilizing the network of project advocates and other actors and influencing the public;
3. attempting to influence evaluation of the conditions for action by actors perceived as originators of the trouble, especially through intensive public relations work; and
4. creating organizational slack by changing the internal conditions for action in order to support externally directed coping activities or in order to compensate the consequences of trouble.

Scientists can (if they belong to one of the relevant groups of users, see 2.1) change jobs, i.e. seek conditions for action permitting them to attain their goal.

Changes in project design were implemented prophylactically in informal preliminary negotiations with the licensing authority. This type of prevention has become standard practice by now and does not necessarily presuppose trouble. Activities following commencement of the approval procedure no longer focus on changes in project design, but rather on proof of the facility's safety. It is, in fact, impossible to adapt the project to meet the demands of the originator of the trouble, because they usually call for discontinuing the project. This is an intolerable alternative for the scientists (as explained above).

In order for Strategy 2 to succeed, relatively solid support of the project within the organization is required, along with a variety of competently prepared public relations activities. Some coping activities were aimed at creating these preconditions.

In the case of the BER II project, Strategy 2 mostly involved *waiting*. This waiting was prompted by hopes that the trouble would disappear and, particularly, by the awareness that no action was possible.

In effect, the range of coping activities open to project sponsors is small. Abandoning or considerably modifying the project is taboo, and the slight modifications that are possible cannot decisively transform the troublesome situation. Compensating for this sort of trouble is scarcely possible, since it would require a shift in research goals. There remains the possibility of intervening at the political level in order to eliminate the troublesome conditions for action. In fact, coping in Munich and in Berlin mainly involved such efforts. The network of project advocates, often in a better position to perform this type of coping, provided the research institutions with substantial support.

In the final analysis, there are severe constraints circumscribing the shift in research goals, the adjustment of evaluation, and the changes in conditions for action. They stem from the fact that, for cognitive and normative reasons, the scientists and institutions facing trouble categorically reject a coping strategy of substituting present research goals or methods with new ones.

3.3 Does the Scientific Community as a Whole Cope with Trouble?

The outcome of the empirical analysis supports our initial hypotheses. The fact that the coping with trouble examined in this paper involves a debate on the essence of research autonomy raises the question of whether local coping with trouble is integrated into the behaviour of the entire scientific community.

At the level of the scientific community of physicists, the growing difficulty in achieving social acceptance of research reactors is being increasingly taken into account when new projects are discussed. The schedules for new projects involving neutron sources are more generously calculated, allowing time for expected conflicts.[19] There are even indications that the decision-making processes will, in the interest of long-term prevention, come out in favour of the alternative, safer neutron source: the spallation source.

With regard to projects for new neutron sources, the community of neutron researchers is in a different situation than the local sponsors. Although the overall supply of neutron beams will be reduced if a particular project fails, the continuation of research is not threatened in any of these individual cases. At the level of the scientific community and the hybrid community, threats to an individual project and the related coping with trouble lead to preventive integration of decision-making criteria external to science into established internal scientific discussions.

The question arises whether societal discussion of research reactors (and of genetic engineering, animal experiments, clinical experiments on human beings), which always leads to individual coping-with-trouble processes, indicates the beginnings of a de-differentiation process. If science has overstepped the boundaries of the laboratory into society (Krohn/ Weyer 1989), society, too, has overstepped those very boundaries in the opposite direction, making its presence felt in the choice of research methods: in individual cases via coping with trouble, and globally via the prophylactic modification of decision-making criteria by scientific communities and hybrid communities. This

19 Although the scientific community is not an actor capable of making decisions, it influences decision making in the science policy process via the attitude it assumes towards all such projects. The extent of this influence is at least so great that a large-scale scientific project is unlikely to be realized without the implicit approval of the scientific community (see van den Daele/ Krohn/ Weingart 1979).

interference, which is now perceived in each individual research process as an *infringement* on scientific autonomy and hence triggers coping with trouble, might be indicative of an historical trend toward a *substantive change* in scientific autonomy. Society, exposed by science to qualitatively new dependencies and dangers, forces scientists to anticipate these very effects within the framework of internal scientific decision-making processes. If we look at the debate on alternative research methods in neutron research, the development of alternatives to animal experiments, and genetic engineering, we see a shift at the level of the scientific community. When a certain type of research is prohibited, a long-term change in the scientists' own preference for particular methods is quite possible, and greater emphasis may be placed on alternative research methods which pose fewer risks to society.

The emancipation of science from society, a process that has been going on since the Middle Ages, may thus be undergoing a dialectical negation (in Hegel's sense of the term). Society, having been banished from science due to the threat it posed to research, is now being "gently" reintegrated because of the threat scientific research now poses to society. Coping with trouble as it is analyzed in this chapter appears to be one of the forms this reintegration can take.

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Space Policy in West Germany 1945-1965: Strategic Action and Actor Network Dynamics

Johannes Weyer

1 Introduction: Purposive Actors and the Autodynamics of Social Networks

Empirical studies on the history of West German space policy present us with a variety of actors faced with trouble who sometimes manage to cope, and sometimes do not. However, most examples analyzed in this chapter fit neither into the pattern of total success nor into that of total failure. Most frequently we find what will be referred to here as *successful failure* or *unintended success*, indicating that the key actors were usually able to attain (at least some of) the goals they had set for themselves, but also produced effects which were either unintended, suboptimal or, in the long run, even counterproductive.

This chapter attempts to explain the phenomenon of successful failure by analyzing the mutual interrelations of coping activities and troublesome activities of different actors. An interactive approach will be developed – based on the concept of the purposive actor borrowed from Max Weber, James Coleman, Humberto Maturana and others – which claims that trouble is a cause of coping reactions and that, in addition, coping activities are a trigger of new trouble (either for the acting unit or for other coplayers).[1] Thus, coping and trouble-inducing activities cannot be distinguished systematically; the distinction depends on the view of the respective actor and the interdependen-

E. Peter Germain's translation of the first draft is gratefully acknowledged.

1 Cf. Weber (1985); Coleman (1990); Maturana (1987). See also Stucke and Musselin/ Vilkas in this volume.

cies generated by the interplay of trouble, coping and, again, trouble – sometimes an endless chain. One main argument presented here is: It is necessary to create a notion of the actor in order to understand why he or she reacts (to trouble), acts (strategically, for example, to avoid trouble), and finally gets into trouble or creates trouble for other actors.

It would, however, be misleading to conceptualize social interaction as a simple sequence of trouble (as a trigger of coping reactions), coping activities (as a source of new trouble) and so on, being produced by different actors who are isolated from each other. The success of actors' strategies (of which coping strategies are a subgroup) depends to a considerable extent upon establishing a consensus of interest among strategically acting actors, which in turn is manifested in the creation and stabilization of a social network. Such interest-based social networks form the foundation for social and technical innovations; they are also the social space enabling the participating partners to exercise a mutual influence on each other. For our analysis, the fact that actor networks can develop autodynamics and thus become a source of trouble in their own right is very important.[2] An actor network represents an emergent phenomenon obeying its own rules over which none of the players exercises exclusive control. Maintenance of the network can turn into a serious constraint when further participation comes to depend upon the very existence of the network. In this case, the actions of the participants are more strongly determined by the requirements of the network than by their own individual interests. At this point, questioning the social logic of the network increasingly comes to mean questioning oneself. The alternative becomes: continue playing the game or quit.

Although they are a result of previous coping activities, social networks can develop internal dynamics whose effects may provoke ambivalent or even negative feelings from the participating actors. Central protagonists begin to view themselves as victims of a process that has led to suboptimal results for all participants, although they all have played a prominent role in creating it. Therefore, two levels of trouble can be distinguished analytically: the first resulting from actors' uncertainty about the future and their doubts about being able to succeed at implementing the goals they have set for themselves, the second resulting from the (sometimes very rigid) constraints social net-

2 Readers will note that my notion of an actor network differs from the notion developed by Latour (1988) and Callon (1991).

works can exercise. This second level of uncertainty depends upon the probability that a social arrangement will work whose function is to facilitate the success of the strategies at the first level. In order to understand the interplay of trouble and coping, we must not only develop a notion of the purposive actor, but also comprehend the (auto)dynamics of social networks and the mechanism of self-production of social constraints induced when strategically acting actors form a network.

A case study from the history of West German space policy will serve to illustrate the hypothesis sketched (very briefly) above.[3] After explaining the function of the private rocket and space associations and their contributions to the revitalization of the policy field of "space flight" (Section 2), the analysis concentrates on the extrauniversity aeronautics (later: aerospace) research institutes and their contribution to the creation of that policy field during the period from 1945 to 1965 (Section 3). After a short summary of the case study, theoretical conclusions will be discussed (Section 4).

2 Early Attempts to Reestablish the Policy Field of "Space Flight" after 1945

To set the stage for the case study, the development of German space policy will be reconstructed up to the moment when the aeronautics research institutes became the key actor. As indicated above, this history shall be presented as a sequence of successful failures and unintended successes which can be related to the interplay of trouble and coping.

2.1 Hobby Rocket Constructors in the Immediate Postwar Period

In 1945, the space (as well as the aeronautics) research community was in big trouble. Research activities had come to a standstill because of the destruction of many of the facilities, the lack of resources and the (generally) prohibitive policy of the Allied occupation forces. Immediately after the war,

3 For a more detailed analysis of this case, see Weyer (1993a).

different members of the community started an – uncoordinated – series of attempts to preserve as much as they could of the still-existing potential in aeronautics and rocket research. The active researchers remaining in Germany made quite a variety of efforts, employing all manner of indirect schemes to ensure that a certain continuity was maintained. In the late 1940s, the rocketry community reorganized itself, although the only possible form of institutionalization available to it was private societies such as the Society for Space Research (*Gesellschaft für Weltraumforschung*, GfW), founded in 1948, or the Work Group on Rocketry (*Arbeitsgemeinschaft für Raketentechnik*, AFRA; later DAFRA), founded in 1952. For these societies, developing small rockets not only had the important function of demonstrating the self-confidence of the rocketry community, but also represented a suitable method for testing the limits of tolerance of the Allied occupying powers. Nonetheless, despite its importance for the revival of rocket construction, the DAFRA quickly sank into insignificance during the mid-1950s: The phase of private rocket construction as a hobby ended when political agencies began to become interested in this technology and rocket research was institutionalized in the form of federal big science centers. The DAFRA was transformed within a few years into the *Hermann-Oberth-Gesellschaft*, a private association of rocket research "veterans," which is still in existence but has played at best a marginal role in the policy field of "space flight."

2.2 The Development of Big Science

The initial phase characterized by the private rocket and space societies ended between 1952 and 1954, when the GfW was able to convince the Federal Ministry of Transport *(Bundesverkehrsministerium*, BMV) to support rocket research and to set up the first institute for rocket research in the Federal Republic, the Research Institute for the Physics of Jet Propulsion (*Forschungsinstitut für Physik der Strahlantriebe*, FPS), in Stuttgart in 1954. This event, which occurred before the Allied forces lifted their ban on research and two years before the first nuclear research institutes were founded, marked the beginning of the strategic interaction between science and politics in the Federal Republic. The fact that the BMV successfully reclaimed federal responsibility in questions of research and deliberately oriented the founding of the FPS along the lines of big science reveals that the contours of a new policy area were beginning to take shape. In the face of this development the private construction of small rockets was quickly pushed into the background.

But this rapid development had only been made possible by the GfW's prior occupation of the field through its public rehabilitation of space and rocket technology and its (coping) strategy of informal preinstitutionalization: It had paved the way for the BMV. It was primarily due to the GfW's deliberately pursued policy of integrating German rocketry into international space and rocket research that West Germany was able to reenter the field in this way – just a few years after the last German V-2 rocket had been fired. This policy included systematic efforts to improve the image of rocket technology; its peaceful nature was propagated tirelessly by GfW protagonists Heinz Gartmann, Heinz-Hermann Koelle and Eugen Sänger. A quote by Sänger reveals the argumentative tightrope walk they took: "Rockets are *not only* weapons, *but also* instruments of peaceful research" (*Beiträge zur Weltraumforschung und Weltraumfahrt* 1/1949: 14, italics added). For the Germans to get a new start in rocket research, presenting such an image was absolutely essential. In addition, the phrase coined by Sänger, "Raumfahrt als Verkehr" (space flight as transport) had a high legitimatory value for the federal transport ministry.

Setting up the FPS was undoubtedly an auspicious success, with effects going far beyond the single case. The social network created by the GfW and the BMV played an essential role not only in the establishment of "big science" in Germany, but also in constituting the policy field "research and technology," which helped to legitimate state intervention in research. At the same time, the network also produced constraints which became evident, for example, at the beginning of the 1960s when debates about a European space program started: The West German government sought at all costs to avoid creating the impression that it was pursuing rocket construction as a strictly national policy, possibly even for military purposes. This was, however, precisely the program that Eugen Sänger and his FPS were pursuing. Sänger refused to recognize practical constraints on his work, even accepting the lucrative offer of the Egyptian government to participate in the development of medium-range missiles in Egypt. This resulted in the collapse of the network after only a few years. The remains of his institute passed over into the hands of the largest aeronautics research institute. The remains of his institute passed over into the hands of the largest aeronautics research institute. The BMV lost responsibility for rocketry and space research, and later for aviation in general as well, disappearing altogether from the research policy area of West German rocket research which it had created in the 1950s – and which was to bear the BMV's stamp for some time to come. The social and political structures created during this phase (to which Sänger had refused to yield) remained; other actors entered upon the scene, pocketed the profits and continued the game on another level, until they, too, foundered due to their very successes.

3 The Battle for Autonomy and Control of Extrauniversity Aeronautics Research

In addition to what could be called the FPS precedent, it was the parallel developments in aeronautics research which were primarily responsible for the emergence of the policy field of "space flight" and the development of federal authority for technology policy. In order to understand the interplay of trouble and coping here, we will look at this case in detail.

3.1 The First Step: Informal Reinstitutionalization as a Coping Strategy during the Immediate Postwar Period

After 1945, the aeronautics research institutes – most of which had been founded at the beginning of the century or under the Nazi regime – were in maximum trouble: Aeronautics research was prohibited by Allied law, the facilities were either destroyed or confiscated, and the possibility of resuming research and development activities seemed to have receded into the distant future. In this situation, different groups of former members of the Nazi aeronautics research community developed a coping strategy to survive the immediate postwar period and to get things started again in what they hoped would be "better times."

This coping strategy was facilitated by the fact that the big science centers of aeronautics research in Germany had always had the legal status of *eingetragene Vereine* (e.V.), a special construction often adopted by private associations such as sport clubs or scientific societies in order to avoid being taxed for their activities. From a legal standpoint, the aeronautics research institutes in Nazi Germany had not been state-run big science centers (like the NACA – later NASA – in the United States), but private organizations with an extraordinarily high share of external funding. This enabled them to continue certain activities after 1945 without coming into conflict with Allied law.[4]

A second factor which facilitated the survival and subsequent reconstruction of the aeronautics research institutes was their close connections to a number of technical universities. Friedrich Seewald, for example, who was a leading member of the German Institute for Aeronautics Research (*Deutsche Versuchsanstalt für Luftfahrt e.V.*, DVL), had been a professor in Aachen since 1941. He held onto this position after 1945, managing to gather together a small staff of former DVL people in Aachen and even to reorganize small-scale aeronautics research at his university institute, which he used during the early 1950s to demonstrate to the public that it was necessary to reconstruct the big science centers. This coping strategy of using the technical universities as a location where aeronautics research could go into a "holding pattern" was very successful – Friedrich Seewald and his DVL were ready to get started when aeronautics research was permitted again in 1954.

But all these activities would have failed if the American occupying power had not tolerated and even promoted the maintenance and reconstruction of such aeronautics associations as the DVL and their research institutes. In the immediate postwar period, British and US agencies gave research contracts to various DVL institutes in southern Germany, which expired in late 1945 or mid-1946. And, by keeping plunderers out of the facilities, the US authorities ensured that documentation work on Nazi aeronautics and rocket research (which the Allied forces considered very valuable) could be conducted without interference. What was most important for further developments, however, was the certification given by the Office of the Military Government (US) in 1947 confirming "that the association 'Deutsche Versuchsanstalt für Luftfahrt' does not belong to the organizations which have been dissolved by the Allied Control Council," even though "every activity in the area of aeronautics research still remains forbidden" (quoted in German in Bruders 1962: 50, translation by the author).

To sum up, the coping strategy of an informal reinstitutionalization of extrauniversity aeronautics research in postwar Germany pursued by Friedrich

4 Cf. Trischler (1992).

Seewald and his colleagues was based on three elements, each indispensable for the subsequent success: the legality of (non-research-oriented) engagement in aeronautics subjects in private associations, the possibility to survive and to reorganize research at the technical universities and provide "holding patterns" for parts of the aeronautics community, and, finally, the tolerant and encouraging policy of the US Military Government. It was only due to these circumstances that the scientists even had a chance to resume their activities as quickly and vigorously as they did. But neither the circumstances nor the strategies and intentions of the key actors suffice to explain the remarkable success of their coping strategy. The decisive element in what turned out to be a success was the creation of an actor network consisting of aeronautics research associations and West German federal states *(Länder)*.

3.2 The Second Step: Networking with the *Länder* as a Coping Strategy in the Reconstruction Period

From the point of view of the *Länder*, the coping activities of the aeronautics researchers and, more importantly, the informal reinstitutionalization of the research institutes had created an attractive opportunity for politicians to link up with aeronautics research in order to exploit the political benefits of (what later came to be called) high-tech policy. Leo Brandt, Undersecretary in the Ministry of Economics of North Rhine-Westphalia, considered aeronautics to be a key technology and "an important pacemaker of modern technology," and therefore assigned it "a pivotal role in a modern economy" (Brandt 1954: 35, translation by the author). With this concept of technology policy, Brandt actually became the first research minister in West Germany, although this policy field had not yet been formally created. The aeronautics associations profited very strongly from his activities, which not only protected the steps toward a reconstruction of the research institutes before 1955, but also provided the funds urgently needed to build new facilities. On January 23, 1952, the parliament of North Rhine-Westphalia decided to fund the construction of aeronautics research institutes at the Mülheim airport and in Bad Godesberg. This bold measure not only promoted the recommencement of aeronautics research in West Germany, but also brought a completely new branch of research into the region. This decision had far-reaching consequences: The German Aerospace Research and Test Institute (*Deutsche Forschungs- und*

Versuchsanstalt für Luft- und Raumfahrt, DFVLR; now DLR), which came into being in 1969 as a merger of all aerospace research institutes of West Germany, is still situated in North Rhine-Westphalia (in Cologne). Without doubt this can be regarded as a political success, irrespective of the critical question of whether state funds should not better have been directed into other fields of research.

The creation of an actor network in which different actor groups with various interests and motives link up and build a coalition thus can be regarded as the crucial factor influencing the success or failure of a coping strategy. A major social innovation such as the reestablishment of aeronautics research in West Germany as well as the subsequent technical innovations could only be achieved by networking between politics (which regards aeronautics research as a resource for successful politics) and science (which regards technology policy as a resource for successful research).

As stated in the introduction of this chapter, networks are not only the foundation of success; they can also develop their own dynamics and thus become an independent source of new trouble. For the research institutes, the fact that aeronautics facilities were scattered around the country – a result of wartime and postwar necessities – became a source of trouble in the long run. Some facilities were located in Bavaria, the institutes having been moved there from Berlin when the bombings became too heavy toward the end of the war. After the war, the Bavarian authorities began to imitate North Rhine-Westphalia's new technology policy and supported the reconstruction of former DVL institutes at Oberpfaffenhofen, which is still an important site for German space activities. There are also institutes in Baden-Württemberg and Lower Saxony. The old facilities of Berlin-Adlershof, finally, are the most recent addition since reunification to this complex of regionally scattered institutes with sometimes divergent interests, which the central administration in Cologne was hardly able to "govern" during conflict-ridden phases. But this trouble did not arise until the 1960s – in the 1950s, the aeronautics research institutes were deeply satisfied that they could start up their work again with the help of the *Länder*, which provided the subsidies the federal government could not contribute for legal and legitimatory reasons.

So the aeronautics community had intentionally produced a suboptimal result which can be interpreted as a failure of a successful coping strategy. But the *Länder* also became victims of their own strategy when the financial burden of big science in the aeronautics field grew. In the first phase, when the institutes were to be reconstructed, the promotion of aeronautics was hardly expensive. But in 1956, at the latest, it became evident that the *Länder* would soon be reaching their limit financially: The DVL demanded a sum of 60 million DM (to be spread over five years) for the building of new research and test facilities, in addition to the regular annual budget. Only a few years after the successful reinstallation of aeronautics research under the responsibility of different *Länder* governments and the parallel creation of early predecessors of technology policy, the *Länder* got into trouble, which in the end can be regarded as a result of their attempts to seize opportunities as they arose.

Just as the social network was in danger of weakening or even collapsing, a new actor, the Federal Ministry of Transport (BMV), stepped in, took advantage of the opportunities that were opening up and finally reconstructed the network to such an extent that the *Länder* lost their formerly dominant position and were relegated to the periphery of the policy field. This case provides an example for the thesis that coping activities can have three different (sometimes interrelated) effects:

- They may help to overcome trouble,
- they may produce opportunities which another actor can take advantage of, resulting in an actor network, and finally
- they may produce new trouble for the actors concerned.

The Federal Minister of Transport from 1949 to 1966, Hans-Christoph Seebohm, who regarded both air and space transport as his domain, had been interested in promoting aeronautics research since the early 1950s. Two factors restricted his activities, however: Allied occupation laws, and West German constitutional law, which rendered the West German federal government relatively weak compared to the *Länder* governments in the fields of culture, education and science. One of the few niches left to Seebohm was the supervision and control of technical systems – a classical sphere of responsibility for every central government.[5] But his aim was to establish aeronautics research in institutes directly responsible to the federal government (*Ressortforschung*). From the point of view of the transport ministry, a commitment to aeronautics research was a suitable coping strategy to overcome the re-

5 Cf. Lundgreen et al. (1986); Stucke (1989).

strictions of the competencies of the federal authorities – with the long-term objective of state-controlled research.

Wielding a fair amount of power thanks to the funds at its disposal, the BMV very soon achieved a central position in the policy field, which it immediately used to call for a "reorganization of aeronautics research" (Seebohm 1953: 11) – i.e. coordinating and eventually merging together the aeronautics institutes, which were still small and regionally scattered at this point. The irrefutable argument in favor of such a reorganization was the foreseeably enormous costs of aeronautics research, especially if it was to continue to be conducted in six independent institutes – each of which would soon be requiring its own wind tunnel, a test stand and other expensive devices. For the aeronautics institutes, which had just overcome their previous problems, real trouble was now looming, since it became obvious that the (absolutely necessary) federal funding at the same time entailed political control of science and political intervention into the research process. But it must be noted that the transport ministry did not achieve its objectives, either, since it paid a high price in order to get the research institutes to accept the coordination of their work – maybe a higher price than it would have had to pay for the funding of uncoordinated research. Nevertheless, its policy can be regarded as an important contribution to a process which culminated in the establishment of the first federal research ministry in 1962.

3.3 The Third Step: Coping with Network Dynamics

The coping reactions of the aeronautics research institutes to these political initiatives were stimulated mainly by the prospect of losing their autonomy, which had been considerable during the period of *Länder* sponsorship and even during the Nazi era, the so-called 'golden age' of aeronautics research in Germany, when funds had been plentiful and political intervention had been either chaotic or – contrary to the common perception of the Nazi regime – even nonexistent. The institutes were now in an ambivalent situation: Their consolidation and expansion to an internationally competitive level could only be achieved with a strong partner (especially financially) in politics. Hence, the old network created with the *Länder* became less important, while a new network with federal authorities had to be created. This strategy was, in fact, extremely successful: The aeronautics research institutes enjoyed a period of rapid expansion which was followed by another developmental leap triggered by the European space program in the early 1960s, so that in 1965 the DVL had reached the size (in terms of staff) it had once had in 1935 – undoubtedly a great success. But the risks of that networking strategy were clear. The principle of independence of the different research institutes was constantly threatened, until in the mid-1960s their position became so weak that resistance became useless; in 1969 the fusion of the aeronautics institutes could finally take place. The German Aerospace Research and Test Institute (DFVLR; now DLR), founded as a central organization, was much easier for the central political authorities to control than the six independent institutes, each of which had been sponsored and protected by "its" respective *Länder* government.

The period from 1953 (the first announcement of an interventionist policy by Seebohm) to 1969 (the fusion of the aeronautics research institutes) can be viewed as a continuing defensive battle in which different coping strategies – some active, some reactive – were developed and carried out that led, however, to a result the research institutes judged negatively. The main obstacle making failure almost inevitable was the fact that the aeronautics research institutes were repeatedly confronted with new trouble before they had managed to solve the old. This may explain why they did not react to the political disturbances in the same way an independent observer – living, say, in the 1990s – might suggest they should have, but adopted strategies which, at least viewed with the benefit of hindsight, had to fail.

The first step in this struggle with politics was the creation of an independent representation of the interests of extrauniversity aeronautics research. The Association for Aeronautical Sciences (*Wissenschaftliche Gesellschaft für Luftfahrt*, WGL), founded in 1952, played an important role in reorganizing the aeronautics community – by publicly promoting a new peaceful image of aeronautics research in order to justify their petitions for political support – and in reintegrating German aeronautics research into the international community. The most important function of the WGL was to be a competent partner for the political actor, the Federal Minister of Transport, who had indicated as early as 1951 that he had at his disposal a small amount of money which could be spent on aeronautics subjects and that he needed scientific advice in setting priorities for the distribution of these funds. In 1953, the Committee on Aeronautics Research (*Ausschuß für Luftfahrtforschung*, AfL) was founded as a subsection of the WGL, serving simultaneously, however, as an advisory board for the transport ministry. This was the first case of institutionalized policy advising in the R&D sector in West Germany, which proved to be useful for both partners concerned. For the transport ministry, the institutionalization of a hybrid organization between politics and science was the first step toward a central coordination and control of R&D (here in the field of aeronautics), whereas the aeronautics community, represented by the WGL, succeeded in obtaining quasi-monopolistic access to the political key actor in its respective field. Every research proposal, be it from WGL members or not, now had to pass through the hands of the WGL president, who – not surprisingly – had also become chairman of the advisory committee AfL.

Nevertheless, the WGL could not serve as a strong representative of the interests of extrauniversity aeronautics research, since this umbrella organization of the West German aeronautics community encompassed heterogeneous groups from different branches of aeronautical science and industry. In addition, the WGL had to fulfill bargaining functions in the interplay of politics and science which was just beginning to take place. It became especially obvious that the aeronautics research institutes were in need of a representative organization of their own when the transport ministry began to call more insistently for a coordination of research planning and made its willingness to fund the expansion of the research institutes dependent on their willingness to cooperate. In March 1955, three out of the six institutes therefore founded the Community of Interests of the Aeronautics Research Institutes *(Interessengemeinschaft der Luftfahrtforschungsanstalten)* – the weakest form of cooperation they could have chosen. The reason for this half-hearted move was each institute's separate fear of losing its autonomy. Once again, the success of a previous coping strategy – leading to a network between aeronautics research and the transport ministry – became the source of still greater trouble, for the BMV's objective was clear: the fusion of all research institutes, the creation of one big science center, and the central political control of aeronautics research. Obviously, the establishment of this weak Community of Interests was the wrong reaction to this trouble.

The aeronautics research institutes continued the coping game, the rationale of which was to satisfy the transport ministry by (at least) symbolically uniting the institutes and at the same time letting each of the institutes be able to keep its traditional autonomy. This was difficult to achieve because the smaller institutes suspected that their larger fellow institutes, especially the big DVL, might use this game to swallow them. After long deliberations, the

High Council of Aeronautics Research Institutes *(Präsidialrat der Luftfahrtforschungsanstalten)*, made up of representatives of all six institutes, was founded in October 1956. They now had a stronger representation of interests, but the political actors were still dissatisfied because the unclear legal construction of the *Präsidialrat* made it impossible to use this organization either as an instrument for the distribution of federal funds (as the legal construction of the *eingetragener Verein* would have allowed) or as an instrument for the political control of science. Thus, the political actors insisted that the High Council be reconstituted as a "body corporate" (DGF 1965: 133). The game continued during the following years, but the position of the research institutes became increasingly weaker because they were in a tight financial spot. In April 1959, they founded the German Association for Aeronautics (*Deutsche Gesellschaft für Flugwissenschaften e.V.*, DGF), which all extrauniversity research institutes in the field of aeronautics joined. The DGF served as an umbrella organization which distributed the federal funds among the members and as the bargaining partner toward politics. The statute of the DGF shows that this body was constructed as an instrument of political control of science.

The aeronautics research institutes felt they were the losers of this game; their coping strategy had obviously failed. Their objective during the 1950s had been more funding for aeronautics research; now they received more funds than they had ever dreamed of, but only in combination with political intervention and control of their research work. But also from the point of view of politics, the result of this game was suboptimal, since the federal actor (first the transport ministry, later the science ministry) paid a high price for the unification of the research institutes. The costs of research soared, but now politics was obliged to shoulder its new responsibilities, which was not easy, since the large DGF, later the DFVLR, and, finally, the DLR proved to be difficult to control.

3.4 The Fourth Step: Coping with the Space Age

The outcome of the battle for autonomy and control of extrauniversity aeronautics research was not predictable at the moment when the real trouble began, which finally kicked the transport ministry out of the game, brought new actors in, and led to an (unintended and unwanted) accelerated expansion of the research institutes. In 1960, the first initiatives were launched by Great

Britain and France to set up a European space organization. The federal government of West Germany began to deal with this new subject at the end of 1960 and, more intensively, in early 1961. For German federal politics, space flight was an accidental opportunity which improved its ability to cope with the trouble of its restricted sovereignty in comparison with the neighboring European countries and, domestically, with the trouble of its relatively weak position in comparison with the *Länder*. This coping strategy, however, could only be successful if a national base for the planned participation in European space flight was at hand. Now, the early initiatives of the transport ministry and the aeronautics and rocket societies aimed at reinstitutionalizing aeronautics research finally paid off. But what might have been considered the greatest success in the history of West German aeronautics research was regarded as the most threatening trouble the aeronautics community had been confronted with since 1945, because the federal government's new initiatives to promote space research – in dimensions inconceivable only a few years earlier – inevitably entailed its calling for a fundamental shift of research priorities from aeronautics to space flight and from basic to applied research. Besides, it was foreseeable that the trend toward a political control of science would intensify if research priorities now had to be negotiated not only between different national research institutes, but also between various European states with disparate (political, scientific, economic, partly also military) interests in space flight. The game the aeronautics community had so enthusiastically initiated was about to slip out of its control due to the powerful autodynamics that had developed. At this point, the community would have preferred to stop the "film" rather than to continue playing its role, since it no longer had any control over the script.

The aeronautics community developed and performed a variety of coping strategies, most of them fruitless. Three different types of coping efforts can be distinguished:[6]

a) *Defensive-reactive coping,* which tried to maintain the status quo, complaining that the foreseeable predominance of space flight over aeronautics was unfair to the aeronautics community. In this futile battle, the advocates of aeronautics frequently argued that aeronautics is the real basis of space flight, and that, consequently, space flight can only be successful if a solid

6 Cf. the introduction to this volume by Schimank and Stucke.

foundation of aeronautics research exists. But in politics there was no one who was willing to adopt this argument. Space was on the agenda!

b) A second coping strategy which was launched when the institutes recovered from the initial shock over the fatal threat posed by "space" can be called *half-hearted offensive coping*. The aeronautics research institutes tried to present themselves to the public and politics as the only competent partner with adequate experience in the field now ready to step into space research. Relabelling the research institutes and some of the (formerly pure aeronautical) research fields was part of this effort. This strategy was partially successful: The establishment of new space research institutes and a concomitant phasing-out of support for aeronautics in favor of new competitors could be avoided. It failed, however, in another way: The continuation of former (aeronautical) activities under a new label proved to be an insufficient tactic when the federal government decided in June 1961 to participate in European space flight. By then, at the latest, the scientists were forced to actually make the switch from aeronautical to space research; just talking about space no longer sufficed.

c) An actual reorientation toward space research (as opposed to the tactical one described above) is at the heart of the third response, which can be labelled as an *offensive-preventive coping strategy*. The DGF was too clumsy and its members too much at odds with one another to adopt this strategy. In a sort of double game, the largest single institute – the DVL – supported the activities of the DGF, while at the same time trying a more offensive strategy based on the assumption that only the orientation toward specific space projects and not the desire to conduct general basic research would provide the research institutes with legitimacy and, hence, public funds. The DVL thus proposed to build a German satellite in cooperation with the (emerging) space industry; in November 1962, it presented a detailed project proposal written by the DVL, the aerospace company Bölkow and the Meteorological Institute of the University of Cologne. This cooperation with the industry can be regarded as a new coping strategy with the intention of reducing the influence of politics on research; at the same time, however, it created a new risk of subordinating research to industrial priorities. Here, a new network emerged that supplied its participants with special advantages over rivals in the respective fields. Bölkow (later MBB; now DASA) became the leading aerospace company in West Germany, and the DVL advanced to become the undisputed leader in extrauniversity aeronautics research. The DGF tried to pursue this strategy, too: Together with the Association of German Aeronautical Industry (*Bundesverband der Deutschen Luftfahrtindustrie*, BDLI), the spokesman of the aviation companies, it had established the Committee on Space Technology (*Kommission für Raumfahrttechnik*, KfR) in August 1961. This lobby organization's purpose was to influence the initial political decisions on the West German and European space program. But it was not until July 1962 that a first, very preliminary program proposal could be presented by the KfR. This proposal sank into oblivion very rapidly only a few months later when the Bölkow-DVL satellite appeared; the threat of a complete Europeanization of the West German space program, which would probably mean increasing amounts of German marks flowing into French or British research institutes and lower expenditures for national programs on aeronautics research, produced a new kind of trouble the KfR was unable to cope with. As has been shown in more detail elsewhere, the Bölkow-DVL satellite was an adequate means to cope with this trouble and to redirect the federal funds into building up West German aerospace companies and research institutes.[7]

3.5 The Final Step: The Establishment of the First West German Research Ministry

In the 1950s, the field of aeronautical research was a kind of testing ground for essential instruments of governmental control of research. Furthermore, the organizational prerequisites for translating programmatic political goals into research were created when the big science center, DGF, was established. This fulfilled two of the conditions required for space flight to be classified as big science; what was still lacking was the industrial underpinning. It was the Minister of Defense at that time, Franz Josef Strauß, who was the main driving force behind the development of the aerospace industry and, hence, the establishment of the paradigm of an industrial policy that was not market-directed – a story which cannot be presented here in detail. Finally, the French and British initiatives to launch a European space program triggered

7 cf. Weyer (1993a: 280-315).

the formal establishment of space policy in West Germany, which then became one of the responsibilities of the new Ministry for Scientific Research (*Bundesministerium für wissenschaftliche Forschung*, BMwF), founded in December 1962.[8] Since the mid-1960s, the policy field "space flight" has been governed by a triad of research ministry, space industry and big science centers which has even influenced the style of research policy in other fields. Although this institutional structure had been established by goal-oriented behavior of the participating actors, the actual shape it eventually took had been intended by no one. By the end of the 1960s, this constellation had acquired a dynamic of its own, increasingly becoming a constraint for its participants and influencing their freedom of action. Thus, the social network itself became a source of trouble.

4 Conclusion

4.1 Winners and Losers in the History of West German Space Policy

The history of West German space policy in the 1950s and 1960s reveals that hardly any of the initial actors who had contributed significantly to the creation of a particular aspect of the network were able to profit from their success. Coping efforts employed by the respective actors usually caused new trouble, resulting from the autodynamics of actor networks, but at the same time created new opportunities, which mostly could only be exploited by other actors who proceeded to play a major role in the next part of the sequence. Thus, several of the actors named above disappeared from the network completely or were forced into the periphery (hobby rocket builders, the *Länder*, the transport ministry). In other cases, there was a strange mixture of success and failure (the aeronautics research institutes, the aerospace industry, the defense ministry). These examples of successful failure or unintended success confirm the hypothesis formulated in the introduction: The success of social strategies results from the exploitation of situational opportunities as well as from the networking of various actors. At the same time, these strategies give

8 Cf. Krige (1993), Stucke (1993b).

rise to an autodynamics of social networks whose consequences may well conflict with the intentions of the participants and which, as a result, are often judged negatively by the initial actors. Although the networks are created strategically, unintended structural effects issue from them which can result in the failure of the manifest strategies of the founders of the network.

One reason why the participating actors continue to play the game despite its having unintended consequences is that, having begun it, they cannot give it up without abandoning themselves. The special advantages they have gained over their various opponents depend on the – continued – existence of the network. This means that maintenance of the troublesome network can become an independent rationale for playing the game.

4.2 Trouble as a Permanent Condition

Summing up, the history of extrauniversity aeronautics research in the 1950s can be reconstructed as a brilliant success story, but the winner of this game had lost so much of its former identity that it felt like a loser. In 1950, there were six autonomous aeronautics associations lacking in resources, institutes and funding but, at the same time, free of political control. By the mid-1960s, they had turned into one unified, well-equipped, politically directed, quasi state-run agency for research in a field that was dominated by the requirements of European space technology instead of the inner logic of basic aeronautical research.

Despite twenty years of nonstop coping efforts, trouble never decreased; on the contrary, the coping strategy of networking, which can be detected at every stage of the development, always produced new, usually greater trouble. External trouble such as the emergence of a European space program and internal trouble such as the unintended effects of networking (between the transport ministry and aeronautics research, for example) sometimes reinforced each other, as has been shown in detail in the case study. Trouble does not seem to be an extraordinary state, but the normal business of social actors who act strategically and are, at the same time, the focus of other actors' strategic activities. The interactive character of social action proves to be a major source of trouble which in general allows only two meta coping strategies: exiting the policy field or continuing the game, which usually requires an actor to change its own identity and adapt its aspiration level to the changing context.[9]

4.3 Coping Activities as a Source of Trouble

What theoretical conclusions can be drawn from the case presented above?

a) To begin with, we can conceptualize social organizations such as ministries, science associations, and research institutes as social actors which pursue organizational interests and develop strategies to achieve their objectives. Whether these strategies are rational or not, measured by a quasi-objective criterion of social rationality, does not matter in this context; the strategies must, at the very moment of their conception, be considered (by the respective actor) to be adequate, i.e. the best alternative available for accomplishing the aims that have been set. Which alternative is the best depends largely on the options available within the social context, which in turn are products of actions of other co-players. Thus, the foundations of actors' decision making vary in the course of social interaction.
b) The chances of actors' strategies being realized and, ultimately, successful can be related to their ability to establish social networks, which must be regarded as a very important base of social innovations. The opportunities to profit from network dynamics, however, are inevitably connected with its risks, which largely result from the fact, that – as frequently mentioned above – networks can develop autodynamics and thus produce social constraints which run counter to the actors' initial intentions.
c) Assuming these conclusions are accurate, we can distinguish between two kinds of trouble and two types of coping with trouble. Trouble may be produced by *external forces* outside the respective actor's sphere of influence, which are usually a surprise and can hardly be anticipated. This applies for example to the initiatives to launch a European space program, which were triggered by Sputnik and other events that could not have been anticipated in the mid-1950s. On the other hand, trouble may be the *internal product* of network dynamics, as was shown, for example, in the

9 Cf. the introduction to this volume by Schimank and Stucke.

analysis of the interaction of the aeronautics community and the transport ministry. Here trouble is not caused by unforeseeable external forces, but is related to an actor's own (risky) decision to join a network and to profit from its advantages.

d) In addition to this distinction between "internal" and "external" (sources of) trouble, we can also distinguish between two types of coping strategies: The first type can be called *defensive-reactive coping*, a behavior (not really a strategy!) that only activates organizational energies when trouble actually occurs (i.e. when the focal actor has perceived certain events and interpreted them as being trouble). The actions of the DGF, the umbrella organization of the extrauniversity aeronautics research institutes in the late 1950s, fall into this category. The second type can be called *offensive-preventive coping*, a strategy that anticipates that somewhere in the future trouble might happen and that it would be advantageous now, in advance, to equip oneself with (financial, legitimatory and other) resources in order to deal with it. The strategy of the DVL, the largest aeronautics research institute, to risk going out on a limb and cooperating on its own with the aerospace industry can be regarded as an example of this type.

The two points I want to emphasize here are that this very strategy of offensive-preventive coping is one main source of trouble, and that each actor's perception that trouble resulting from other actors' offensive measures will occur in the future in turn accelerates this process. The mutual assumption of offensive activities and the tendency to grasp at every opportunity that opens up (even if it cannot be exploited now, but only – presumably – in the future) seems to be an important trigger of the autodynamics of the social process. In advanced industrial societies in which each actor stands to gain from his or her own (risky) decisions, trouble seems to be more likely than stationary equilibrium, which would imply that everyone is satisfied (and which, at the same time, means that there is no room for maneuver to improve one's own position). Coping with trouble and, in so doing, producing new trouble – this seems to be an endless evolutionary game with, at best, short periods of calm.

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A Theoretical Examination of the Cases: Why Coping Is Often Difficult and Defective

Uwe Schimank and Andreas Stucke

If there is one thing all the preceding case studies make unequivocally clear, it is that effective coping with trouble is not at all easy. Most of the cases are not success stories. Even those cases where the research actors could finally get rid of their trouble to a considerable extent, or were at least able to significantly reduce the damage done to their research conditions, show nevertheless that coping requires great effort – see the molecular biologists in the case presented by Hasse and Gill, the laboratory endangered by disassociation from the CNRS in the case presented by Musselin and Vilkas, the first laboratory in Wolf's study of the East German Academy of Sciences (AdW), or many of the professors in Schimank's case. Often, good luck is also a vital element of successful coping, as, for instance, the nuclear physicists in the Berlin case presented by Gläser et al. illustrate. And not only Mayntz's case of the AdW reminds us of the very real possibility of fruitless coping. In all cases there are examples of actors who simply had to suffer their trouble – the third institute in Wolf's case, for instance, or the biomedical researchers in Braun's case who could no longer acquire the resources they needed. Weyer's case of the German aerospace research institutes, finally, points out that even successful coping may often provide merely temporary relief, which may already contain the seeds of future trouble.

Of course, as Krauss's case of French agricultural research demonstrates, sometimes trouble is only staged. Or, as in the case of the research group from the Parisian laboratory of the CNRS presented by Musselin and Vilkas, what seems to be trouble may be an initial misperception of a situation that turns out, in fact, to be a good opportunity. But most often the trouble is real, and coping with it is difficult. Admittedly, this is a rather trivial finding. But as such it offers an uncontroversial starting point for further analysis. As we stated already in the introductory chapter, we are not interested in investigating the manifold potential causes of trouble in this analysis. Here, a reference to the case studies will have to suffice. We take trouble, or at least the real possibility of trouble, as given and ask, what happens then?

Three principal reactions to trouble are possible: trying to *prevent* trouble which has not happened yet; trying to *cope* with already existing trouble if one perceives an opportunity to reduce the damage done; or, if neither of these alternatives seems feasible, helpless *suffering* of trouble, perhaps made bearable by waiting for better times. Whereas the first two reactions are usually difficult, demanding appropriate skills, social influence, knowledge, and resources, the third is easy because it is a passive reaction. Helpless suffering can be equated with failure if the extraordinary – and improbable – stroke of good luck does not come along. The success of waiting depends on other actors, who cannot necessarily be expected to behave as one would hope. The nuclear researchers in Berlin described by Gläser et al. had, indeed, good reason to hope that the political constellation in the Berlin government might change again to their advantage, or that the federal government might press the Berlin government to issue the operating license for the research reactor. But by no means could the researchers be sure of either development. In contrast, prevention as well as coping imply that actors facing trouble take their fate into their own hands. Since our case studies provide examples of all three kinds of reactions, we can put coping into perspective by comparing its difficulties with those of the other two reactions.

We would like to focus our analysis on one particular group of factors which very often make coping difficult: the factors associated with the circumstance that coping takes place in a constellation consisting of a plurality of research actors facing trouble and the political actors who caused that trouble. As we also asserted in the outline, such a constellation has three dimensions: the horizontal juxtaposition of different research actors on the same level of action, the vertical arrangement of different levels of research actors, and the relations of the research actors to the relevant political actors. For example, in Stucke's case the institutes constituting a big science center are actors facing trouble on one level of action, while the big science center itself faces trouble on a different level; both the institutes and the center each have specific relations to the Federal Ministry for Research and Technology on the political level. What we primarily want to know is how this *horizontal and vertical structure of the constellations of research actors as well as their* *relations to political actors* determine the set of coping alternatives. Which alternatives are possible at all in a given constellation, and how difficult are they to achieve?

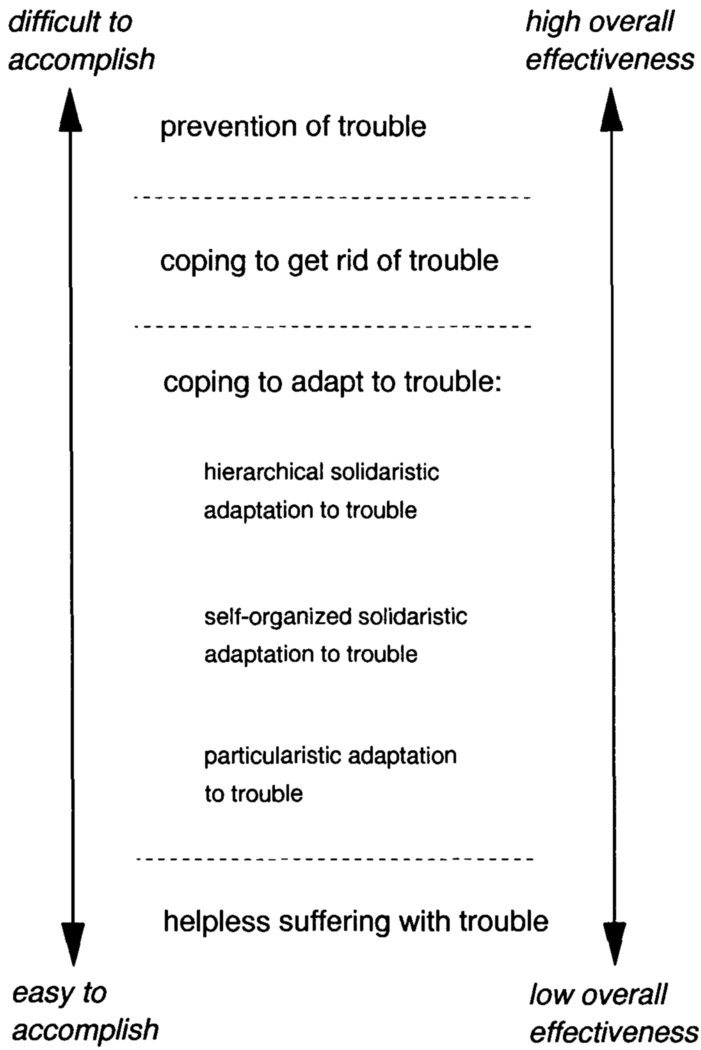
In addition, we will explore the relative effectiveness of different kinds of coping. We have chosen cases in which one particular type of trouble is shared by a plurality of research actors[1] in order to emphasize that our point of reference for assessing effectiveness is not the isolated actor – at whatever level of action – affected by trouble, but the group of actors jointly facing a particular type of trouble, i.e. the *population in trouble*. Consequently, the overall effectiveness of coping refers to this population's ability to maintain its research conditions in spite of political disturbances.[2] It is certainly not totally unconvincing to suspect, in a first rough guess based on the cases, that the easier a particular reaction to trouble is to accomplish, the lower its overall effectiveness will be. Let us take, for the moment, just the two extremes: Helpless suffering is very easy to accomplish, but has a rather low overall effectiveness; in contrast, prevention of trouble has a high overall effectiveness, but seems to be very difficult to accomplish. The various types of coping now to be considered (see Figure 1) are located between these two poles (which, as we indicated in the introductory chapter, are *not* coping). We will elaborate on these coping strategies, which range from getting rid of trouble to a variety of ways of adapting to it.

As we stated in the introductory chapter, coping with trouble is an analytical perspective which belongs to the political sociology of science. Therefore, we can benefit from many concepts and models already developed within political sociology and political science. Our reflections here will be based upon a number of general theories about collective and corporate political action, and upon elements from sociological theories of social differentiation and organization theory. Combining analytical tools from these theories with

1 This plurality may be the organizational subunits or the individual researchers of only one research institute.

2 Applied here in a very strict sense, effectiveness must be distinguished from an improvement in a particular research actor's situation, the research system's situation or the situation of the society in general. It may certainly be that what is good for the population in trouble is bad for some of its members, for the research system, or for society. As we stated in the introductory chapter, our concept of trouble is strictly related to the point of view of the research actors affected; consequently, the same applies to the effectiveness of coping.

Figure 1: Possible Reactions to Trouble



insights from our cases, we will construct a middle-range theory about coping and its effects. Since there is no general theory of coping with trouble in political sociology or political science, we cannot simply specify such a theory for our topic of the coping of research actors. Instead, we have to apply and link elements from the existing toolboxes of the theoretical perspectives to our empirical subject. On the one hand, there is no alternative to this way of inductive theorizing "from below." We have to extract our theoretical insights from the cases because, with no comprehensive general theory of coping with trouble at hand, theorizing deductively "from above" is impossible. But a pure theoretical incrementalism, on the other hand, would not yield more than a fragmented, incoherent store of propositions. Therefore, before we examine the cases, we will lay a foundation by asking more generally what types of political influence research actors can exert. How research actors try to cope obviously depends critically on the type and degree of influence they have on those political actors who produce their trouble. Thus, a framework for our examination of the cases is made up of some peculiar features of the research system which crucially determine the possibilities of coping with trouble.

In Section 1, we will look at the political influence of research actors, showing that while their collective influence is rather weak, many have individual opportunities to articulate their interests and, sometimes, to achieve their goals. This finding explains our overall impression from the case studies that coping is most often a particularistic adaptation to trouble. In contrast, the occasions when research actors can successfully prevent trouble or get rid of it are quite rare. In Section 2, we will concentrate on the specific factors which make it so difficult to prevent anticipated trouble or to eliminate actual trouble. In addition, we will describe the exceptional circumstances under which both reactions may be successful. In Section 3, we will turn to those coping reactions which, if successful, only bring about an adaptation to trouble. We will focus on how much more difficult it is to achieve solidaristic adaptation to trouble – be it hierarchically imposed or self-organized – than particularistic adaptation and explain why the latter exhibits a low overall effectiveness.

Our goal, therefore, is to draw some general conclusions about coping with trouble as a constellation phenomenon. Reviewing the case studies in this volume and, occasionally, cases cited elsewhere, we will try to identify *patterns of coping*.[3] This theoretical strategy presupposes that there are no simple two-factor causal relations between coping activities and *single* structural or dynamic features of the relevant actor constellations. The diversity and complexity of the case studies suggest that it makes no sense analytically to study the effects of single determinants – such as the degree of self-organization of troubled actors – on coping activities. No elegant laws stating "if x, then y" or "the more x, the more y" can be formulated. Consequently, we will not arrive at easy recipes for successful coping, either. For us as social scientists, though, it does not suffice to assemble an assortment of cases without drawing at least some tentative conclusions that might apply to future cases. Two reductions of historical complexity must be achieved by generalizations in the social sciences: The multitude of concrete past events as well as the uncertainty of concrete future events must be reduced to a smaller number of well-conceived abstract patterns. Even if these patterns do not exhibit the simple structure of two-factor causal relations, the analytical reduction of complexity contributes to a better understanding of social reality.

1 The Weak Political Influence of Research Actors

Theories of societal differentiation point out the curious fact that the research system is distinguished from other societal subsystems by being primarily its own public (Stichweh 1988). Whereas a doctor, a teacher or a company executive – the central actors of the health care, the educational and the economic systems respectively – works for his patients, his pupils or his customers, a researcher works for other researchers, who read his books and articles and then quote him in their own publications, thereby providing him with a scientific reputation. This feature of scientific research, strange when compared to other societal subsystems, derives from the character of its product. Scientific research produces bits of true knowledge. But truth, as the guiding principle of research work, is *orientationally closed* to the concerns of non-researchers. The scientific value of a "piece" of research work is never depen-

3 Rather than attempting to provide an all-inclusive catalogue of the many valuable insights into coping with trouble offered in the case studies, we will selectively choose those we need for our present argument.

dent upon its societal usefulness, whereas the medical value of a new treatment for a particular illness or the educational value of a new pedagogical principle will naturally be tied to its efficacy in healing patients or educating pupils. Health, or education, as the guiding principle of a doctor, or a teacher, relates to the concerns of patients, or pupils; profit-making, as the guiding principle of a corporate executive, relates to the concerns of customers. The performance of the systems of health care, education or corporate business would be meaningless without counterparts outside. Medical, educational or economic action is intrinsically oriented towards reaching beyond the boundaries of these subsystems. Research action, on the other hand, is intrinsically enclosed within the research system. Other concerns remain extrinsic to this logic of action, even if occasionally particular researchers or, on an institutionalized basis, whole disciplines such as the medical or engineering sciences bridge the gap to other societal subsystems.

As long as scientific research was an inexpensive, small-scale affair, usually conducted by wealthy amateurs or promoted by even wealthier patrons, the closed circles of researchers working on the same topics were rather self-sufficient. But as research grew more and more expensive and developed into a profession during the last century, the research system became strongly dependent on its societal environment for financial resources. Research facilities became much more complex and costly, and researchers had to earn a living from their research. With this resource dependency, the major potential for trouble came into being. Although specific causes of trouble do not concern us here, we nevertheless have to inspect briefly the general vulnerability of research actors to political trouble. This is necessary if we are to understand the weak political influence of research actors.

Basic research which is not oriented toward any potential applications has a particularly difficult time legitimizing its resource needs. Why should anybody not involved in the self-sufficient communication circles of specialized researchers want to finance such a hobby? Although a work of art is just as useless as the results of this kind of basic research, the artist's patron can at least enjoy the intrinsic aesthetic qualities of the works produced with the help of his money. Thus, an artist still works for a public which does not only consist of other experts like himself. Despite its uselessness, there is a societal demand for his kind of work because it fulfills certain needs of at least some members of society who are not artists themselves. In contrast, those who do pure basic research without any prospects of applications can offer nothing in return for the growing amount of money they demand from society.

On the surface, this legitimation problem does not exist for the researchers involved in applied research, or in basic research promising applications at some point in the future. But a closer look reveals that they have this problem, too – though it is admittedly less critical for them. In periods of prosperity, it is not that difficult for these research actors to find other societal actors – especially in government and industry – whose interest in particular applications of their research activities is so strong that they will be willing to finance the research. But whenever these other actors' funds become scarce, promotion of research tends to be one of the first budget items to be cut back. This tendency to reduce the support for research activities grows if it takes longer and longer for applications to materialize and, especially, if it becomes doubtful that applications will emerge at all. As funds become scarce, the time horizon of actors shrinks, and their risk-aversion grows. Suddenly, the promotion of research activities – even of applied research – can appear to be a investment in a luxury good that will not pay off for decades, and that is now unaffordable because of the many serious, pressing problems requiring immediate attention.

Frequently, research actors have to face not only resource cutbacks, but also strong demands to make themselves more immediately useful. Thus, for the money they still do allot to research, the financial sponsors insist upon a quicker and better "return" on their investment. Not content to confine themselves to general demands, the sponsors try to intervene directly in specific research decisions – especially when it comes to choosing research topics. Under these circumstances, the scientific interests of researchers may be confronted with divergent extrascientific interests.

Thus, trouble is inherent to the peculiar character of the product of scientific research. Basic research without potential applications is vulnerable to resource trouble. In a weaker sense, this also holds true for applied research which, in addition, often has to face the trouble of being instrumentalized for extrascientific interests. The smaller the circle of actual or potential sponsors is for a particular research project, the bigger the researcher's trouble is. To take the extreme case, if there is only one sponsor, the researcher has no exit option at all and is thus completely dependent. The *state-financed* research institutes upon which our cases have focussed get a substantial, often predominant share of their funding as institutional financing from government. The higher the share of institutional financing a research institute has, the more dependent it is upon the sponsoring government agency for resources.[4] Project grants are the institute's other main source of funding. If their share of an institute's financing is high, the institute's dependency on the government agency will be more relaxed; its susceptibility to the insecurity of the grant market, however, will increase correspondingly. Whether this is preferable for a research institute depends on the degree of competition within this market and on the institute's competitive strength. As a sponsor's allocative flexibility is higher with project grants than with institutional financing, the former can also be better used for a short-term instrumentalization of researchers. Thus, financing by means of project grants always implies – in addition to the basic insecurity of the grant market – the trouble of instrumentalization. Project grants like those provided by the German Research Foundation, which are not linked to a specification of research topics by the grant-giver, are the exception. A sponsor providing institutional financing usually has the prerogative, however, to participate in decisions about the institutional set-up of a research institute – i.e. its basic organizational structure, its research program, and the recruitment of its directors. At the very least, the sponsor has the right to veto an institute's decisions on such matters; at the most, the sponsor bears the sole responsibility for these decisions. The trouble of institutional restructuring, therefore, is another potential consequence of a research institute's institutional financing by government.

The researchers' narrow-mindedness, resulting from the differentiation of the research system within modern society, leads to their being indifferent not only about the societal usefulness of scientific truths, but also about the risks scientific research may inadvertently pose to society at large. Truth is orientationally closed to the risks of its production or application. Therefore, the political regulation of types of research which may be dangerous to society is necessary. While legal instruments are the principal means used to

4 A special constellation exists if a research institute's institutional financing comes from more than one government agency. In Germany, this is the case for most extrauniversity state-financed research institutes which are jointly financed by the federal government and the states. This situation is a mixed blessing for the institutes (Hohn/ Schimank 1990). While joint financing may help an institute to defend its autonomy against instrumentalization by letting it play the government agencies off against one another when they disagree, it often results in a situation where the stingiest government agency determines how much the institute will receive from all the others.

control these negative externalities of research, the resource dependency of state-financed research institutes upon government agencies also gives government considerable leverage. Because financial resources might be withdrawn if an institute breaks the law, the regulations serve to reinforce the research institutes' obedience doubly: The loss of resources may have even more massive and long-lasting repercussions than legal sanctions. Government agencies can even use financial incentives and disincentives to discourage or eliminate research behavior which is not (yet) legally forbidden or to foster the development of research behavior which is not (yet) legally prescribed. Thus, the trouble originating from political regulation of research, an inevitable consequence of the differentiation of the research system, affects state-financed research institutes most strongly.

In sum, from the point of view of theories of societal differentiation, a peculiar characteristic of the product of scientific research – the orientational closure of truth to all aspects of societal usefulness or riskiness – causes its high vulnerability to the different kinds of trouble political actors can generate. If this vulnerability was counterbalanced by a respectively high degree of *political influence of research actors*, these actors would not have a serious problem. Research actors would be able to defend themselves against political actors, and could prevent trouble most of the time, or nip it in the bud. But if research actors' political influence is actually low, their possibilities of coping are greatly restricted. Therefore, we will now examine how research actors are able to collectively influence political actors.

1.1 The Low Obstructive Capacity of Research Actors

A general finding from political science which we can start with is that the degree of political influence social actors have varies strongly with their ability to obstruct processes of societal reproduction, either by withholding services perceived as indispensable by others or by hindering other actors from performing such services (Offe 1969). Research actors' *obstructive capacity*, however, is very low. At first sight, one might imagine that research actors are highly influential because they provide other societal subsystems with a growing stock of knowledge which is functionally required by those systems to maintain and improve the systemic level of performance and, thereby, to contribute to the reproduction of society. To withhold or to threaten to withhold this supply of scientific knowledge would seem to be a powerful weapon research actors could wield in political conflicts. As has become especially apparent since the technocracy debates in the 1960s, the scientification of modern society is indeed very high, and it is still growing (Schelsky 1961; Bell 1973; Kreibich 1986; Böhme/ Stehr 1987; Stehr/ Ericson 1992). This fact is also appreciated by societal actors in general and political actors in particular. Nevertheless, the latter do not refrain from causing trouble for research actors again and again. The estimation of the high societal usefulness of research manifests itself, interestingly enough, in the trouble arising out of political attempts to redirect research according to extrascientific criteria. Somehow, modern society's irrefutable dependence upon scientific research does not supply research actors with the type and degree of influence which organization theory, for instance, demonstrates for intraorganizational groups controlling critical uncertainties of organizational performance (Pennings et al. 1969; Hickson et al. 1971; Hinings et al. 1974). Why are research actors not as influential as, say, the repairmen in a production plant upon whom everybody else depends because they alone can ensure or restore the smooth working of the assembly lines (Crozier 1963)?

The more alternative suppliers of these indispensable services there are, the less influence an actor will be able to mobilize by offering his special services (Emerson 1962). The particular scientific knowledge a societal actor – a firm, for instance – needs can very often be provided by more than one researcher or research institute. It is only in some very specialized fields of research that one research actor may temporarily have a monopoly on the supply of knowledge. Political actors often deliberately promote the emergence of alternative suppliers of certain scientific knowledge in order to reduce potential dependencies of customers. Of course, even a plurality of suppliers can organize themselves to prevent their being subjected to a "divide and conquer" strategy by the customers. One of the reasons this happens rarely among research actors – others will become clear later – is that even a successful solidaristic organization of research actors would not help very much. The deeper cause of their low obstructive capacity lies not in their social fragmentation, but in the peculiar character of scientific truths.

It is true that in an increasingly science-based society, a growing number of societal actors perceive being provided with scientific knowledge to be a critical functional prerequisite for attaining their goals. But this remains an estimation of the long-term relationship between the research system and actors from other societal subsystems. In the short run, almost anybody can do without new scientific knowledge. Many years can elapse before actors in other societal subsystems notice that they have been receiving a suboptimal supply of useful research results – by then, though, it is usually too late. A general strike by all researchers would not leave a trace for a long time, whereas a strike by doctors, teachers, or garbage collectors would hurt many people immediately. Most societal actors – especially the political actors – are well aware of the immense long-term damage research actors can do to society by not producing knowledge. A threat in this direction, however, endows the researchers with hardly any political influence; those involved in basic research with distant – and maybe even doubtful – potential applications have the least influence of all. The short time framework within which societal actors pressed by an overload of urgent demands and interests almost always act obstructs their view of the future. To put it in drastic terms, if caring for one's long-term survival critically reduces one's chances of short-term survival, one cannot but act according to the maxim "First things first!" and hope for good luck in the future. This applies even more to political actors than to other actors. Thus, although research actors do provide important services to many other societal actors, this gives research actors no significant influence on political actors.

On the contrary, research actors are heavily dependent on political actors who act toward them as benevolent and trusting sponsors and protectors. When they invest money into research, the sponsors deviate considerably from the usual logic of political action in two respects. Firstly, financing research is almost always a high-risk investment. Most research, even if it is already quite focussed toward a particular application, fails to satisfy the extrascientific world's expectations. Thus, some of the funds are inevitably "wasted." This does not fit into the "politics of blame avoidance" (Weaver 1986) usually pursued by political actors. Financing research makes a political actor very vulnerable to delegitimation, because societal groups will inevitably justify their own demands for money from the state by pointing out how much tax money is wasted on research. Secondly, even if a considerable return on investment is yielded by the financing of particular research activities, a long time usually elapses before this return becomes visible. Political parties and party politicians, in particular, forced to present quick successes to the public in order to be reelected, tend to pass this orientation on to the ministries, too.

Although these considerations would lead us to believe that it is improbable that any political actor would ever invest in research, they do it all the time. There are ministries responsible for financing research, and politicians dedicated to furthering the cause of research policy. The various reasons for this surprising confidence of political actors in research are of no concern here. What is important, however, is that their benevolence is limited in two ways. Whenever the state's financial resources become scarce, the ministry in charge of research comes under pressure from the ministry of finance, and from other ministries competing for these resources, to cut back "luxurious" expenditures for "useless" research, or to induce a reorientation of research toward more useful goals. And whenever societal risks of particular research activities become visible, the ministry is pressured to introduce regulations in order to reduce the risks. To some degree, the ministry can act as a buffer for the research system against the trouble originating from other political actors, such as the ministry of finance (Stucke 1993). Nevertheless, the very political actors whom research actors need as their benevolent sponsors and protectors are also the ones who have to implement the measures which cause trouble. Sometimes they do this reluctantly because they are forced to by another political actor; sometimes they want to do it to achieve their own ends. The sponsorship and protection from these political actors thus breaks down just when research actors are most desperately in need of sponsorship and protection.

Lacking an effective obstructive capacity and support from benevolent political actors, research actors are left with *persuasion* as their most powerful means of exercising political influence. *Direct persuasion* occurs when a research actor facing trouble tries to change the minds of the respective political actors by convincing them that their actions which caused the trouble were unwise or unjust. *Indirect persuasion* happens when a research actor mobilizes influential allies to take a stand in opposition to the political actors. Persuasion, thus, consists of bringing forward arguments that will convince either the political actors or the potential allies. The political actors have to be convinced that what they are doing is not in their own best interest, or that it is not in keeping with their own moral principles; or other actors have to be convinced that they, too, will suffer from what the political actors are doing to the research actors.

Of course, the principal problem with all kinds of persuasion is that one must be able to find convincing arguments. While they may well harm a research actor's self-interests, the measures implemented by a political actor will probably be fair and wise from the latter's point of view. Thoroughly convinced of his opinion on this matter, the political actor will probably be unshakable. Moreover, even if one does come up with convincing arguments, they can only be persuasive if the actors to whom one is appealing are willing to listen and to reflect critically upon their own point of view. If they have immutable prejudices, because strong self-interests compel them to see things in a particular way, it will be very difficult – and often virtually impossible – to persuade them with arguments that do not fit into their frame of mind. This applies especially when the arguments are not airtight.

1.2 The Low Capacity of Research Actors for Coordinated Collective Action

The research actors thus have no guarantee whatsoever that their attempts at persuasion will impress political actors at all; and even if persuasion has an impact, it remains to be seen how strong it will be. Persuasion is clearly a second-best means of political influence upon which research actors fall back because they lack the much better means of an effective obstructive capacity. But for persuasion to work at all, it is very important that the arguments put forward are presented as the unanimous point of view of all affected research actors. Each dissenting opinion among the arguers increases skepticism exponentially among the political decision makers to be persuaded.[5] If the decision makers are already somewhat skeptical, giving their skepticism even the slightest reinforcement may make any attempts at persuasion entirely futile.

Only if all the affected research actors speak with one voice do they have a chance to be listened to attentively by the political actors. This often requires an effective *capacity of research actors for coordinated collective action.* But looking at the research system from the perspectives of organization theory and of interest-group theory, one detects that this capacity, too, is largely lacking.

5 Scientists in many countries experienced this firsthand in the debates about the safety of nuclear energy (Nowotny 1979; Nelkin 1987). A few dissenters sufficed to shatter the public credibility of nuclear physicists.

Most kinds of research organizations, universities and state-financed research institutes outside of the universities exhibit a comparatively *weak hierarchy*. The main reason for this is that most of these research organizations have a relatively weak influence on their individual researchers' scientific careers (Luhmann 1990: 679-680). A scientist's career is mainly determined by the reputation he acquires within his scientific community. This reputation is the researcher's major "social capital" (Bourdieu 1975) on the "academic market place" (Caplow/ McGee 1958). In other words, researchers are primarily "cosmopolitans," not "locals" (Gouldner/ Newcomb 1958). Of course, the research organization to which a researcher belongs shapes his opportunities to acquire a reputation by the amount of resources and time it provides him for research, and by the degree of autonomy it leaves him in his choice of research topics. But this means that research organizations are, from the point of view of their individual researchers, merely opportunity contexts within which they do *their own* work. Research organizations are the means for their individual researchers' goal attainment. The best expression of this individualism of research work is the fact that publications are attributed to an author, not to his research organization. All researchers share such an egocentric view of their research organization. Even the directors at the top of a research organization take this view, or at least have to concede it to their researchers. Thus, a research organization is seen and used by its individual members mainly as a "common pool resource" (Ostrom 1992) which they have to divide among themselves, with each one trying to acquire the most for himself.

This peculiar social structure of research organizations is a consequence of their prevailing internal structure of interdependence between different research activities. Research organizations exhibit a high degree of *pooled interdependence,* which is the most loosely-knit type of task interdependence (Thompson 1967: 54-55). Pooled interdependence means that there are no direct unilateral or reciprocal dependencies among researchers or research groups with regard to their research results. None of them needs the results of any other researcher or research group within the research organization to continue his or its own work.[6] In many scientific disciplines, an individual

6 This does not exclude that the work of others within the organization is often inspiring to a researcher. But such inspirations, while they may certainly be helpful, are not necessarily vital to one's work.

researcher still does his own work independently from his colleagues within the same research organization. At most, a few younger researchers form a small group around a professor or experienced researcher.

In other disciplines, especially in certain areas of the natural, medical, and engineering sciences, the advent of "big science" has led to groups taking the place of the individual researcher. Sometimes, as in particle physics, the cooperation of very large groups of researchers is necessary. However, these groups do not make up the whole research organization, since large groups tend to be parts of very large organizations. Thus, "big science" has not altered the picture fundamentally. Research organizations still tend to exhibit pooled interdependence among groups; only within groups is there sequential or reciprocal interdependence. The egocentric view of individual researchers is partly transcended by or embedded within a group-centered view, and the research organization itself becomes the means of the various groups' goal attainment. The research organization is still seen from a particularistic point of view.

Within this pooled interdependence, researchers or research groups are dependent upon each other only in terms of each one's contribution to the research organization's standing, which is an aggregate result of their individual reputations.[7] The organization's standing, in turn, determines the amount of the "common pool resource" to be divided among its members. But even this type of pooled interdependence is often quite loose. If attractive exit options exist for individual researchers, they can leave a research organization whose standing is declining because too many of its researchers or research groups have performed poorly. Thus, research organizations are extremely fragmented, "loosely coupled" (Weick 1976) organizations.[8]

As corporate actors, however, research organizations are not only weakened "from below" by the particularistic way their individual researchers or research groups view and use them. In addition, state-financed research orga-

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7 This also means that the research organization and its leaders are quite dependent upon the research performance and reputation of the individual members. Institute directors thus have to respect their individual researchers' or research groups' egocentric or group-centered view of the organization.

8 This is most obvious at universities, whose internal division into departments and chairs has the purely enumerative character of a catalog of unconnected specialties. But a closer look reveals that the same situation prevails at many research institutes outside of the universities, despite the much more narrow focus of their research programs.

nizations are also weakened "from above." As already mentioned, the ministry providing a research organization's institutional financing usually acquires rights to participate in important organizational decisions. Therefore, as instruments of coordinated collective action against political actors, research organizations are strongly impaired. The respective political actors will exercise their right to participate in a research organization's decision-making process if this is necessary to obstruct collective action directed against the political actors themselves. Because the individual researchers or research groups cannot simply switch from their particularistic use of the research organization to a mode of action which will allow the organization to become a strong corporate actor representing their common interests, they clearly face a dilemma. When their determination to preserve their opportunities to exploit the research organization for their own particularistic goals prevails, they welcome a weak hierarchy. When, for a while, their well-being depends on an increase in the organization's political influence, they welcome a strong hierarchy because that might be necessary to save them collectively. But if it does save them, which is by no means certain, each individual researcher (or group) might have lost his (or its) autonomy – a price that might turn out to be too high.

Another kind of corporate actor capable of coordinating collective action against political actors is an interest association. Interest associations are voluntary organizations of individual or sometimes corporate actors with common interests. Usually, interest associations are polyarchic organizations. Those at the top are elected by the majority vote of all members. The elected leaders will probably try to influence political actors, but their scope of action will be partially circumscribed by the extent to which they are able to impose sacrifices on the association's members without having to consult them on every decision. Do the elected leaders have enough *generalized support* at their disposal to arrive at such decisions even against the wishes of some or all of their members?[9] This generalized support is so important because it expands the interest association's bargaining capacity in negotiations with political actors. With adequate generalized support, the interests of some members can be sacrificed in order to realize important interests of the major-

9 For the distinction between generalized or "diffuse" support and specific support see Easton (1965).

ity of members; or some narrow-minded short-term interests of all members can be sacrificed in order to realize their vital long-term interests.

There are interest associations within the research system, although this kind of corporate actor is much less common than in other societal subsystems. This fact itself already raises suspicions about the strength of interest associations within the research system. Indeed, those that do exist are usually weak polyarchic organizations which are unable to impose serious sacrifices on their members. Interest associations within the research system are weak because they need a very high level of consensus among their members, which strongly restricts their leaders' bargaining capacity in negotiations with political actors. The "logic of membership" forcefully impedes the "logic of influence" (Schmitter/ Streeck 1981). The high level of consensus is needed because persuasion, which is the only kind of political influence these interest associations can exert, tends to suffer irreparably from even minor dissent, as we explained above. Under these circumstances, interest associations within the research system can only articulate demands and make bargains for Pareto-superior changes. No member shall suffer; instead, as many members as possible shall profit from the bargains struck with the political actors. Indeed, no member shall even fall behind the others too much. Therefore, not only absolute losses have to be avoided, but relative losses must also be kept within strict limits.[10] Only if these conditions are fulfilled will political demands get the unanimous consensus of the interest association's members. This requirement sharply reduces the set of alternatives for coordinated collective action. Interest associations within the research system are thus often forced into totally unrealistic bargaining positions. But this means that they have nothing to offer or to promise which might move political actors to make concessions.

Another cause of the improbability of coordinated collective action of research actors is that many of them have good chances to realize their interests on their own. This is also a consequence of the low task interdependence of scientific research – within as well as between research organizations. The fragmentation of research activities allows most research actors – individual researchers such as university professors as well as research groups or research institutes – to behave as independent entrepreneurs pursuing only their own interests without restrictions imposed by task interdependencies with

10 Relative losses can provoke envy, which in turn may give rise to dissent.

other research actors. This entrepreneurship operates mainly on the market for separately budgeted funds, which are granted by political actors – either directly or via their financing of specialized agencies for research promotion such as the German Research Foundation – or by firms and other customers in the market for contract research. Of course, there is competition on this market. But, nevertheless, many research actors can hope to realize at least a substantial part of their interests by independent entrepreneurship. For them, the possible additional benefits of coordinated collective action are often smaller than their own additional effort required for participation. Thus, they refrain from it and stick to entrepreneurship. Even the research actors who would benefit from coordinated collective action have to estimate carefully whether enough fellow actors will come to the same conclusion, because the success of coordinated collective action depends on a critical mass of participants. Obviously, this may result in a mutually reinforcing discouragement: Ego does not participate in coordinated collective action because he expects that Alter Ego will not participate, and vice versa.

All in all, the low political influence of research actors is *overdetermined* because it results from two independent causal factors, each of which alone would already be sufficient: the low obstructive capacity of research actors, resulting from the peculiar character of scientific truths, and the research actors' inability to launch coordinated collective action, resulting from the peculiar character of task interdependence in scientific research. Moreover, the research actors' perception of these effects of both factors reinforces the second one even more. Actors are more inclined to join in coordinated collective action if they perceive that the respective corporate actors possess effective capacities for political influence. No one engages in a hopeless struggle.

As we asserted at the beginning, these general reflections about the political influence of research actors are meant to lay the foundation for our theoretical analysis of coping with trouble, which rests on the assumption that a research actor's possibilities for coping are decisively shaped by its political influence. Guided by this assumption, we will now turn to the cases.

2 The Improbability of Preventing or Getting Rid of Trouble

Obviously, the weak political influence of research actors strongly reduces their chances to prevent or get rid of trouble. Our cases reflect this correlation, first of all, in that only a few actually exhibit instances of an attempt to prevent or eliminate trouble – and only some of these were even partially successful. In Schimank's case, the advocates of the German universities demanded from government that university research be compensated for the losses of general university funds. This pressure politics was utterly unsuccessful. As Mayntz shows, the East German AdW's attempts to survive as an institution were equally futile. On the other hand, the two French case studies by Krauss and by Musselin and Vilkas show how anticipated trouble was sometimes prevented by INRA or the CNRS. In the period following the events examined by Hasse and Gill, the community of German genetic researchers was also quite successful in pressing government to reduce the legal regulations of their research significantly.

2.1 Consequences of a Low Obstructive Capacity

Each of the cases of an unsuccessful attempt to prevent or get rid of trouble, and every case in which such reactions to trouble were not even tried, illustrates the various features of the research actors' low political influence outlined above. In none of the cases did the research actors have a significant *obstructive capacity*, which is plausible as structurally determined from our theoretical framework. Because of their chronic inability to influence political actors by obstruction, research actors have to rely on less effective kinds of social influence.

To counteract some kinds of trouble, certain research actors can insist that *formal rights* granted them by the state be protected – for instance, the German professors' autonomy with regard to choosing their research topics. This right is even constitutionally guaranteed. Its observance by political actors keeps them from ordering professors to do particular kinds of research or work on particular research topics. However, the political actors can often change such rights if they seem to be hindering the realization of political goals. In this respect, the German professors' autonomy, which is based on a law that is very difficult to amend, is a rare exception.

A more promising way to influence the political actors who have caused trouble is *social exchange*. Research actors are sometimes in a position to offer the political actors something in return for their refraining from or discontinuing measures which will cause trouble. For instance, research institutes may promise to redirect their type of research according to the wishes of political actors if the latter agree to revoke plans to cut back the institutes' budgets. As in Braun's case of the British biomedical research community, the political actors may even propose such a bargain to the research actors.

But neither formal rights nor opportunities for social exchange are readily at hand for research actors. Most often, they have nothing to fall back on but *persuasion*. Persuasion, be it direct or indirect, will be attempted in almost all situations of trouble. Some of our cases exemplify the types of arguments used and the kinds of allies sought after. In Schimank's case, the advocates of the German universities not only tried to convince the government that its responsibility for the country's future demanded an increase of general university funds, but they also presented the same arguments to the general public and to special interest groups such as business associations and trade unions. In this way, the advocates made an appeal to the long-term interests of government and societal groups. In the cases presented by Hasse and Gill and by Gläser et al., the genetic researchers and the nuclear researchers tried to assure the protest groups and the general public that genetic engineering and the research reactors did not pose a threat to their well-being. This was a primarily cognitive argument, which tried to eliminate what the research actors perceived as a judgmental error on the part of the public. Mayntz's case exemplifies a strong moral plea. The feared dissolution of the East German Academy was declared by some of its members to be another proof of an unjust and merciless conquest of East Germany by the West Germans.

As these examples suggest, such attempts at persuasion often bear no fruit. Particularly Stucke's case of the German big science centers indicates that persuasion is sometimes used even though the research actors are well aware from the beginning that it will be ineffectual. Such consciously fruitless prevention or coping activities can be interpreted as "symbolic politics" (Edelman 1964) by which the normative expectations of the individual members of the respective research organization or interest association regarding how their leaders should act in such a situation are fulfilled. In such cases, persuasion with arguments sure to be ignored is just an habitualized "standard operating procedure" (Nelson/ Winter 1982) for dealing with trouble situations, which follows a "logic of appropriateness" rather than a "logic of consequentiality" (March/ Olsen 1989).

A special difficulty of certain situations in which persuasive attempts are made is illustrated by Hasse and Gill in their study of the German genetic researchers. Sometimes different actors whose support is needed have to be persuaded by mutually incompatible arguments. What persuades one actor dissuades the other, and vice versa. If the research actors do not meticulously separate these different audiences, it becomes apparent to everyone that the persuasive efforts are ridden with contradictions,[11] which destroys their convincing power. Thus, if different actors whose support is needed can only be reached by mutually incompatible arguments, this fact must be carefully concealed. Care has to be taken to prevent an actor from hearing arguments not directed at him. If this cannot be accomplished, as with the German genetic researchers, persuasion becomes very difficult.

Sometimes, though, persuasion is successful. Mutual trust between research actors and political actors seems to be an important supportive element of successful persuasion. As mutual trust generally increases with the density of contacts, it is not surprising that the French cases presented by Krauss and by Musselin and Vilkas exhibit several instances of successful persuasion. Political and scientific elites are much closer to each other in France than, for instance, in Germany.

In sum, research actors are often not very influential, so they are unable to prevent or get rid of trouble. In certain situations, however, they do not need to defend their interests themselves because they coincide with the interests of other, highly influential actors. This is indicated by recent developments in the case presented by Hasse and Gill. The genetic researchers from state-financed institutes had *powerful allies* in the big pharmaceutical and chemical firms, whose research was also restricted by the regulations of genetic engineering. These allies were able to press the federal government to remove quite a number of the legal restrictions of genetic research; researchers from the universities and the Max Planck Society profited from this, too. Such a coincidence of interests occurs when what is at stake is an indivisible good, so that there is no competition among the negatively affected actors. Regulative trouble often meets this condition whereas resource trouble usually does not.

11 See Goffman (1956) for this requirement of "impression management."

Finally, there is the phenomenon of *"mock trouble"* detected by Krauss in the case of the INRA in France. "Mock trouble" is a mere show for the public and the political opposition[12] by which the political actors can symbolically display their determination to take a hard line with obstinate research actors without actually doing them any real harm.[13] This kind of "mock trouble" is staged by both sides – research actors and political actors – for a third group of political actors. This collaboration presupposes that the institutes are well-informed about the politicians' situation and in close contact with them – a constellation which is characteristic for France, as already mentioned. Another kind of "mock trouble" which happens even more frequently is the staging of trouble by the research actors for the political actors. This amounts to a dramatic rhetorical exaggeration of the potential or actual suffering of research actors resulting from certain political interventions. Of course, this is done to keep political actors from intervening further. If it works, this is the most elegant way of preventing real trouble. At least the research actors can hope that loud protests will deter the political actors from implementing measures which would increase the trouble even more. Stucke's case may be interpreted partly in this way. Thus, by complaining bitterly about actual financial cutbacks, the German big science centers hoped to keep the federal research ministry from pondering even harsher measures, especially the dissolution of whole centers.

2.2 Difficulties of Coordinated Collective Action

All these makeshift attempts to influence the political actors can hardly compensate for the research actors' low obstructive capacity. The research actors' *capacity for coordinated collective action* is a further important prerequisite for the prevention or elimination of trouble. This kind of solidarity does not necessarily imply that the research actors will transcend their narrow self-interests for the welfare of some collectivity to which they all belong. While such altruistic solidarity involving an actor making sacrifices for the collective

12 See the comparable phenomenon of "mock bureaucracy" described by Gouldner (1954: 182-187, 216-217).

13 This is one of the most common strategies of "politics as symbolic action" (Edelman 1964).

good may sometimes occur, instrumental solidarity in keeping with the research actors' particularistic view of the research organization will be more common. According to the familiar maxim that there is strength in numbers, research actors may simply realize that their self-interests could best be furthered by coordinated collective action.

As we already pointed out, this assessment is obvious neither to individual researchers nor to research institutes. While research actors are well aware that their trouble is of such great magnitude that collective action is necessary if the trouble is to be prevented or eliminated, they also realize that particularistic coping by each research actor individually will often result in an adequate – albeit small-scale – adaptation to the trouble. Such a piecemeal solution is sufficient for some researchers. As we explained, this applies primarily to those research actors for whom particularistic coping is the better bargain because solidaristic coping's added benefits are smaller than the added effort it requires. It also applies to the many research actors who are not harmed much by the trouble, or even profit from it. Such researchers, who are actually winners – or at least not losers –, can be found in all the case studies. For instance, in Schimank's case there were some professors whose research was not very resource-demanding and others who benefitted from the ministries' policy of redistribution of general university funds. Another example, in the case presented by Musselin and Vilkas, is the research group of the CNRS laboratory that perceived the move away from Paris not to be trouble but, actually, a good opportunity. For the first laboratory in Wolf's case, dissolution of the East German AdW presented no trouble at all. In Stucke's case, too, some big science centers were exempted from the resource cutbacks, as were some institutes at centers generally faced with trouble. Finally, in the case presented by Gläser et al., some of the users of neutron beams who could do their research in other laboratories were not seriously affected by the trouble facing the research reactors in Berlin and Munich.

For these two reasons, the troubled population of research actors is often divided into two segments: those who prefer solidaristic coping, and those who prefer the particularistic approach. The larger the second segment is or appears to be, the less mobilization for coordinated collective action will occur aimed at preventing or getting rid of trouble. Some political actors who are aware of this weakness exploit it, employing the strategy of *"divide and conquer"* (Baumgartner et al. 1978) to break down the potential for solidaristic coping; others at least welcome such side-effects of their measures if they arise. In Schimank's case, government actors knew that the professors who profited from the redistribution of general university funds would hardly be inclined to join solidaristic coping activities. Such side-effects of "divide and conquer" would have been even more important for a suppression of solidaristic coping in Mayntz's case if there had not been a widespread preoccupation within the AdW with reforming its internal structure. This focus of attention kept the members of the AdW from using the Academy as an instrument of coordinated collective action. But even if they had, the government's declared intention to ensure that scientifically qualified institutes and research groups of the East German AdW should survive the Academy's dissolution probably would have contributed to inhibiting effective collective protests. When the situation of the AdW became critical, but before its fate was sealed, the institutes expecting to be among the survivors would have begun to care less for the Academy's fate and to concentrate, instead, on their own future prospects. The institutes which hoped, but were not sure, that they would be among the survivors would have concentrated all their efforts on improving their own chances, increasingly neglecting the possibility of surviving within the Academy. The institutes, however, which considered their chances to survive on their own to be low and which had not already given in to helpless suffering would have tried the hardest to mobilize collective action for the rescue of the AdW as a whole because this was their last chance to ensure their own survival. This hypothetical scenario shows a pattern of solidaristic coping frequently resorted to by actors who estimate their chances of particularistic coping to be rather low. To put it drastically, this kind of instrumental solidarity is often rooted in desperation. If political actors succeed in giving just a few troubled actors a ray of hope, this can suffice to stifle effective solidaristic coping.

Still, even if a research actor concludes that coordinated collective action would serve its interests best, two potential problems remain. The first is the familiar phenomenon of free-riding (Olson 1965). If an actor expects that a sufficient number of others will engage in coordinated collective action to prevent or get rid of trouble, and if this actor stands to profit from their potential success, it will be tempted to refrain from participation; but if many actors think this way, nothing will happen. This does not occur very often, though, because a research actor already begins to doubt the feasibility of its participation in collective action when it considers how unlikely it is that enough others will actually participate. This insecurity of attaining the necessary "critical mass" (Marwell/ Oliver 1993) arises partly from the perception that some of the others are not strongly affected by the trouble, and that most of the others have opportunities to help themselves at least to some extent.

Thus, the probability that coordinated collective action will prevent or get rid of trouble is rather low. If collective action arises at all, it manifests itself very often via interest associations expressing Pareto-superior demands without sufficient scope for bargaining with the political actors. In Schimank's case, to give just one example, the West German Rectors' Conference (WRK) was forced to demand nothing less than full-blown compensation for the resource losses of professors, although everybody knew that government actors were neither willing nor able to meet this demand. As outlined above, interest associations within the research system *lack autonomy* because their members will not give it to them. An instructive example is found in Weyer's study of space research in Germany: The aeronautics research institutes founded an intentionally weak interest association because they each feared losing their institutional autonomy. Interest associations are unable to disregard the interests of even small minorities of their members in order to represent the interests of the whole population facing trouble. The leadership of the WRK had to respect the vote taken among all members regarding every important decision. Formally, majority decisions are taken; but informally, a total consensus is aimed for because decisions are not binding for those who disagree. The members of the Association of National Research Centers (AGF), a relevant actor in Stucke's case, guard their autonomy so jealously that they do not endow their interest association with any significant decision-making authority whatsoever. Instead, each big science center tries to lobby for its interests on its own, often openly against other big science centers. Coordinated collective action supported by many – or even all – of them happens only rarely and spontaneously, and it is not initiated by the AGF.

When hierarchically structured research institutes – or other corporate actors which are not of an associational character, like the funding agencies in Braun's case – are the potential initiators of coordinated collective action on behalf of their individual members or clients, other problems occur. Such institutes usually have much more autonomy in relation to their members than polyarchic interest associations.[14] This gives them greater leeway when bar-

14 German universities in particular lack this hierarchical structure. In this sense, German universities resemble the interest associations just mentioned more than they resemble

gaining with political actors and enables them to participate effectively in collective action, but it may also cause them to diverge widely from their members' interests. Such a divergence may result from the fact that trouble is often *level-specific* in at least three respects.

Firstly, even if we assume here for simplicity's sake that the interests of a research institute as a corporate actor are merely the sum of all its individual members' interests, its interests are nevertheless broader than those of each individual researcher. The same holds true, of course, for the relation between departments of an institute and the institute as a whole. Higher-level interests are always more inclusive than lower-level interests: It can be in the higher-level actor's interest to sacrifice the interests of some of its members in order to protect the interests of other members. Examples for this type of sacrifice can be found in the case of the CNRS in France presented by Musselin and Vilkas or in Stucke's case of the big science centers in Germany.

Secondly, in addition to having more inclusive interests than each individual member, institutes as corporate actors have interests of their own which go beyond the aggregate interests of their members. Each corporate actor is interested in its own continued existence and growth, the preservation or expansion of its domain, and the maintenance of its autonomy (Scharpf 1989: 45-46; Schimank 1992: 263-264). These interests coincide sometimes, but by no means always, with the interests of the respective lower-level actors. Thus, in the case presented by Gläser et al., the interests of the Hahn-Meitner Institute converged with those of its constituent institutes and of the researchers who needed the research reactor because atomic research was still an important element of this big science center's corporate identity. But, as Stucke's case shows, the situation was quite different in many of the other big science centers. There, the interests of the individual researchers and institutes working in research areas that were no longer scientifically promising and were not in government demand were not shared by the centers' directors, whose primary concern was the standing of their own center compared to others and its reputation at the federal research ministry. Another example is the Max Planck Society, which as a group of research institutes has to defend its autonomy in the face of political actors' attempts to intervene in the selection of research topics. In periods of resource scarcity, each institute of the Max Planck Society may very well be interested in acquiring as much separately

research institutes.

budgeted funds as possible from federal or state ministries although this implies the danger of weakening the group's autonomy. Mayntz's case is perhaps the clearest example for a strong divergence of interests between levels. The individual researchers and institutes of the AdW whose research performance was excellent had no strong interest in the AdW's preservation, whereas the AdW, in turn, did not share their interests in finding new and better institutional niches for their research activities.

Thirdly, as outlined above, state-financed research institutes have to act to a certain degree as agents of their political principals. Thus, unfortunately, the research actors with the best connections to the political actors – which we have shown to be helpful for the prevention or elimination of trouble – are frequently obliged to implement the political measures which cause the trouble. This applies certainly to the leadership of the German big science centers in Stucke's case, to the director of INRA in Krauss's case, and to the directors of the CNRS in the case presented by Musselin and Vilkas.

Accordingly, research institutes as corporate actors are not always an obedient instrument of their members' coordinated collective action. Even if institutes represent their members' interests to political actors, they may do it only half-heartedly for the reasons just mentioned. The members, in turn, reflecting upon this, will often refrain from any attempt to push their institute towards coordinated collective action to prevent or get rid of trouble.

All in all, the chances for coordinated collective action as a means of political influence to prevent or get rid of trouble are small. Moreover, even if such action is actually taken, its success is still extremely dependent upon the political actors' action space. They must be able to retract or discontinue the measures which caused the trouble to the research actors. The extent to which the relevant political actors are forced to act as they do by circumstances they cannot change determines just how futile attempts to prevent or get rid of the trouble can become. The action space of the political actors who caused the trouble, in turn, is determined by the extent to which other actors can narrow it down. Foreign states may apply considerable pressure on political actors, as Weyer shows in the case of the prohibition of German rocket research after World War II. Public opinion and protest groups can also attract political actors' attention, as the two cases of regulatory trouble presented by Gläser et al. and by Hasse and Gill emphasize. By forcing budget cuts on the political actors responsible for research policy, the ministry of finance can impose serious constraints, as the cases of the professors and the biomedical researchers described by Schimank and by Braun illustrate. Other policy concerns – in Schimank's case, for instance, educational policy making it impossible to restrict the number of students admitted to the universities – may dominate research policy.

From our first examination of the cases, we can conclude that the conditions necessary for preventing or eliminating trouble are seldom fulfilled. Our general considerations about the research actors' low political influence originating in their inadequate capacity to obstruct processes of societal reproduction and to engage in coordinated collective action are confirmed by the cases. But if such a political mode of coping is possible only under exceptional circumstances – some of which could be detected in the cases – how *do* research actors usually cope? Which alternative reactions to trouble, apart from helpless suffering, remain open to them? With this question in mind, we shall review the cases again.

3 Adaptation to Trouble and Its Shortcomings

Whenever trouble cannot be prevented or got rid of, coping can be nothing more than a "response to irreversible loss."[15] Research actors' adaptive reactions to worsened research conditions go in two main directions: accommodating themselves to their worsened research conditions, or searching for a way to escape from their trouble.

Efforts to accommodate aim at making the best of a bad situation. In a situation of resource trouble, research actors may economize, either by proportional cutbacks, by queuing resource demands according to their temporal order, or by putting greater emphasis on a rank-ordering according to substantial priority. Research actors may deal with regulative trouble or trouble from extrascientific interventions in their research program by being obedient and, at the same time, working toward a step-by-step shift in their research topics and methods. Research actors may handle trouble from institutional restructuration by gradually learning how to pursue their own research interests under

15 As Fritz W. Scharpf put it in a discussion at the conference on "Coping with Trouble" in November 1992 at the Max-Planck-Institut für Gesellschaftsforschung in Cologne.

their new institutional conditions. There is sometimes only a thin line between such accommodating efforts and helpless suffering.

Searching for a way to escape from a troublesome situation is a very different means of adaptive coping. Individual researchers may leave their present institute for one that is trouble-free, or at least seems to have less trouble. Such an exit option is usually not available to research institutes. But they can try to find new sources for funding in situations of resource trouble – an option also open to individual researchers. Research actors can react to regulative trouble by secretly trying to get around the regulations, and they can counteract trouble from extrascientific interventions into their research program by pretending to conform to these expectations while actually trying to continue research according to intrascientific priorities. Confronted with trouble from institutional restructuring, research actors can use, for instance, networking strategies to find themselves a new niche that is trouble-free. All of these possible strategies of adaptive coping are illustrated in our cases.

3.1 Conditions for Solidaristic Adaptation to Trouble

Adaptive coping can be particularistic in character, but it may also be solidaristic. This latter type of coping can either be hierarchically enforced upon actors facing trouble, or it can be self-organized by them. The advantages of *solidaristic adaptation* over particularistic adaptation are obvious. Solidarity makes it possible for relatively better-off research actors to help ones that are worse off; at the very least, ruinous competition among actors vying for the scarce opportunities to escape from their troublesome situation can be prevented. Nevertheless, in our cases there are not many examples for solidaristic coping. Although it is easier to accomplish than the prevention or elimination of trouble, it is still too difficult in most situations.

Hierarchically enforced solidaristic adaptation to trouble is something quite common in other societal sectors – in industrial firms, for instance, where the managers decide which departments shall suffer to what degree from necessary budget cuts so that the firm as a whole suffers least. No matter what kind of trouble is facing a research institute – resource cuts, submission to political regulations, or imposed research topics – the hierarchical leaders' capacity to enforce a differential allocation of the trouble within the collectivity will depend on the strength of their authority. As already mentioned, interest associations within the research system usually do not possess such authority over their members. Within research organizations – particularly within universities – strong polyarchical elements and professional solidarity can interfere with the leaders' authority. Thus, there are several reasons why a president or dean at a German university will be quite reluctant to impose a differential allocation of trouble on the professors, the main one being that he is often bound by the mutual attitude of cooperativeness described in Schimank's case. On many of the relevant issues, moreover, the dean or president cannot decide alone, but must heed the majority vote of the respective group of professors. This differs greatly from the situation in American universities, for instance, where the deans and presidents possess greater authority and the stage is thus set for hierarchically enforced solidaristic adaptation to trouble.

For hierarchically enforced adaptive coping to be successful, the hierarchical leaders must be willing to use their power for a differential allocation of trouble. That such assertiveness is often lacking stems not only from factors common to all kinds of hierarchies, such as the superiors' fear of conflicts and of taking the responsibility for allocative decisions that may be wrong, but from a special factor peculiar to the professional socialization of researchers and, hence, the research system: the academic's conviction that neither professors nor their counterparts in extrauniversity institutes should be subject to hierarchical orders from someone who is their professional equal. This widespread attitude may keep a university president, a dean, or a director of a research institute from effectively using his power, as is explicitly mentioned, for instance, by Musselin and Vilkas in the case of the CNRS in France.

Coping that takes the form of solidaristic adaptation to trouble will therefore tend to be *self-organized* rather than hierarchically ordered. Even adaptation that formally appears to be hierarchically enforced is often de facto self-organized. Self-organization in this context means that a plurality of actors faced with trouble assemble into a solidaristically coping collectivity. Self-organized solidaristic adaptation to trouble does not come about "from above" but "from below." Since this does not require a hierarchy, it is easier to achieve. At its minimum, self-organized solidaristic adaptation to trouble is nothing more than an implicit mutual nonaggression pact resulting from an attitude of cooperativeness, as exemplified by Schimank's case of the German professors mentioned above. This adaptation requires neither binding agreements among those involved, nor even an explicit communication of the intention to respect the others' interests. All that is necessary is *continual mutual observation,* so that everybody knows about everybody else's moves. If this is guaranteed, risk-averse actors whose primary concern is to avoid others dumping their trouble on them, and conflict-averse actors who shy away from the stress of conflicts with colleagues, will jointly bring about such an implicit mutual nonaggression pact.

Mutual observation requires mutual visibility of the actors involved, which confines the size of the respective collectivity. The multilevel structure of actors within the research system sometimes makes such visibility quite easy. The vertical architecture of the actor constellation at the German universities, for instance, produces visibility by decomposing the multitude of individual professors into small units of mutual observation. Professors within faculties, faculties represented by their deans within universities, and universities represented by their presidents within the WRK: On each level, the actors can survey what the others are doing. Thus, nobody can expect to be able to cheat the others secretly, and nobody has to fear that he might be secretly cheated by the others. The more the higher-level actors depend on their lower-level members' acceptance, the better an overall coordination of a great number of individual researchers can be achieved, as the universal mutual nonaggression pact within the German university sector demonstrates.

For explicit agreements to result from bargaining, the number of actors within a unit of communication has to be much smaller than the number of actors within a unit of mutual observation. Weyer's case contains examples of self-organized networking among research institutes trying to cope solidaristically with their common trouble. These institutes agreed upon goals whose realization would make each of them better off. This is a comparatively simple situation for solidaristic self-organization. Explicit agreements are even more necessary, but much more difficult to reach, if solidaristic adaptation to trouble is to achieve a differential treatment of the actors involved. In most situations, many different allocations of trouble within a collectivity are possible in the sense that many winning coalitions meeting the demands of institutionalized decision rules could be formed. Which coalition actually results depends on diverse factors shaping the actors' interests and opportunities. Such winning coalitions are able to exploit the losers by shifting their own burden of trouble onto them. But again, because of this very multitude of possible winning coalitions, such exploitative moves are frequently not initiated.

Thus, each coalition of exploiters would be highly unstable, because someone now belonging to it might be a loser if a new coalition is formed tomorrow. Even to suggest forming a certain coalition of exploiters harbors the risk that others might decide to form a coalition themselves to which one might not belong. Only if some factors, such as strong cleavages established for other reasons, sharply limit the number of possible winning coalitions of exploiters can such a coalition actually be established.

Thus, if a differential treatment of actors facing trouble is achieved at all, it tends to go in the opposite direction. The better-off actors help the worse-off ones.[16] The Fraunhofer Society (FhG) in Germany, for example, had a very hard time just surviving in the 1950s and early 1960s (Hohn/ Schimank 1990: 181-211). Financial resources from contract research were very scarce and unevenly distributed. Some institutes were not able to cover their own costs for a long time, while others took in much more than they needed for themselves. To survive as a group, the FhG redistributed resources from the comparatively well-off to the worse-off institutes. The better-off institutes approved of the redistribution, perceiving that they, too, would profit from the preservation of the group as a whole. In addition, there was the implicit understanding that a sort of mutual insurance was established. The institutes now helping the others would have a right to call upon them for help in future if they needed it.

Solidaristic adaptation to trouble, therefore, tends to be self-organized. When it is, it usually results in a proportional allocation of trouble among the affected actors. Thus, solidaristic coping often stabilizes the status quo of the distribution of research conditions among the troubled research actors. When solidaristic coping is involved, the trouble does not result in an escalation of the fights among research actors for the maintenance and improvement of each one's research conditions – although political actors often want to trigger just that, as the government actors in Schimank's and Braun's cases explicitly declared. But a crucial prerequisite of solidaristic adaptation to trouble is that the affected actors are able to mutually observe each other,

16 One might interpret this as an exploitation of the former by the latter. But the better-off actors differ from the winning coalitions just discussed in that they agree to help the others, often without having to be persuaded to do so. Sometimes a large majority of actors decide to help a small group of worse-off actors which would not have been able to press for this help.

so that deviations from the attitude of cooperativeness can be detected and sanctioned. Often this condition is not fulfilled, so that only a *particularistic adaptation* to trouble is possible – the kind of coping least difficult to accomplish because it requires neither hierarchical authorities, mutual trust, communication among actors, nor even mutual observation.

3.2 Particularistic Adaptation as the Prevalent Reaction to Trouble

The majority of instances of coping cited in the cases must be categorized as particularistic adaptation to trouble: the competition of researchers for separately budgeted funds in Schimank's and Braun's cases; the strategies of the institutes and research groups endangered by disassociation from the CNRS in the case presented by Musselin and Vilkas, by dissolution in Wolf's case, by resource cutbacks in Stucke's and Weyer's cases, and by restrictions of their research methods in the cases presented by Hasse and Gill, and Gläser et al.; finally, the exit of individual researchers from troubled institutes reported in many of the cases. In game-theoretical terms, particularistic coping is a *game against nature*: against other actors whom one regards as causal forces which one cannot influence strategically.

Very often the allocation of trouble among actors trying to cope particularistically results in a skirmish akin to the Hobbesian war of everybody against everybody else – although, of course, particularistic coping does not take place within a "state of nature." These struggles to grasp scarce opportunities to reduce one's own damage from trouble occur whenever these opportunities are available to many other actors facing trouble as well,[17] as is the case with separately budgeted funds, or with research facilities like the research reactors for the groups of users of neutron beams in Gläser et al.'s case. Under such circumstances, particularistic adaptation to trouble quickly develops into a ruinous competition for coping opportunities. Some actors confronted with trouble will come out of this sooner or later without any opportunity to cope. They will have been reduced to helpless suffering. These are the absolute losers of particularistic coping. Most other actors will find – and take advantage of – opportunities to cope, but the competition will exact an ever-

17 See, again, Ostrom's concept of a "common pool resource" (Ostrom 1992).

increasing price. These actors are, therefore, relative losers. Thus, if coping opportunities are available collectively, almost everybody involved in particularistic adaptation to trouble will lose.

Sometimes, though, particularistic coping can rely on discrete coping opportunities, each of which is exclusively available to one particular actor facing trouble. In such cases, the actors can avoid the additional losses resulting from ruinous competition. For example, many of the methods employed by professors to cope with the time pressure of an increasing teaching load do not interfere with each other.[18] If one professor reduces the quality of his teaching, this does not stop another professor from doing the same, whereas one professor's gain of separately budgeted funds is another's loss. Similarly, many strategies used to cope with regulatory trouble identified in the cases presented by Hasse and Gill, and by Gläser et al., are available exclusively to the actors faced with that particular trouble. For instance, if one group of researchers tries to secretly circumvent the legal restrictions of genetic research, that does not deprive another group of the same opportunity – as long as this tactic does not become so evident that government agencies detect it and react with stricter controls.

The widespread use of particularistic coping is not only due to the comparative ease with which it can be accomplished, but also to the research actors' prevalent *weakness of will*. Actors engaged in any kind of solidaristic adaptation to trouble or in the solidaristic elimination or prevention of trouble will repeatedly pass by situational opportunities for their own particularistic adaptation to trouble. These opportunities are temptations to openly or secretly abandon the solidarity achieved – the bigger one's troubles are, the more tempting the opportunities look. "High cost situations" (Latsis 1972) are bad times for solidarity. Each actor, moreover, knows that this weakness of will is not confined to himself, but that the others are afflicted with it, too. Thus, each research actor has to take into account not only his own weakness of will, but everybody else's, the latter reinforcing the former. The others' assumed or proven weakness of will not only necessitates one's own grasping of opportunities for particularistic coping, but also serves as a ready excuse for it. Thus, even just a few situational opportunities for particularistic coping

18 As long as their aggregate effects are not so conspicuous that the ministry responsible for the universities is prompted to interfere by instituting a stricter monitoring of teaching.

can forcefully undermine solidaristic coping[19] – as if it were not difficult enough to achieve for so many other reasons.[20]

Particularistic adaptation to trouble is possible on any *level of action* – from the lowest level of individual researchers up to the highest level of interest associations which deal directly with political actors. Stucke's case illustrates this. Just as an individual researcher working in one of the German big science centers may cope on his own with the resource cutbacks as they affect him, any of the centers may do the same even if other centers are affected by the same resource cutbacks and even if an attempt at a solidaristic removal of trouble has been made. Nevertheless, as already mentioned, the particularism of higher-level actors has a broader scope because they have to aggregate the interests of a plurality of lower-level actors. Higher-level actors able to observe each other and communicate with each other are not easily swayed by the particularism of their individual members. In this way, higher-level actors serve an important order-preserving function against the disruptive effects of individual particularistic coping.

Sometimes, though, higher-level actors deliberately allow for particularistic coping on the lower level of action or even promote it. This happens whenever it is in their interest to foster "healthy" competition among lower-level actors because it will make them more compliant or increase the quality of the lower-level actors' performance, especially by eliminating poor perform-

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19 In game-theoretical terms, the actors' weakness of will constitutes a Prisoner's Dilemma game in which each actor strongly discounts the future so that no "evolution of cooperation" (Axelrod 1984) – i.e. mutual trust in each other's firmness against temptations to grasp situational opportunities of particularistic coping – can occur.

20 However, a certain number of opportunities for particularistic coping may also stabilize solidaristic coping. These opportunities may give the actors facing trouble a little leeway for coming to terms with their weakness of will, which would otherwise destroy their solidarity altogether. To continue in the game-theoretical vein, this means that the game has a certain tolerance for what are called "trembling hand" phenomena (Holler/ Illing 1991: 106-107). Schimank's case might illustrate this. Only because the German professors were able to cope with their resource troubles particularistically by the acquisition of separately budgeted funds were they not tempted to destroy the self-organized solidaristic adaptation to trouble in the intrauniversity nonredistributive allocation of general university funds. But although this case demonstrates a mutually reinforcing coexistence of solidaristic and particularistic coping, it nevertheless points to the strong tendency of coping efforts towards a particularistic adaptation to trouble. This tendency has to be paid tribute to if solidaristic coping shall be maintained over time.

ers. Funding agencies occasionally try to use shrinking budgets in this way, as Braun's case shows. In the same manner, research institutes may stimulate the competition for resources among their departments or research groups, as is evident from the case study of the CNRS.

Quite often, however, higher-level actors try to initiate action by the lower-level actors which the latter can view as being solidaristic adaptation to trouble. Higher-level actors may do this because it is in the interest of the whole population of troubled lower-level actors to avoid ruinous competition. This goal of higher-level actors is frequently not at all easy to realize, as has been elaborated upon above. Solidarity can seldom be ordered or arrived at simply by incentives. For example, it is almost impossible for research institutes in trouble to prevent their best researchers from leaving for better jobs. Thus, attempts by higher-level actors to keep lower-level actors away from particularistic coping are often futile. Still, higher-level actors can at least sometimes cope solidaristically among themselves and thus tame the particularism of the respective lower-level actors somewhat.

Another factor which further reinforces tendencies of lower-level actors toward particularistic adaptation is the exceptional character of big trouble, which demands high sacrifices from many affected actors. The smaller the trouble is, the better particularistic coping works for an affected actor. But comparatively small trouble may happen frequently enough for actors to learn how to cope with it over time. Schimank's case of the German professors and Braun's case of the biomedical researchers illustrate this. Thus, particularistic coping is routinized to some extent. In contrast, actors are usually not prepared for the rare occasions of big trouble, when only solidaristic coping together with other affected actors might help them effectively. They have neither acquired adequate skills nor learned to rely on established social mechanisms which might promote solidaristic coping. Mayntz's case depicts such helplessness, which leaves no option but particularistic coping. Since big trouble is often accompanied by a strong time pressure, there is almost no chance to build up solidarity on the spot.

All in all, there is a weighty tendency towards particularistic adaptation to trouble on each level of action; the lower the level of action, the stronger this tendency is. All other kinds of coping, not to mention prevention, are much more difficult – and often impossible – to accomplish. But particularistic adaptation, although it may be advantageous to some of the actors in trouble, has only a comparatively *low overall effectiveness* for the whole population of these actors. Its defects are manifold and often interconnected.

First of all, by definition, particularistic adaptation to trouble does not eliminate its causes. It can be likened to treating only the symptoms of an illness. The pain vanishes, but the illness gets worse, unnoticed, until a point is reached when a cure is no longer possible. For example, if researchers' work is thwarted by restrictive regulations in one country, and they leave to do their research in another, less restrictive country, this may solve their problems for the time being. But what is to stop this other country from adopting similarly restrictive regulations? Of course, researchers might move into a third country, and so on. However, sooner or later there will be no more feasible exit options. Then particularistic adaptation to trouble will have come to a standstill, and the whole trajectory of coping turns out to be a dead-end street. Moreover, by the time the researchers notice this, their trouble has multiplied and is, hence, all the more difficult to cope with. As long as the restrictive regulations prevail in only one country, researchers may use the other countries as points of reference to indicate to the political actors how unreasonable their measures are. But if similar legislation is enacted everywhere, this argument becomes much less persuasive. It is not surprising, therefore, that in the cases studied by Hasse and Gill and by Gläser et al. not only the research institutes – which have no exit option – but also researchers and research groups were persistently trying to get rid of the trouble. That German genetic researchers can now work again under less stiff regulations is due to the fact that they did not confine their coping to particularistic adaptation.

More generally, when they opt for particularistic adaptation to trouble, actors run the risk that their coping, as successful as it may be in the short run, may even intensify their trouble in the long run. Weyer's case is an instructive example of how serious the *shortsightedness* of particularistic coping can be. When the institutes coped with the scarcity of resources at the state level by accepting generous funding from the federal government, they incurred a debt: Sooner or later, the rescuer wanted a reward. Thus, this kind of particularistic adaptation almost inevitably brought about the future trouble of becoming the object of instrumentalization by the federal government. This example points out that it might be very useful to systematically investigate which kinds of potential future trouble are inherent in the different kinds of particularistic coping.[21]

Another example of the shortsightedness of a particularistic adaptation to trouble demonstrates how it is often coupled with certain other serious defects resulting from "unhealthy" competition. If research actors, such as the German professors in Schimank's case, react to resource cutbacks by increased efforts to acquire resources from other sources, this quickly results in an escalating competition among them. This shifts more and more intellectual energy and working time from research work to resource acquisition, with decreasing – and increasingly insecure – returns. This is the first reason why competition beyond a certain point is no longer "healthy." Secondly, the insecurity as well as the time pressure associated with increased efforts to acquire resources furthers a preference for conventional, uncontroversial research topics and approaches, as Braun's case shows. Thus, beyond a certain point, increased competition strengthens the research orthodoxies, which is detrimental to the long-term innovativeness of the respective research areas. If research actors have adopted particularistic adaptation to trouble as their way of coping, they drift helplessly into a situation where competition becomes ineffective. This is, moreover, associated with increasingly sharp conflicts among the research actors. These conflicts emerge from the process of escalating competition as well as from its result, an increasing inequality of research possibilities.

As several of the cases show, some researchers, research groups, or research institutes are actually better off after their coping efforts, while most are worse off. Wolf's case demonstrates aptly that the research actors who are successful with their particularistic adaptation are not necessarily those whose research performance is superior. Other factors – such as political skills and connections, belonging to the established research orthodoxy, or the good luck that one's research area appears to be more promising with regard to its societal usefulness – are often equally or even more important than the quality of one's research work. Thus, because factors which have nothing to

21 Even an attempted prevention of trouble may produce future trouble, as the case presented by Hasse and Gill shows. The genetic researchers who wanted to prevent public distrust by openly discussing possible risks of their research and, later, by advising government to establish certain regulations ended up having to cope with heightened public distrust and extremely bureaucratic handling of the regulations.

do with research performance gain weight in a situation of increased competition, particularistic adaptation to trouble, again, makes research more ineffective.

Sooner or later, these negative effects of "unhealthy" competition are suboptimal from the point of view of the respective *political actors*, too. It is true that what is trouble to the research actors is often the desired result of political actors' measures to increase research performance or to direct research by initiating more competition. The difficulty often neglected, however, by political actors is that it is not at all easy for them to keep competition within "healthy" limits once it has started. This is partly because the point at which competition becomes counterproductive is not discernible until it has been passed. Only then do the negative effects mentioned materialize; only then are they taken seriously by the political actors, who always suspect – and not without reason – that research actors' laments about their troublesome situation are totally exaggerated. But if the political actors finally realize that the competition they stimulated has gone too far, they often lack adequate means to reduce it again.

Another suboptimal feature of particularistic adaptation to trouble from the point of view of political actors is sometimes the *anarchic character* of this kind of coping on the aggregate level. By definition, all kinds of solidaristic coping and solidaristic attempts to prevent trouble achieve some degree of intentional coordination of action among the actors affected by the respective trouble. This coordination brings about a strong reduction of the set of relevant alternative action possibilities. For instance, efforts to get rid of trouble converge into a few clear-cut options of collective action that otherwise would be a chaotic variety of independently pursued actions by a multitude of actors in trouble. Even self-organized solidaristic adaptation to trouble – such as a mutual nonaggression pact among the respective actors – has a recognizable pattern which allows political actors to calculate the endogenous dynamics and their aggregate effects. Such a calculation is a prerequisite of a purposeful political intervention. Obviously, however, the more independently acting actors there are to be taken into account, the more difficult this calculation becomes. Admittedly, sometimes political actors deliberately create an anarchic confusion of particularistic adaptations to pit the research actors against each other so that everyone's resistance to political interventions will be broken by exhaustion after a while. But this is not what political actors usually intend to achieve. Instead, their aim is usually to bargain with the research actors. This, however, is only possible with a small number of actors; the multitude of actors affected by some trouble is therefore required to bring about a solidaristic collective coordination of their actions.

Consequently, particularistic adaptation to trouble is often harmful not only to the research actors but, in the long run, to the political actors as well. Compared with other societal sectors, the research system exhibits a rather *defective trouble management.* Political actors are confronted with a highly fragmented multitude of research actors. It is not unusual for the level of the individual researcher coping by himself according to his own situational opportunities and individual interests to have a significant and disturbing impact on political interventions. But, as we spelled out at the beginning of this chapter, these defects of trouble management are unavoidable within the peculiar structural framework of the research system, which, in turn, is strongly determined by the character of research work. Thus, the catalog of dysfunctions of the prevailing particularistic adaptation to trouble presented here should not be misunderstood as a naive plea for more solidarity among research actors. Although this would benefit them as well as the political actors, such a plea would be in vain. This insight, in turn, amounts to the diagnosis of a growing political crisis of the research system in contemporary societies – if, that is, we are correct in our estimation (elaborated in the introductory chapter) that all kinds of trouble will intensify in future. Under these circumstances, the predominant pattern of coping by a particularistic adaptation to trouble will become increasingly defective.

There seems to be only one way out of this escalation of trouble. If political actors perceive that their measures which cause trouble for the research actors are becoming harmful for themselves, they may refrain from or discontinue measures which are – or would be – troublesome. This would amount to a political recognition of the politically dysfunctional aggregate effects of particularistic coping. For instance, referring to Schimank's and Braun's cases, if political actors intend to improve the overall research performance by increasing the competition for separately budgeted funds, they may discover after a while that they have overdone it and caused serious trouble to the good researchers whom they wanted to promote. They may find that they have initiated ruinous competition among researchers, which indeed has eliminated bad research, but has also harmed good research because the good researchers have become too occupied with resource acquisition and had to neglect research activities. This perception would give political actors the chance to correct their measures. Whether they are willing and able to do so depends on many other factors, though. Thus, even this way out of the crisis is anything but certain. The research actors' main problem – their incapacity to rescue themselves from trouble – persists. Since no ready solution is in sight, trouble will probably intensify in the near future.

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