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Empowering applicants

Conclave in the Tower of Babel: how peers review interdisciplinary research proposals

Grit Laudel

Peer review is a practice of research assessment where a researcher's work is evaluated by colleagues working in the same field on similar topics. Since interdisciplinary research is a new synthesis of expertise, the problem arises that peers in that sense do not exist. The aim of the paper is to show how under these conditions a specific institutional form of peer review counteracts the additional stress stemming from the interdisciplinarity of grant proposals and the multidisciplinary composition of the panel. The basis is an empirical study of networks of research groups belonging to different specialties. The key features of the procedure are the empowerment of applicants and the enforced interdisciplinary learning of reviewers. The applicability of this procedure appears to be limited to areas where interdisciplinary research is common and where interdisciplinarity is only 'moderate'.

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PEER REVIEW — the assessment of scientists' undertakings by colleagues working on similar topics — is the most important endogenous method of evaluation in science. It owes its dominance to the conviction of both scientists and non-scientists that the competence for assessing research can be acquired only by conducting research in the same domain of knowledge. In particular, peer review is ubiquitous in assessments of scientists' journal articles and project proposals.

This reliance on peer review is inherently problematic when interdisciplinary research is concerned. Since interdisciplinary research is a new synthesis of expertise, peers in the strong sense of the word do not exist. When new combinations of knowledge are tried in interdisciplinary projects, no one but those conducting the work are competent in all aspects of that combination.

These difficulties seem to produce a bias in grant decision-making against interdisciplinary grant proposals. While sound empirical evidence is scarce, the belief that peer review disadvantages interdisciplinary research is supported indirectly by observation that peers tend to favor research belonging to their own field. Porter and Rossini have shown that where the subject area of the grant reviewer and the subject area differed, the peer ratings were significantly worse (Porter and Rossini, 1985: 36). Other studies reported that "the structure of grant-giving agencies often mirrors the organizational structure of academia, so that those whose work falls outside of the established boundaries often have difficulty getting financial support" (Bechtel, 1998: 409; similarly Foltz, 2000: 436). In their observational study of a panel's decisions on grant proposals in a British funding agency, Travis and Collins found that

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reviewers tended to favour their own specialty areas or scientific school: "It is not that committee members are not of goodwill but that they simply do not fight so hard for subjects that are not close to their heart" (Travis and Collins, 1991: 336). They termed these phenomena "cognitive cronyism" and "cognitive particularism" respectively.

Langfeldt's (2001) study of assessment procedures indirectly confirmed the earlier findings. In her observation of the Norwegian Research Council's grant panels, Langfeldt found that the size of the budget and procedural factors discriminate between types of projects that are likely to be funded: tight budgets, fine-rating scales, average marks and majority decisions tend to favour established and low-risk research. This implies that interdisciplinary research, which is usually not established and more risky than disciplinary research, is at a disadvantage.[1]

These partial results also indicate how little we know about peer-review processes, let alone the peer review of interdisciplinary research. The whole area of interdisciplinary peer review has been neglected by science studies. The majority of the rapidly growing body of peer-review studies are conducted by practitioners, and are characterised by an atheoretical approach that is interested only in the reliability of assessments (for a critical review see Hirschauer, 2002). These studies leave the actual process of peer review black-boxed (Foltz, 2000: 428). Interdisciplinarity studies often emerge from the need to manage this type of research. Sometimes, suggestions are made for new procedures to assess interdisciplinary work in this context (eg IKAÖ, 1999; COSEPUP, 2004). However, the effects of the proposed procedures are not reported. Only very few studies systematically link conditions and outcomes of interdisciplinary research, and even fewer include peer assessment processes.

The much-needed progress of the field depends on our ability to say why certain institutional solutions work while others fail. It calls for a comparative approach, which in turn needs a framework that is grounded in theory. The aim of this study is to contribute to a theory of interdisciplinary research and research assessment by developing a comparative approach to peer review, analysing a specific procedure of interdisciplinary peer review and identifying the causal links between the conditions of the peer-review procedure and its outcomes.

Conceptual and analytical framework

The dominating perspective on peer review is scientistic in that peer review is seen as rational decision-making in which a set of 'objective' criteria is applied consistently by various reviewers. This yardstick, which is used in most criticisms of peer review as well as in suggestions to improve it (eg Cicchetti, 1991), has recently been challenged from a constructivist perspective. Hirschauer (2004) argues that the peer review of manuscripts is a process of collective knowledge construction, in which authors, reviewers and editors jointly construct the published article. This perspective, which can be extended to the construction of project proposals, has gained some empirical support by observations of scientists (Knorr-Cetina, 1981: 81, 88–89), as well as analyses of manuscripts and reviews (Myers, 1990). It has been shown that authors and applicants anticipate the peer review while writing, and that reviewers and editors contribute demands and suggestions that co-shape the outcome of the review process.

This process is as 'messy' (idiosyncratic, shaped by personal interests and power constellations) as any knowledge construction process in science. It cannot be 'objective' because the actors who take part in the collective knowledge construction can only apply their individual scientific perspectives, which are shaped by their individual research biographies, interpretations of the existing knowledge, personal networks and local working environments (Gläser, 2004: 74–78).

The interpretation of peer review as a process of collective, negotiated knowledge production implies that it is characterised by a specific actor constellation, and that it is necessary to analyse how varying actor constellations produce different outcomes (ie reviews) depending on their specific conditions of action. Institutional conditions can be assumed to be among the most important because they specify who takes part in the review process, what the roles of the different actors are, and what power the members of the actor constellation have in the negotiation of knowledge claims.

These considerations suggest the application of a framework that emphasises actors and institutional conditions of action. In my study, I applied the actor-centred institutionalism, a neo-institutionalist analytical framework that has been developed for policy analyses (Mayntz and Scharpf, 1995; Scharpf, 1997), and has been modified to support the analysis of institutions in science (Laudel, 1999). Actor-centred institutionalism is an analytical approach which responds to a specific theoretical interest in institutions by making them the central independent variable whose effects are to be investigated. It focuses on actor constellations and interactions shaped by institutions, but considers institutions as only one of several factors influencing actions, thus leaving room for the inclusion of other social and non-social conditions of action. The adaptation of this analytical framework to science studies required special attention to epistemic conditions of action, because actions in science are known to be influenced by the subject matter and tools of scientific work. Applying this approach entails answering the following general questions:

- Which actors are involved in the review process, and what are their respective roles, interests and power relations?

- What negotiations/knowledge construction processes occur in this actor constellation?
- What conditions (institutional, epistemic and others) affect the negotiations/ knowledge construction processes?
- How do the actor constellation and the specific conditions of action shape the outcome of the review?

It is obvious that any assessment of interdisciplinary research is characterised by a particularly heterogeneous actor constellation. Interdisciplinary research processes combine knowledge claims from different specialties, often by using methods from one specialty to solve a problem that is formulated in the context of another specialty (Parthey, 1983: 18–19; Bechtel, 1993: 282). The degree of interdisciplinarity varies depending on the number and dissimilarity of the involved knowledge claims (Laudel, 1999: 36–37). Usually, the actors involved in the review process belong to different fields. Therefore, their perspectives are shaped by different systems of knowledge, disciplinary standards, methodologies and work practices.

From these considerations it follows that, in the assessment of interdisciplinary grant proposals, specialised assessors judge proposals from a partial perspective shaped by their disciplinary background. At the same time, they hold more power because their scientific opinions cannot be challenged by their fellow assessors. This can amount to a veto position wherever their scientific specialty is concerned.

Interdisciplinary peer review of grant proposals is thus particularly problematic because it is necessary to synthesise several disciplinary opinions; and because a multidisciplinary panel has difficulties reconciling different perspectives and developing a commonly agreed yardstick for evaluating proposals. To a certain extent, these problems exist in disciplinary peer reviews, too. However, as criticisms of interdisciplinary peer review show, they are the reason why peer review of interdisciplinary research is perceived to be 'even worse' than disciplinary peer review.

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Empirical objects

As part of a larger qualitative study, I analysed the peer-review processes of two collaborative research networks, so-called 'Sonderforschungsbereiche' (SFB). SFB are financed by the Deutsche Forschungsgemeinschaft (DFG), Germany's most important funding agency for university research projects. SFBs are networks of research groups that receive additional funding for pursuing a collaborative research programme. An SFB consists of about 10 to 20 research groups from different specialties, mainly from universities; a few groups from non-university research institutes may be included. The research groups must be located in the same city. The initial proposal and proposals for extensions that have to be submitted every three years are evaluated by peer review. An SFB can be funded for up to five three-year periods.[2]

Since the funding program aims to promote interdisciplinary collaboration, an SFB consists of research groups from different scientific specialties. The occurrence of interdisciplinary collaborations is secured by the application of two rules: the principle of coherence and a collaboration norm. According to the principle of coherence, the SFB must choose its subject matter in a way that epistemic connections exist between the research groups. According to the collaboration norm, each research group must plan collaborative research projects and include them in their research proposals. Since the research groups belong to different specialties and since collaborative projects are the basis of funding, almost all proposals contain interdisciplinary research of some sort.

I investigated two SFBs in an interdisciplinary field in which biological, physical and chemical approaches to a common object were combined.

In addition to the main empirical case presented here, I will draw on data from a second study. In order to support the development of research profiles and to promote interdisciplinary collaboration at East German universities, the DFG funded specific collaborative research networks (Innovationskollegs) for a period of five years (Laudel and Valerius, 2001). The external funding was supposed to provide an initial boost for the network leading to an autocatalytic development of continued interdisciplinary research. The funding decisions were based on the same peer-review procedure as that used for SFBs.

Data and methods

The data on the peer-review process of SFBs was collected in 1994 to 1995[3] by applying the following methods:

1. Analysis of documents: This included documents of the DFG which contained the formal rules for

the assessment of SFBs. About 60 individual project proposals from each investigated SFB were analysed to understand the interdisciplinary nature of the project and to prepare the interviews. Furthermore, I studied the correspondence between the DFG's Head Office and the SFBs concerning the review process, minutes of the peer-review processes, and grant approval letters of the DFG.

2. Semi-structured interviews: I conducted interviews with five reviewers and two staff members of the DFG's Head Office specifically about the review process. The interviews with the reviewers took place shortly after one of the SFBs was reviewed, so that they were still able to remember the details of it. As part of the larger study, 50 principal investigators (among them seven whose proposal was rejected) and 49 research group members of the SFBs were interviewed. These interviews contained questions about the review processes and their outcomes. Many principal investigators were also experienced reviewers of other SFBs. Their (unsystematically) reported experiences were included, too.
3. Observations during one SFB peer-review process: These observations included the more 'public' parts of the discussions between the reviewers and the SFB members but not the actual panel decision-making.

The data about the second type of collaborative networks — the Innovationkollegs — were collected between 1998 and 2000. The same kind of documents were analysed and semi-structured interviews with principal investigators were conducted.

Results

The actor constellation

The actor constellation consists of the scientists from the research network; the delegates of the funding agency (the DFG); and the reviewers. Four delegates of the DFG are involved in the review process, among them two administrators and two members of the DFG's Committee for SFB Grants. One of the committee members is scientifically 'remote' and has the task of preventing epistemic coalitions and supporting comparisons between SFBs from different fields.

The final actor constellation already results from a negotiation process between the scientists who apply for funding and the funding agency. The DFG Head Office selects referees from the DFG's general 'pool' of reviewers and from a list of proposed reviewers submitted by the applicants. The prime criteria for the selection of reviewers are: competence; absence of obvious indicators for conflicts of interests (exclusion of former teachers/pupils, direct collaborators, direct competitors, etc); coverage of all specialties participating in an SFB; and achieving a mix of 'specialists' and 'generalists'. Competence is the most important criterion because in any peer-review process researchers expect to be judged by colleagues from their own field.[4] After the list of reviewers has been composed, the applicants may raise objections against proposed reviewers. This didn't occur in the two SFBs investigated, but in other cases has led to the removal of reviewers from the list.

In the end, an assessment panel of eight to 14 referees is composed, depending on the size of the SFB and the number of different specialties it contains. This group of reviewers accompanies 'its' SFB over the whole period of its existence, with the panel being partly renewed for every subsequent assessment. To keep reviewers over a long time is also a means to secure competence:

> Yes, you know at least a large part of the work … This is easier than entering a completely new field of which you don't understand anything. In this case, you must learn yourself first. And only then you can check the internal relations if they are logical and feasible. (a reviewer)

The assessment panel has a very powerful position because its recommendations about funding are rarely altered by the bodies of the funding agencies. Thus, in fact it makes the final decision.

It has become a standard practice in both SFBs to invite the referees to scientific meetings during the funding period. The referees actively took part in such meetings by discussing presentations by SFB members and by presenting their own research. This participation and the contacts with the applicants during the whole period, often over many years, led to the emergence of trust between the reviewers and the participants.

> The work is so fruitful that sometimes I feel in fact like a member of these SFBs … I don't feel like a distanced panel member because I know nearly everybody there and I know their works very well. (a reviewer)

Negotiation/knowledge construction process

Figure 1 shows the main steps of the review process. One of its unusual features is that the scientists whose proposals are assessed actively take part in five of the seven steps.

First, the process begins with negotiations about the actor constellation itself, that is, about the reviewers who take part (as described in the previous section).

Second the DFG programme officer assigns direct responsibility for about four to five SFB projects to each reviewer. Among these projects is one that lies at the margins or even outside of the reviewer's area of expertise. The reviewer receives the SFB research proposal, a list of questions which

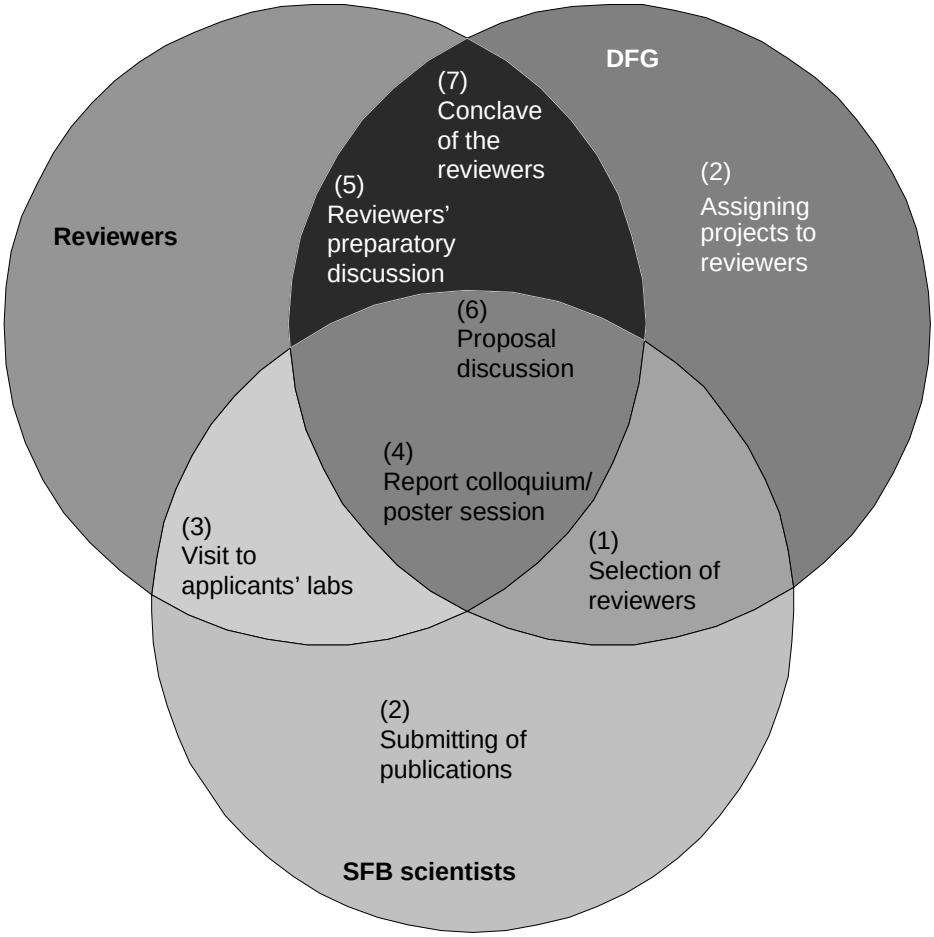

Figure 1. Procedure of a SFB assessment

contain the assessment criteria, and — in later funding periods — the SFB research report. The applicants may send publications to the reviewers.

It is expected that the referees make themselves familiar with all other projects of the SFB proposal, too. Indeed, all interviewed reviewers knew not only the projects assigned to them but had also read at least large parts of the whole SFB proposal.

> … even if this is always a lot of work, these fat books [of SFB proposals], you must read it all. You must understand it all, also the background, and assess it critically. (a reviewer)

The third and all following steps (the assessment itself) take place on the site of the SFB. The assessment procedure is not anonymous, it involves direct interactions between applicants and assessors. It relies on group discussions among the reviewers as well as between reviewers and applicants. The on-site assessment begins with visits to the laboratories. The reviewers visit the laboratories of 'their' projects in order to get an impression of the existing research equipment and to have first discussions with the applicants.

Fourth, the SFB now presents itself in a report colloquium and a poster session (each of which takes about three hours). In the report colloquium the SFB as a whole research network is scrutinised. The poster session is devoted to specific discussions of the projects of each research group. The reviewers are obliged to discuss the projects with the applicants, including projects that have not been assigned to them. The following quotes illustrate the approaches of reviewers with a specialised disciplinary perspective and of reviewers who lack detailed knowledge about a project:

> There were five or six [reviewers] who asked quite detailed questions. It was interesting to observe … depending on the research area of the reviewer, they asked completely different questions. Everybody asked something different. T was more interested in things he is concerned with, such as reflectivity, and particularly the new method. M of course stuck to the thermodynamics … The chemists were more interested in the chemistry. Only B, he is not so familiar with these kind of systems. And he just took the title of our poster and went over it, word by word and asked: "Now, what does this mean?" and "What does this mean?" This way he got to know everything about the project. That was clever. (an applicant)

> Being a reviewer does not necessarily mean that they are competent in this subject. But they let themselves be guided through the poster. Of

> course, the basic knowledge or background knowledge is there, but they need the specific questions to be explained to them. This is too difficult and too broad an area. (an applicant)

The applicants actively used this opportunity to explain their research goals to the reviewers and to justify their funding requests. This is also a chance for interdisciplinary learning by the reviewers. In the following example, the reviewer originally misjudged the labour intensity of a method. The applicant convinced him that he needs two PhD students in order to apply the method (as requested in the proposal):

> [The reviewer] came to ask me questions. He is going to write something about my proposal. And I told him that it is simply not possible to do it like this and it is useless with only one student. So I told him and he asked me whether I was threatening him. He didn't understand, I said: "No, I'm not, I just telling you that …" I mean, you can see the problem, it is a lot of work. This structure determination by NMR is not automated yet, it's not like X-ray where a lot of things are automated. What we do here we do manually. The spectrum is automatically run by the spectrometer and by a computer. But then you have a plot of this spectrum on the screen and everything is done manually. So the more people you have the faster you do it. If you get one student it takes about two years for a new student until he knows what he is doing and then he has one year to finish … So I have to work with at least two students, one is senior and one is new, a new guy who is trained by the other one. (an applicant)

Fifth, a preparatory discussion of the reviewers follows. This is the first assessment of the SFB and the individual research projects. A preliminary opinion on each project is formed. The way this discussion is carried out is similar to the conclave of the reviewers (see the detailed description of step 7). Finally, the reviewers formulate questions for the applicants.

Sixth, in the so-called "proposal discussion" between applicants and reviewers these questions are answered. The applicants must openly defend their projects if reviewers raise objections against them. A longer passage from a transcribed observation record is included to illustrate this discussion. A reviewer voices doubts about whether an expensive device is really necessary. The applicants try to provide the reviewers with convincing scientific reasons for their request. Chief Investigator P intends to apply an advanced microscopical method to his biological object which has not previously been tried in this context. The difficulty for him is that he cannot refer to published work but must refer to experimental work done elsewhere in the world and to work done in his own lab, the latter showing the unsuitability of other methods.

> *Reviewer* [microscopy specialist]: In project B2, a laser scanning microscope is requested. The panel wonders what exactly you plan to do with this equipment … Does it really give you what you hope to get? I don't believe that you can resolve that highly … Another question is: How important is this instrument for the SFB? Because there is already one in [chief investigator] D's group.
>
> *Chief Investigator P*: Our idea is that you take a normal network and you put a few filaments in it which are fluorescence-marked. And these filaments, you can excellently track three-dimensionally. J [a researcher at Harvard] shows that it works (unfortunately we forgot to show you that) … It is not possible to do this with the vesicles, which are a bit dynamic, in D's group. You need to get deeper into dynamic image processing.
>
> *Chief Investigator D*: I would like to support this … [Our instrument] is optically outstanding for stationary objects … But it is indeed not suitable for P's work.
>
> *Reviewer* [microscopy specialist]: I disagree with you. We've carefully checked the resolution, we tested it experimentally. It is a crucial argument that it has a lower aperture … As long as you cannot resolve single filaments in a 3D network, I am not convinced of its advantage. The best would be to do cryo-tomography …
>
> *Chief Investigator P*: May I add a point to that? We did a diploma thesis about this. If you freeze things for using cryo-electron microscopy you have surface tensions that you never get rid of.

Seventh, the conclave of the reviewers is the final assessment of the SFB proposal. Assessments are made at both the level of single projects and the level of the whole research network. The reviewers who were assigned to a certain project discuss the project first, before the whole panel becomes involved. Usually a lively discussion among four or five reviewers develops. The moderating DFG programme officer ensures that the discussion is not dominated by individual reviewers.

Each project is discussed extensively in order to gather all aspects and to form a shared opinion about it. Only in exceptional cases the reviewers vote. If there is only a narrow majority, then the discussion will be continued.

> Well, there is a lot of talking about each project. And this is probably good. I often think: My god why don't we come to an end. But you must talk a lot to each other to avoid misunderstandings. Well, you need an incredible

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> redundancy … There is so much that you might understand wrongly. (a reviewer)

> It is often the case — and I have reviewed many SFBs — that you, well, arrive at the assessment with a somehow preconceived opinion. That's somehow natural because you've read the proposal and got a picture about it, so that you must on-site sometimes revise your opinion. This is very often the case. That's why I am now, after long experiences, quite open. I get a picture about the projects but in fact the reviewer discussion is the opinion-forming discussion. I had also one or two cases where I changed my mind really radically. (a reviewer)

The reviewers get a list of questions, containing the assessment criteria. On the level of the single projects, these are the usual criteria: the quality of the proposal (originality and feasibility); are the applicants qualified to conduct the project, etc. Additionally, some questions are directly related to the interdisciplinary collaboration. Is the project strongly connected with the collaborative research centre as a whole, or could it be considered marginal? To what extent is this project necessary for other projects? What means of collaboration are planned with other projects (DFG, 2003)?

Since the idea of interdisciplinary collaboration in SFBs has been related to applying methods from one field to problems of other fields, the reviewers have tended to promote methodical innovations even when the successful application of a method was still vague.

> There were new groups which had only very short proposals. And these short proposals were sometimes not very elaborated but they involved some modern methods. And this was presented to us on the first morning. This let us think that it brought something technologically new and might make it possible to develop concepts further … If the man was good and the method seemed to be relevant for solving biological problems then this was okay. (a reviewer)

In these cases, the reviewers compromised on the criteria "quality of the proposal" and "feasibility" and put emphasis on the "quality of the researcher" or on the embeddedness in the overall network of the SFB, as is also expressed in the following quote:

> I've heard this so and so often that colleagues in the panel discussion said: In the Individual Grant Scheme, the project would have no chance. But it was so and so often a wrong argument, because you must of course see that the project is embedded into two, three other projects and then perhaps might help these other projects, or that they even rely on it, or that it is dragged along by these other projects. (a reviewer)

In other cases, the reviewers liked the project but were suspicious of the "quality of the researcher" criterion because the applying scientist stemmed from the "wrong" field:

> In the first period, they [the reviewers] wanted to polish it off because a physicist should do something physical and nothing molecular biological. But I had a good "assistant" in the reviewer panel. I had to defend it and he said, this is good what I planned and we should try it, for three years. And after three years we had success and this was established. Although — when I came here [to another SFB], in the new reviewer panel, there were again people who said, P is a physicist so he shouldn't do that. (an applicant, physicist)

Although the project was finally funded, the reviewer's leap of faith was initially limited since the combined specialties were quite unusual. In the oral communication, the scientist could convince the reviewers about the quality of the proposal and show that he had enough background knowledge to conduct the project.

Most problems occurred with the assessment of a project's feasibility; this criterion was sometimes replaced by evaluating the general scientific abilities of the applicant (track record and trust). Occasionally, the reviewers additionally used second-order criteria such as the number of publications or citations.

Projects comprising of more than one research field were evaluated by dividing the project along the boundaries of the specialties. For example, a biochemist had to assess the biochemical aspects of a mainly biophysical project. All interviewed reviewers stated that they tend to be reluctant with statements about really 'remote' projects. In this case they had primarily a meta-function in asking questions about the relevance of the project for the interdisciplinary topic of the SFB.

> As for thematically remote projects, there I am a bit cautious, honestly speaking. Actually, I use the whole thing quite selfishly; I let myself be taught. You can easily be carried away and

> possibly misjudge the importance of something and talk nonsense. It rather happens that you ask what role does this play for the whole SFB, or how is this linked to the biologically relevant projects. (a reviewer)

Besides assessing the single projects and their integration into researching the network, the SFB is assessed as a whole: how well all projects are connected; if additionally specialists are required in order to conduct research in the SFB's subject area; if the SFB is innovative; how the collaboration developed; etc. Since many reviewers evaluate more than one SFB, they are able to make comparisons between different SFBs. Unfortunately a detailed description of the interdisciplinary assessment at the macro-level could not be obtained because the interviewees were not able to reconstruct these processes with the required precision.

The whole procedure lasts two full days. Although the interviewed reviewers stated the time-consuming nature of the review process, none of them perceived it as a particular problem. Rather than being distracted from their own research work, they stressed the scientific gains and the research character of the communications during the on-site assessment:

> That's why I like to go there, even if it is always a lot of work. And this work you usually only do if you get something positive from it. And the positive is that you learn something; just as background knowledge for your own work it is important … This way you learn more than if you read these journals. You know how much is published each year. Thus, you get more in such presentations and discussions. (a reviewer)

Outcomes

In spite of their close ties to 'their' SFBs, the reviewers make hard decisions. Not all SFBs get funded. When an SFB is approved for funding, the reviewers' recommendations usually contain cuts to the original proposal. In every extension period some projects are not funded, particularly if their quality or their interdisciplinary connection to other SFB projects is considered to be insufficient. Most projects receive less money than they applied for; sometimes considerably less if funding requests of personnel were not approved. This is also governed by a general expectation of the DFG to cut funding because the budget is tight. The results of the assessment consist of reviewers' recommendations for funding which are generally followed by the decision-making bodies of the DFG.[5] In the observed SFB assessment the funding was reduced to about two thirds of the sum the SFB requested in its proposal.

The SFB is shaped by the reviewers in various ways. Firstly, the review process is anticipated and the applicants take into account the way in which a reviewer might look at their project. Thus, the "invisible hand of the unknown reviewer" co-writes the project proposal. Secondly and more directly, the reviewers shape projects by making decisions about the funding of personnel and research equipment, which affect topics that can be addressed, methods that can be applied, etc.

Reviewers not only shape the projects through their financial conditions, they also check whether a method or the result produced with this method is useful for other projects (from other fields). Furthermore, they detect structural gaps in the SFB's composition and they may suggest including certain methods, specialists or strengthening certain areas such as protein chemistry.

Finally, the reviewers also contribute their ideas in the interactions with applicants. As a result of the intimate knowledge of the SFB's work, even collaborations between reviewers and applicants occurred frequently. Usually, these are weaker forms of collaboration such as the exchange of substances and ideas.

> [Reviewer] M is somebody who is not only exceptionally well informed but is very collaborative as well. You can discuss with him very well; he really helps you. But he can also ask very uncomfortable questions. (an applicant, describing his discussion with a reviewer during the poster session)

> However, I am often interested myself in what they do. And why they do it this way and not differently. You also wonder how to tackle similar problems yourself with your own doctoral students. You learn from that. Sometimes you give them a hint by saying: "Do it this way, not that way", or "Have you thought about doing this?" This happens during the talks; it does not become "DFG-official". And you get to know what particular people do, what scientific past they have. Then you invite them sometimes to give a presentation [at your university]. (a reviewer)

While there are still grounds for contesting the reviewers' judgements, the most likely argument against the funding recommendations (lack of understanding on the side of the reviewers due to the interdisciplinary character of the proposal) did not occur. None of the interviewed SFB scientists complained about insufficient competencies of the reviewers (not even scientists whose proposals were rejected). Reviewers as well as principal investigators who had experience with peer-review procedures of other funding schemes stated that the procedure works well and leads to fair decisions.[6] When asked about disciplinary coalitions, reviewers declared that fierce discussions occur but that they were never shaped by fights along disciplinary boundaries.

It can be assumed that both the process of selecting reviewers and the way interactions between reviewers and applicants were designed secured the necessary competence that is required for interdisciplinary peer review. However, certain limitations of the procedure became apparent in a third assessment.

This assessment took place in the framework of the DFG funding programme "Innovationskollegs". While this research network was assessed by applying the same procedure as for the SFB funding programme, an important difference in the assessment criteria could be observed. For a variety of reasons that had to do with the situation in East German universities, the DFG encouraged a more 'risky' approach by approving unusual combinations of fields and more risky research programmes. The research network in question consisted of a combination of fields that were new in Germany. Therefore, the reviewers had limited expertise in this new area of transgenic animals. They decided to fund the network nevertheless. After the first funding period of three years, the network was assessed again because a decision about the continuation of funding was needed. The relation between the network's subject and the reviewers' expertise hadn't changed. National reviewers for the specialty of transgenic animals were still lacking. The network had not yet produced many results because it took a long time to breed the transgenic animals. However, preliminary results indicated a breakthrough in the field.

> We … experienced a big surprise. And this turned out only in the end of the first funding period. Unfortunately, this big surprise wasn't enough to convince the jury to continue because they didn't believe us. They said: "This cannot be!" One reviewer explicitly said he doesn't believe that this has been overlooked for such a long time.

However, the results existed only in the form of experimental data. Publications were submitted but had not yet been judged by the scientific community. Being unable to judge the content of the research, the reviewers resorted to second-order criteria such as publications, which did not exist yet because of the dynamics of the research.

> It simply took more time. [The methods] take their time … [The reviewers] had liked hard results or publications … Because it hadn't been published — it had not been accepted by the scientific community — they didn't believe us. We could project nice data at the wall with slides. They didn't want to accept it. They would only accept a paper and this will come — it isn't out even now … Thus, even now we don't have the paper out because simply too many things must be checked. It is necessary to check things, to secure them twice. All that hadn't happened.

When trying to decide if the described procedure enables a successful evaluation of interdisciplinary research networks, how do we measure the success of a peer-review procedure?

A significant number of high quality publications came out only after the peer review had taken place. By then, it had already been decided to cancel the funding of the research network.

Discussion

When trying to decide if the described procedure enables a successful evaluation of interdisciplinary research networks, we face a methodological problem: How do we measure the success of a peer-review procedure? The criterion that can be derived from the rationale for applying peer review is that success means selecting the best proposals for funding. However, it is a matter of opinion which proposals are the best, with scientists being heavily biased in favour of their own proposals. Another criterion of success would therefore be the agreement of all parties — successful and unsuccessful applicants, reviewers, officers of the funding agency and funders — that the best proposals are funded, that the procedure is fair, and that the assessments are competent. But even this agreement is suspect because the problems inherent to the peer-review process — idiosyncracies, power imbalances, personal agendas, etc — are well established.

Therefore, the most realistic (and empirically testable) criterion of success is *whether the specific additional strain put on the peer-review process by the interdisciplinarity of the proposals and the multidisciplinarity of the panel could be compensated for by the procedure*. In the framework of actor-centred institutionalism, we must establish to what extent the specific institutional conditions (the rules of the game) and other conditions of action could compensate for the impact of specific epistemic conditions (the heterogeneity of knowledge involved) on the actor constellation.

Figure 2 lists the major conditions of action that characterise the specific peer-review procedure described in the previous section:

1. The participation of applicants in the selection of reviewers made sure that all aspects of the interdisciplinary work could competently be assessed while the funding agency made sure that obvious conflicts of interests and cognitive particularism were reduced.[7]

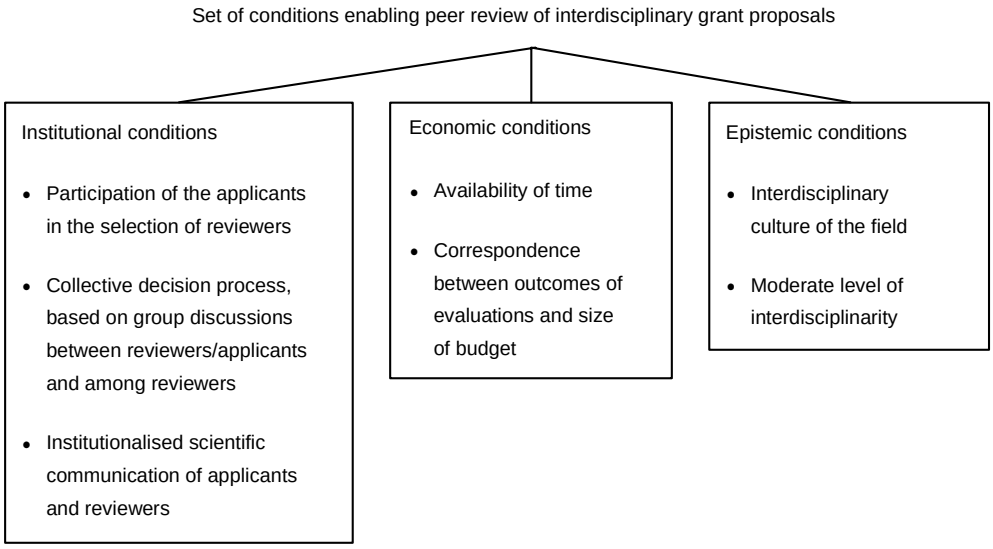

Figure 2. Conditions which secure the success of the SFB peer review

2. The core practice of the assessment process is the group discussion, both between reviewers and applicants and among reviewers. A significant amount of time is set aside for these discussions. The reviewers spend two days on-site. Yet, these group discussions are demanding, because the reviewers must establish an interdisciplinary communication, that is, learning the language of the other specialties, to a certain extent.
3. The process is embedded in the everyday scientific communication of applicants and reviewers, who conduct scientific discussions at the workshops and conferences of the SFB.

A first important impact of the institutional conditions on the actor constellation is the relative empowerment of the applicants and the corresponding relative limitation of power of the reviewers. This is achieved by the following elements of the procedure:

- The right of applicants to propose reviewers and to veto against them prior to the procedure;
- The non-anonymous procedure that implies that the performance of the reviewers is subject to public scrutiny;
- The framing of the review process as a scientific debate in which objections to projects or against the SFB as a whole must be given the form of a scientific argument, which can be publicly countered by the applicants on several occasions. This does not prevent the misuse of power by a reviewer in the final conclave. However, the strategic arsenal of the reviewers is somewhat limited because by then the whole panel knows most of the objections and the applicants' counter-claims.

A second important effect of the procedure is that it is heavily focused on reviewer competence. Reviewers are obliged to learn about the projects, and are not allowed to insist on their narrow perspectives. The participation of scientifically remote reviewers and of neutral members of the grants committee counteracts cognitive particularism. The selection of reviewers on the basis of their competence for the subject of the specific SFB and the system of rules that enforces scientific discussions between applicants and reviewers regardless of their respective specialties counteracts 'split judgements', that is, several reviewers commenting on the part of a project they understand without achieving a synthesis.

Both group discussions and the participation of reviewers in scientific meetings of the SFB create the common background knowledge that is necessary for successful interdisciplinary communication. Topics of these communications included the progress of ongoing and the design of future research projects. In this way, *competence trust* (Newell and Swan, 2000) (ie applicants having trust in the competence of reviewers), as well as *social trust* (ie trust in the correct behaviour of all actors), emerge. The closeness of the communication style to normal scientific life contributes to the social acceptance of the procedure and legitimates it in the eyes of the applicants.

Finally, a crucial condition is the amount of money available in the funding program. At the time of the investigation, all SFBs that were considered to be worth funding could actually be funded. This condition strengthened the scientific character of the whole procedure because the reviewers did not have to act against their scientific judgement.

Certain elements of the SFB procedure can be found in other peer-review procedures, too. For example, group discussions between reviewers occur in panel-based review processes; or assessment of research organisations sometimes include discussions with the participants. It is the *combination* of the elements outlined above which secure the functioning of the interdisciplinary peer-review procedure.

The described strengths of the analysed process are inevitably linked to two weaknesses. Firstly, the process is both expensive and time-consuming.

Therefore, it seems applicable only to rather important funding decisions. Secondly, the emphasis on a common basis for communication and consensus requires a 'moderate' interdisciplinarity. Since a fine-grained typology of interdisciplinary research is still lacking, I will present only a generalised description of the characteristics of the investigated interdisciplinary culture.

The field of both SFBs is characterised by an interdisciplinary culture that has emerged as a result of decades of successful interdisciplinary research, in this case the use of methods and objects from different specialties. Scientists from different specialties (including the reviewers) have already developed a general ability to communicate with each other. They have learned to ask the right questions and to present research results in a simplified way that is accessible to colleagues from other fields. Interdisciplinary approaches are primarily seen as opportunities to produce new knowledge and gain the benefit of the doubt. The groups have a shared research culture, namely the research culture of the experimental sciences.

Because of this 'moderate' interdisciplinarity, no special assessment criteria were necessary. This is not surprising because any produced knowledge requires some consistency with regard to the knowledge that has been used in its production. If second-order criteria were used, then they were part of a peer-review assessment procedure. Since the main decision-making process was a group discussion, it prevented such an approach.

Conclusions

The few studies of interdisciplinary research processes that address assessment problems tend to favour the introduction of special assessment criteria. For example, Klein has suggested introducing special assessment criteria for interdisciplinary research, such as "how well they integrate knowledge" (Klein, 1996: 211). Boix Mansilla (in this volume) found that scientists interviewed by her applied specific epistemic criteria for interdisciplinary research, which the authors summarised as:

1. the consistency with multiple separate disciplinary antecedents;
2. the balance in weaving perspectives together; and
3. the effectiveness in advancing understanding through the integration of disciplinary views.

However, my empirical study has demonstrated that, under certain conditions, successful interdisciplinary peer review is possible without special criteria. The key points of the procedure for Sonderforschungsbereiche assessments are the relative empowerment of the applicants and the enforced learning of the reviewers. This is achieved in an expensive and time-consuming procedure, which can be applied only to decisions about long-term funding of big projects such as research networks. However, the advantage of keeping traditional criteria is that the assessments can be kept comparable, and that the legitimacy of interdisciplinary research is not endangered by creating special standards for its assessment.

The problem of peer competence is not limited to the assessment of research that is commonly perceived as interdisciplinary. Interdisciplinary research has been defined in this paper as research that combines knowledge from different scientific specialties. Since specialisation continues to increase and occurs within specialties, it seems justified to extend the concept of interdisciplinarity to any combination of knowledge that exceeds the specialisation of a single researcher, thus becoming a continuous variable that describes the heterogeneity of knowledge combined in a research process. The tensions between applicants' and reviewers' specialisations can be assumed to affect not only reviews of grant proposals and publications that are explicitly labelled as interdisciplinary, but also many of those usually regarded as disciplinary. The assessment of interdisciplinary research provides an excellent object for studying this type of tension because it represents an extreme case of mismatch between the expertise of researchers and assessors.

An important methodological problem for this kind of study is that the ways in which reviewers form their opinion about a proposal need to be investigated in *statu nascendi* (in the state of emergence), which has been rarely achieved by science studies. Interviewing reviewers means investigating their reconstructions, which are bound to be selective and affected by their opinion on a subject. For future studies it appears to be crucial to apply fine-grained methods, such as ethnographic observations, in order to find out what reviewers actually do when they are assessing their colleagues' work, and how their practices vary. The rarity of observational studies of peer-review processes is due to the general reluctance by funding agencies and assessors to grant access to their 'black box'. Science studies could probably contribute much more to the design of peer-review procedures if the black box was opened.

Notes

1. A counter-example has been provided by Rinia and colleagues who investigated the assessment of physics programmes at Dutch universities. The peer judgements, made by international panels, were compared to the degree of interdisciplinarity of each programme, measured as a percentage of non-physics publications (Rinia *et al*, 2001). No bias against programmes with a higher degree of interdisciplinarity was found.
2. The DFG has since extended the length of the funding period to four years and reduced the overall time-span of funding to 12 years for each network.
3. One could argue that the data are relatively old and that the review procedure has changed in the meantime (slight

changes have indeed occurred; see note 5). However, it does not change the causal relationship that was found between a certain institutional form of peer review (as it existed then) and the success of the procedure.

4. In a survey about the NSF peer review, applicants voiced criticism about the qualifications of reviewers (Chubin and Hackett, 1990: 65–69). This is an implicit confirmation that most of all researchers expect to be judged by their peers.
5. This was at least the case at the time of my empirical investigation when there was not a disastrous competition for funding from the SFB programme, a situation which has recently changed. The DFG has to cope with an enormous amount of funding proposals and has therefore shifted decisions on proposals into processes before and after the on-site peer review (Wissenschaftsrat, 2002: 12–16).
6. An independent evaluation has shown that the funding programme for SFBs is regarded as successful by science policy (Wissenschaftsrat, 1998).
7. While other funding agencies sometimes allow their applicants to suggest reviewers who should be avoided, they rarely have an active suggestion procedure. However, the latter seems to secure the competence that is a key element in the functioning of interdisciplinary peer review.

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