Research Evaluation Advance Access published February 15, 2012

Research Evaluation (2012) pp. 1–13 doi:10.1093/reseval/rvs001

Conservatism and risk-taking in peer review: Emerging ERC practices

Terttu Luukkonen*

The Research Institute of the Finnish Economy, Lönnrotinkatu 4 B, FIN-00120 Helsinki
**Corresponding author. Email: terttu.luukkonen@etla.fi.*

This article explores the newly founded European Research Council's (ERC) peer review system and its ability to sustain its mission to promote excellent, groundbreaking research. The article explores the extent to which the selection of groundbreaking research is constrained by inherent limitations in peer review by analysing the informal practices of ERC peer reviewers. This article notes that controversy and uncertainty are central characteristics of potentially groundbreaking research proposals. The selection of truly innovative research is constrained by the boundaries on current knowledge, against which the value of proposed research is judged; these boundaries affect the extent to which peer review panellists feel they can take risks in their judgments and the rules of interpretation and deliberation they adopt. The role of customary interpretative rules is to limit the risks involved in decision making. Predicting the outcomes of peer review in controversial situations is difficult, however, as contingent factors play an important role.

Keywords: peer review, groundbreaking research, research funding, the ERC.

1. Introduction

This article presents the initial findings of a study on peer review, which is the core mechanism of the European Research Council (ERC), the recently established European funding body implementing the Ideas Programme in the Seventh Framework Programme. The ERC supports investigator-driven, bottom-up research in any field of research aiming to ensure 'that funds are channelled into new and promising areas of research with a greater degree of flexibility'.[1] The study was motivated by an interest in the ability of the ERC's peer review system to sustain its mission to promote excellent, groundbreaking research by European scholars. The mission statement of the ERC defines its objective 'to stimulate scientific excellence by supporting and encouraging the very best, truly creative scientists, scholars and engineers to be adventurous and take risks in their research. The scientists are encouraged to go beyond established frontiers of knowledge and the boundaries of disciplines'.[2] Living up to this mission is decisive for building and maintaining the legitimacy of the ERC (Luukkonen 2010).

The ERC is not alone in its objective to support groundbreaking research. Other research funders, such as the National Science Foundation and the Howard Hughes Medical Institute (HHMI) in the USA as well as many other European research councils, aim to promote truly innovative research. Variants of peer review are most often used by funding agencies or councils to select research worthy of support. The ability of peer review mechanisms to select truly innovative and groundbreaking research is an especially pertinent question for funders targeting this kind of research. However, insofar as much of the research on peer review draws the conclusion that peer review has an inherent conservative bias, these funders face a challenge.

This study examines the general question of the potentially inherent conservatism in peer review based on the particular peer review system of the ERC, which is geared towards selecting both excellent and groundbreaking research proposals. The study focuses on unpacking the peer review practices of the ERC and analysing them to discover whether there are processes that hinder or promote the selection of groundbreaking and excellent research proposals. This article draws attention to the meanings that peer reviewers give to the evaluation criteria, the ways in which they negotiate the relative merits of excellent versus groundbreaking research proposals, and the considerations and constraints they take into account when evaluating proposals. These considerations and constraints provide interpretative rules that help peer review panellists apply the ERC general criteria to specific and concrete cases. In a way, panellists act as moderators who may intervene and affect—in positive or negative ways—the selection of groundbreaking proposals.

To frame this study, the article first reviews findings from previous research on peer review, particularly regarding claims of its inherent conservatism. It then discusses the ways in which groundbreaking research might be defined conceptually and continues on to report results from the empirical study on the ERC's peer review practices. Note that this study is exploratory by nature and not based on hypotheses.

2. Definition of the research task

One of this study's central questions concerns, on the one hand, the customary and interpretative rules and practices that help peer reviewers apply general principles to concrete evaluation situations and, on the other, the extent to which these rules and practices promote conservative, uncontroversial research rather than groundbreaking and potentially controversial research, or vice versa, during the ERC's peer review processes. The article thus focuses on discovering what factors enable, enhance, or hinder the selection of groundbreaking research proposals and the criteria and interpretative devices that frame such decisions. The ways in which the panellists solve controversies around these decisions is particularly pertinent.

This article first examines reviewers' understandings of 'excellence' and 'frontier research'. The notion of excellence refers to quality requirements concerning scientific and scholarly research. The degree to which this notion is related to, or equated with, groundbreaking research by ERC peer reviewers is an important question, as it reveals whether they aim to select proposals that represent more than merely excellent research. Furthermore, the reviewers need interpretative rules when judging the nature of research proposals in terms of their excellence or novelty and when weighing various considerations against each other. The study further considers 'customary rules of deliberation', which was a concept first discussed by Lamont (2009) to indicate rules that facilitate agreement. This article asks what kinds of rules the ERC peer review panels apply in this context. The degree to which these rules promote or hinder the chances of groundbreaking proposals in the evaluation process is also discussed.

The term 'peer reviewers' refers to the chairs and members of the ERC's expert panels.[3] In their considerations during the second stage of the evaluation process, the panels also use reviews submitted by external experts, but these experts are not included in this study. Rather, the focus is on the panellists because they are involved throughout the selection process. Furthermore, individual external reviewers may only have one proposal to review, which gives them a limited view of the entire selection process. Additionally, they do not see how their own review is treated in the deliberations of the panel.

This study examines the peer review processes of both the Starting and Advanced Grants schemes and, with the exception of a few specific references, does not differentiate between the reviews of either grant scheme in terms of the groundbreaking nature of the proposed research. Applicants to the Starting Grants scheme present greater uncertainty as to their ability to lead research teams due to their lack of experience relative to the applicants to the Advanced Grants scheme; for this reason, starting with the implementation of the 2011 Work Programme, the applicants have been divided into two groups of 'starters' and 'consolidators', which are evaluated separately. However, the essential nature of research evaluation remains the same for both groups.

This study is exploratory and open-ended, and it is based on accounts of the panellists regarding the evaluation process. Because of its exploratory nature, it does not formulate specific assumptions about the degree to which the ERC peer review process does indeed promote controversial or groundbreaking research proposals. For instance, the role that the assessment of risk and feasibility play in the evaluation process, as described in the empirical findings of the article, emerged from the interview data and was not related to any hypothesis set in advance. Nevertheless, it was known that the ERC specifically highlights the importance of supporting groundbreaking research in its guidelines and that this is important for the legitimacy of the funding scheme. It is thus expected that the peer review system reflects this central aim in its design. It is further expected that the interpretative rules applied by the peer reviewers are related to the promotion of this objective.

3. Previous research on peer review

A strong strand in the research on peer review focuses on the way in which research traditions, personal commitments, and other interests affect peer review, essentially resulting in a conservative bias (Chubin and Hackett 1990; Travis and Collins 1991). Chubin and Hackett (1990, p. 62) suggested that cronyism and scientific feuds influence the outcomes of peer review. They further reported surveys with scientists (Chubin and Hackett 1990: p. 66) showing that scientists generally suspect that peer reviewers are reluctant to support unorthodox or high-risk research. In a related study, Travis and Collins (1991) differentiated cognitive from institutional particularism, where institutional similarity can lead to reviewers to prefer proposals from a similar—or similar type of—institution. Institutional particularism can unjustly discriminate against individual proposers, but according to Travis and Collins, cognitive particularism is prone to affect the progress of science overall. Cognitive particularism or cronyism is the same phenomenon as 'old boyism', but in a cognitive register. Cognitive similarity has implications for the cognitive development of research, for example, when the reviewers have incongruent cognitive perspective and then judge a proposal negatively or, alternatively, when they share cognitive similarities and unduly favour the proposal. Interdisciplinary research, frontier science, controversial research, and risky departures from standard approaches are more likely to suffer from cognitive cronyism than mainstream research.

An alternative approach in peer review research looks at peer review from the perspective of organizational practice (Langfeldt 2001; Heinze 2008). According to Langfeldt (2006: 33), an emphasis on meeting established standards or thoroughness in peer review may promote uncontroversial and safe projects because such peer review uses a larger number of peers as reviewers, thus increasing the likelihood that one of them is sceptical. Furthermore, tough competition among the proposals leads to a situation in which a consensus among the experts is an important prerequisite for a proposal to be ranked high on the priority list of projects to be funded.

Langfeldt (2001) also noted that the way in which the peer review is organized can have greater influence on the outcomes than the actual evaluation criteria. According to her findings, original and controversial projects have better chances in situations where budgets are ample and rating scales rough, while their chances are smaller when the research budgets are small and the rating scales are fine-grained. Rough ratings produce several proposals with identical scores, and this factor, together with ample budgets, enables the panels to consider policy objectives as additional selection criteria. Original and controversial research could be one of such additional criteria. According to these findings, peer review does not necessarily have a conservative bias once it is organized appropriately.

A third body of research on peer review is represented by Lamont (2009), who takes a pragmatic approach to studying the creation of trust through problem-solving, dialogue, and learning, in addition to the cognitive aspects of evaluation. Her studies indicated that despite different disciplinary evaluative cultures, formal procedures, and criteria of evaluation, evaluation panellists together develop shared so-called customary rules of deliberation that facilitate agreement and help avoid situations of conflict (Lamont 2009: 6). In this line of reasoning, questions of whether and the degree to which peer review upholds cognitive particularism or conservatism are not relevant, as this approach is aimed at uncovering how the peer review panellists deliberate and achieve agreement.

With the exception of Lamont's approach (see also Lamont and Huutoniemi 2011) and the above study by Langfeldt (2001), the majority of the research on peer review concludes that it is inherently conservative and unable to select truly innovative research proposals (Chubin and Hackett 1990; Braben 2004; Langfeldt and Kyvik 2010). Braben (2004: 70) even goes so far as to maintain that 'the natural inclination to oppose major challenges to the status quo has become institutionalized' in peer review.

Langfeldt's (2001, 2006) studies on organizational practice and Lamont's (2009) approach, however, paint a more nuanced picture. First, the outcome of the peer review process is related to the ways in which peer review has been organized and to the goals and instructions given to the peer reviewers. As above, there are many variants in peer review and, thus, also in its outcomes. Further research would be needed to draw more definite conclusions about the relationship between the organization and outcomes of peer review. Nevertheless, it is important to acknowledge that systems of peer review vary. Lamont has to some extent opened up the black box by looking at the customary rules that peer review panels use to achieve consensus. The degree to which these rules enhance or hinder the influence of cognitive and institutional cronyism on the evaluation outcomes is the focus of the present study.

4. What is groundbreaking research?

4.1 Scientific revolutions and other discontinuities

The ERC uses several terms to describe its mission in research funding: *excellence, adventurousness*, *taking risks,* and *going beyond established frontiers of knowledge and boundaries of disciplines*. All of these terms, except for excellence, are usually used to denote groundbreaking[4] or innovative research, with innovative meaning truly novel research.

Theories of scientific revolutions implicitly deal with what constitutes groundbreaking research insofar as they describe what happens when a profound discontinuity—that is, revolution—takes place. One of the most well-known theories of scientific revolutions is Kuhn's (1970) theory, which uses the notion of paradigm shift to describe revolution. According to Kuhn (1970: 175), a paradigm is 'the entire constellations of beliefs, values, techniques, and so on, shared by the members of a given community'. A scientific revolution emerges when there is 'a growing sense . . . that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way' (Kuhn 1970: 92). This inadequacy is a crisis that leads to the adoption of a new paradigm. The paradigm shift is seen as a revolution by those whose paradigms are affected by it. A paradigm shift essentially entails looking at the empirical data from a different angle and viewpoint, which may result in complete reinterpretation.

These Kuhnian notions have been criticized for overemphasizing the discontinuities in scientific thought (Toulmin 1972; Hacking 1985; Kusch 1991: 103), for it has been highlighted that changes in science are in fact more evolutionary by nature and do not affect all the strata of scientific research traditions (Laudan 1977). Without going too deeply into these discussions among the various theoreticians about their understandings of scientific discontinuities or revolutions, it is essential to note that this kind of profound change can occur more or less gradually. Furthermore, it does not affect all aspects of the scientific and scholarly enterprise at the same time; while methods, techniques, or even research problems may stay the same, there may be a profound change in the ways in which scientific problems are solved or approached over time (Laudan 1977: 140). Even Kuhn maintains that a paradigm shift can occur after a longer period of accumulation of anomalies that cannot be explained within the prevailing paradigm.

Another notable idea from these discussions on scientific change and revolution is that a paradigm shift is not brought about by planned action but by the accumulation of anomalous observations or problems that cannot be solved by the prevailing paradigm's scientific method of problem-solving. The outcomes and implications of a revolutionary change are not necessarily observable to those immediately involved in the debates and conflicting viewpoints surrounding that change. One of the implications for peer review is that during a period in which a paradigm shift is occurring, there are multiple scientific contenders who support highly variable viewpoints, which will make the achievement of consensus in peer review more difficult. The outcome is not necessarily a conservative bias if peer reviewers are selected by taking different scientific traditions into account. The same situation involving variant knowledge claims applies to most social sciences and humanities, since these disciplines generally do not share common paradigms about the phenomena under study (or, for that matter, what the appropriate phenomena to study are).

Examples of groundbreaking advances in science are not related solely to conceptual advances, however. The history of science provides ample evidence of methodological or instrumental advances that open up whole new research areas or fields, with profound impacts on applications as well. The invention of the scanning tunnelling microscope, which is an instrument for imaging surfaces at the atomic level, in 1981, and its further development through the invention of the atomic force microscope in 1986 (OECD 2010) were fundamental to the development of the research and technological area known as nanotechnology.

Another type of potentially revolutionary advance is the recent discovery of graphene, the one-atom-thick crystal with unusual quantum conductive properties by Konstantin Novoselov and Andre Geim, the 2010 Nobel Prize winners in physics. This is an example of a new substance that requires a great deal of basic research because its extraction is not yet stabilized. Moreover, it is an example of a groundbreaking discovery that may cause as yet unfathomable impacts in many areas including a number of future applications in electronics and photonics.

It may be argued that the concept of a paradigm is not universally applicable to all scientific and scholarly fields. At the very least, the idealized type of paradigm change that Kuhn describes applies to traditional scientific fields such as physics. In general, it is important to note that different types of phenomena constitute groundbreaking discoveries or discontinuities across different scientific and scholarly fields. There is a need to elaborate on field-to-field differences in the nature of groundbreaking research both conceptually and empirically. Although it acknowledges the importance of a comparative, cross-disciplinary approach, this study focuses on more general features of the peer review process as it relates to groundbreaking research and thus pays attention to differences across fields only to a very limited degree. This article represents a first exploration into the research problem at hand; as such, the empirical data so far collected are too small in quantity to offer evidence-based conclusions regarding field-to-field differences.

4.2 Research in peer review and groundbreaking research

Researchers who have studied peer review often refer to groundbreaking or unconventional research by terms that emphasize risk, with this risk implying that the outcome of a potential research project is highly uncertain. The following list provides additional terms that have been used to describe research that is not mainstream or conventional:

- Frontier science, risky new departures, and areas of controversy (Travis and Collins 1991);
- adventurous, innovative, novel, risky, and speculative (Grant and Allen 1999);
- groundbreaking, risky, unconventional, outside-the-box, speculative, and original (Heinze 2008);
- new, original, and surprising (applied to creativity in scientific research) (Heinze et al., 2009); and
- innovative, exotic, and risky, with interdisciplinary research falling into this latter category because it is perceived as risky (Laudel 2006).

Terms such as those above characterize new openings and opportunities where there might be a paradigm shift or a revolutionary change that, nevertheless, is uncertain. They also allude to research conducted in an unconventional way and the risks involved therein. Interdisciplinary research often falls by definition into the category of risky or unconventional research (Laudel 2006), and not surprisingly, peer review in the context of interdisciplinary research areas has attracted attention (Lamont 2009; Huutoniemi 2010). Laudel (2006) elaborated on the nature of risky research, and based on her study of experimental physicists, she defined risky research as new ideas 'where one is unsure whether it will work' (*ibid*., p. 9).

These characterizations share an emphasis on situations that are conducive to controversy. Still, controversy can exist in many forms, and not all controversy is related to issues of revolutionary change. A deep change, such as a scientific revolution, is typically an *ex-post* characterization of change that has already occurred. However, almost any novel aspect of research or any novel research finding may be contested; in these cases, the debate need not involve revolutionary change. Novelty and progress are fundamental values in scientific and scholarly research, and scientists and scholars have therefore developed criteria and principles through which they can assess whether and the degree to which proposed or completed research represents progress. In the following empirical study, peer reviewers' understandings of what constitutes excellence are to a large extent related to a set of such criteria. However, in the evaluation of proposed research, there are new uncertainties, as the work has yet to be performed. Therefore, peer reviewers apply further rules, here called interpretative rules, which help them to apply general evaluation criteria to concrete cases. The ways in which peer review panellists deliberate and achieve agreement is also highly germane to an enquiry into peer review processes and the evaluation of groundbreaking research.

5. Features of the ERC peer review process

The Ideas Programme, which has been implemented by the ERC, is the first European funding programme set up to support investigator-driven, bottom-up 'frontier research' in all fields of science, including the social sciences and the humanities (Luukkonen 2010). The budget of the ERC in 2007–2013 is 7,5 billion EUR. The ERC has been delegated a lot of autonomy in the selection of its strategy and in implementation. It has a Scientific Council, the members of which - representatives of the scientific and scholarly communities - are selected by a Selection Committee, consisting of eminent scientists and scholars. The Scientific Council members are chosen purely on their scientific and scholarly merits, and they do not represent Member States, specific organisations or interest groups. The ERC has designed a thorough peer review system for proposal selection. The guidelines for the evaluation are slightly modified from year to year and the procedure as described here refers to the system in place in 2010. The material for this study was obtained from the call evaluations in 2010. Each applicant to its two major grant schemes[5]—Starting Grants and Advanced Grants—is reviewed in two stages, with the panel meeting in both stages. In each stage, applications are reviewed by three members of one of the 25 panels per call. Each panel has 12–16 members. In stage one, only the extended synopsis of the research proposal is evaluated, while in stage two, both the synopsis and the full research proposal are evaluated. In this second stage, the panel members evaluate the proposal again, as do members of other domain-specific panels if the proposal is related to their domains. In addition, the synopsis and the proposal are sent to six or seven external reviewers, but, according to the interviewed experts, the panel usually receives only two or three of these external remote reviews due to time pressures. Typically, a proposal has six reviews in the second step. All of the reviews contain numerical scores as well as a detailed written review of the proposal. The domain-specific panels meet and discuss the reviews by the panellists and external reviewers and come to a conclusion. In 2010 a specific procedure was reserved for clearly interdisciplinary proposals. If such proposals passed the threshold for the potentially supported projects, they were examined and deliberated upon by a panel comprised of the chairpersons of the domain-specific panels; 13% of funds were reserved for such interdisciplinary proposals.

The evaluation criteria, as expressed in the evaluation guidelines, emphasize the groundbreaking nature of the proposal and the capacity of the Principal Investigator to fulfil his or her promises, as shown Table 1, which indicates the major criteria of selection. The merits of the Principal Investigator and the project plan are given equal weight in the evaluation.

The evaluation panels rank the proposals and effectively decide which will be funded. A notable feature of the system is that the Starting Grant applicants who are selected for the second stage are invited to Brussels to meet with the panel, where they first give an oral presentation of their planned research and are then interviewed by the panel members. These interviews are conducted following strict rules that provide equal time for each applicant to present their research and to answer the questions posed by the panel.

6. Empirical study of the ERC peer review practices: the data

This analysis is based on interviews with 20 chairs and/or members of 7 panels; 13 of the panellists were from Starting Grant panels, and 7 were from Advanced Grant panels (see Table 2). The interviews[6] followed a semi-structured scheme that enquired about the work-load, the role of the participants in the process, the rules followed at panel meetings, the role of the interview

Table 1. ERC selection criteria

Principal investigator	Project
Intellectual capacity and creativity: To what extent are the achievements and publications of the PI groundbreaking and demonstrative of independent creative thinking and capacity to go significantly beyond the state of the art?	Groundbreaking nature and potential impact of the research: To what extent does the proposed research address important challenges at the frontiers of the field(s) addressed? To what extent does it have suitably ambitious objectives, which go substantially beyond the current state of the art (e.g., including inter- and trans-disciplinary developments and novel or unconventional concepts and/or approaches)?
Commitment: Is the Principal Investigator strongly committed to the project and willing to devote a significant amount of time to it?	Methodology: To what extent does the possibility of a major breakthrough with an impact beyond a specific research domain/discipline justify any highly novel and/or unconventional methodologies ('high-gain/high-risk balance')? To what extent is the outlined scientific approach feasible? To what extent is the proposed research methodology (including the proposed timescales and resources) appropriate to achieve the goals of the project?

Source: ERC Work Programme 2010.
Note: Proposals will be evaluated for research environment on a 'pass/fail' basis and commented but not marked during the second step of the evaluation. The hosting research environment, that is, the immediate setting (like a Department) of the research team, will be evaluated as to whether it provides the infrastructure necessary for the research to be carried out, an appropriate intellectual environment, and infrastructural support; furthermore, the hosting research environment is evaluated as to whether it assists in achieving the ambitions of the project and the Principal Investigator.

in the funding process, and the interpretation of evaluation criteria. Some of the interviews were conducted in person, but the majority were conducted over the phone during the summer and autumn of 2010 (until the end of the year) as soon as it was possible to organize interviews after the 2010 call evaluation panels had completed the review process. All interviews were recorded and transcribed.

The procedure for selecting the panellists for the study was as follows. Six fields were chosen for the study, two from each of the main scientific and scholarly domains: life sciences, physical sciences and engineering, and social sciences and humanities. The chairs and three or four panel members from each of these panels were solicited for an interview. If a panel member declined to be interviewed or an interview date could not be arranged due to scheduling conflicts, another member was asked to participate in the study. The criteria for the selection of interviewees included representation from different countries and gender balance.

Forty-three panellists were contacted in total. The share of the panellists who were eventually interviewed out of those originally contacted was 47%; of those, 68% were Starting Grant panellists and 29% were Advanced Grant panellists. It is unknown why the Advanced Grant panellists were either busier or less willing to be interviewed for this study. One factor that may have played a role was the time period in which contact was made. As noted above, the interviews were conducted as soon as possible after the panels had completed their evaluations. For the Starting Grants, this meant late summer and early autumn, while for the Advanced Grants, it was late autumn and early winter, potentially a busier time of year. Furthermore,

Table 2. Interviewees by panel field

	Starting Grant	Advanced Grant	Total
Life sciences	4	3	7
Social sciences and humanities	5	4	11
Physical science and engineering	4		4
Total	13	7	20

there was a smaller share of interviewees from the physical sciences and engineering because the panellists from these areas were less willing to be interviewed than panellists from other areas. Their responses to solicitation revealed a disinclination to see the evaluation process as a topic of a scholarly study.

The picture provided by the peer review panellists is presumably affected by their preconceptions of what a thorough and fair peer review involves.[7] We should thus acknowledge the possibility that peer reviewers may provide a more idealistic picture of the process than is perhaps warranted. Careful wording of the interview questions may, however, address this issue by soliciting a more realistic view of the process.

In addition to panellist interview data, this study also draws on the official documentation on the ERC rules and principles for the evaluation process as well as additional interviews with three ERC Executive Agency officials involved in the design and coordination of the evaluation system. These additional data are intended to complement other data to develop a picture of the ERC evaluation process and its evolution. Not all of this material is utilized or mentioned specifically in this article, however, as this analysis focuses on the two major evaluation criteria—namely, groundbreaking research and excellence—and the ways in which peer reviewers interpret them.

This analysis builds on the themes and topics identified in the interview transcripts, and the structure of this article reflects the inductive analysis. Note that the important role of risks in the evaluation of groundbreaking research proposals is consistent with the previous literature described earlier.

7. Findings

7.1 The panellists' understandings of excellence and groundbreaking research

This study first attempted to explore the ways in which the panellists understood the concepts of excellence and frontier research. In the communications on the aims and mission of the new funding body, support of both excellence and frontier research is highlighted. The use of the concept of 'frontier research' in the connection with the ERC has a specific history. In this context, it means studies that can support both fundamental research and useful knowledge (Frontier Research 2005: p. 18). Moreover, the concept was used to justify the foundation of this funding body, which was in many ways based on principles different than those in the EU framework programme for research in general (Luukkonen 2010).

When interviewed, most of the ERC panellists preferred concepts such as path-breaking, groundbreaking, and cutting-edge research to frontier research, which was generally perceived to be a politically loaded concept. As indicated by Table 1, the ERC guidelines use the term groundbreaking.

The study thus pursued the ways in which the panellists understand excellence, excellent research proposals, and groundbreaking research, the degree to which those concepts were perceived to be overlapping or distinct, and whether broad fields of science and scholarly research see a difference between excellent research and groundbreaking research.

Out of the 20 interviewed panellists, nearly half (9) thought that there was a distinct difference between excellence and frontier research and proposals characterized as such. Five thought that there was no difference, and six thought that there was some difference. Excellence typically entails originality, novelty, and going beyond the current state of the art, which implies that it makes a difference for the development of science, and possibly also for applications, and that the research problem is important. In that sense, it is the same as groundbreaking research and, moreover, is actually in line with the official guidelines of the ERC evaluation procedure. However, excellence also involves the robustness of the research, the methodological rigour, the use of up-to-date methodology, and a clear and coherent discussion of the research problem and research purpose.

Frontier research was not necessarily regarded as excellent in terms of rigour, coherence, and so forth. When panellists perceived a clear difference between excellence and frontier or groundbreaking research, they highlighted that excellence relates to the methodological rigour and solid quality of the research, while frontier or groundbreaking research was emphasized as original and novel. Some panellists mentioned the paradigm-shifting and revolutionary nature of frontier research, and overall, it was pointed out that frontier research had a great impact on the development of science, e.g., by opening up new horizons and enabling new research directions. Some interviewees used special descriptors, such as a click or fantastic, to characterize the feeling when one reads a research proposal that is indicative of frontier research. In other words, there is something groundbreaking about these studies that is immediately understood, but which is not so easy to define. There is novelty; the applicant proposes to do something that has not been done before, or something that opens up a new way to look at the research area. In the same vein, a panellist described the recognition of an excellent research proposal by saying that 'you can just tell when you read it'.

A few panellists equated frontier or groundbreaking with interdisciplinary research, while most of the interviewed panellists did not see a direct connection between the two. The former view was justified by the fact that interdisciplinary research combines features (that is, theories, viewpoints, methods, or data) from more than one established research area, which automatically brings novel features to research.

Those representing engineering and, to a lesser degree, other applied sciences emphasized that applications and impacts on technology and society were part of excellence. In their view, excellence was not only about scientific progress but also about the impact of science on society.

Among the interviewees, the humanists perceived the least difference between excellence and frontier research. Both entail novelty and opening up a new and unexpected perspective. The reason why the humanities panellists saw the least difference could be related to the nature of their fields as pre-paradigmatic in the Kuhnian sense, meaning that there are multiple avenues of progress and none of them affects the whole field decisively in the way that a paradigm shift could in the natural sciences. Furthermore, representatives of the sciences and engineering were more inclined to use quantitative measures of excellence, such as bibliometric indicators, to judge the excellence of the applicant, and they discussed much more than the humanities panellists the numerical marks that panellists and external reviewers gave to the applicants and the research plans. These views are not necessarily generalizable, however, due to the small number of interviews conducted.

7.2 Interpretative rules

The panels take pains to select both excellent and frontier or groundbreaking proposals, where, if there is a difference, the former emphasizes the methodological robustness of the planned research. There are, however, additional considerations that the panellists take into account when judging the value of the proposals, especially in their attempts to separate promising groundbreaking proposals from those less likely to deliver. These considerations are here regarded as interpretative rules that help the panellists apply the general criteria to particular cases. These rules have evolved out of the general research and evaluation practices of the peer reviewers and are customary in the sense of Lamont's (2009) 'customary rules of deliberation'.

7.2.1 Overarching concerns: feasibility and risks. An important interpretative rule that acts as an excluding principle involves a proposal's feasibility and its associated risks. Feasibility and risks were mentioned spontaneously throughout the interviews in response to several questions. Even in the context of defining frontier research and proposals, panellists highlighted that pure speculation is not frontier research and that feasibility is one of the aspects to which attention they pay when judging a frontier proposal.

Feasibility can entail many things. First of all, it is related to the capabilities of the Principal Investigator who conducts and leads the research, particularly their abilities to apply up-to-date methodologies, use the required instruments, and carry out the research within the proposed time frame. Especially if the Principal Investigator is a young researcher, a lack of evidence that the Principal Investigator has the required capability to conduct the planned research would lead to a situation in which the project may be judged to be too risky for funding. Capability is usually judged based on the CV and the publication list of the applicant. The interviews with Starting Grant applicants also explore this dimension.

Second, if the research plan is regarded as speculative or unrealistic (in other words, dilettantish or trendy), it is not judged to be fundable. For instance, a research plan may not take into account that planned experiments or other parts of the project could in fact take much more time than envisaged or that the available methods would not provide the kind of data the research plan requires. Thus, both the feasibility of the planned experiments and the appropriateness of the methods were factors cited by interviewees that could downgrade an otherwise excellent proposal or an excellent investigator.

Feasibility is also related to the risks that the research will not go as expected, even if the plan may seem feasible. An investigator has to be prepared for any contingency and have an alternative course of action in case the plan does not turn out as expected. One of an investigator's important capabilities is to foresee alternative scenarios of action if plan A fails. The interviews with the Starting Grant applicants who passed to the second stage of review explore this question, among other things. If a researcher has not submitted a contingency plan as part of his or her proposal and is unable to respond to such a query in the interview, his or her abilities are judged negatively. However, this does not imply that an investigator would obtain an overall negative decision solely based on a feasibility judgment; the final judgment is based on many considerations.

A further interpretative rule that helps in assessing the feasibility and risks entailed in a proposal concerns the proposal's relationship with and relatedness to previous research. In order for planned research to pose important and relevant questions, it must take into account the research tradition and research front both empirically and theoretically; it has to be placed in 'context', as described by some of the interviewed panellists. This contextualization is in fact part of the excellence criteria insofar as the current state of the art provides a basis for judging scientific progress. A credible research plan, if it aims to entail a leap forward, must begin with the current knowledge base, or it is seen as pure speculation and not considered serious. This is yet another consideration that aims to reduce risks in proposed research. Thus, a proposal must find a balance between ideas that are innovative and ideas that are so innovative as to be fanciful.

Another interpretative rule that helps in assessing feasibility concerns the planned use of instruments and equipment. First and foremost, such equipment must be available to the investigator, and the applicant must convince the reviewer that he or she has the specific capabilities to pursue this line of research and experimentation, perhaps by drawing on unique capabilities not found elsewhere. A research plan involving the use of expensive instrumentation limits the possibilities of trying something truly new, and without a proof of feasibility, truly novel ideas are rejected. In many areas, the use of fairly large scale and expensive instrumentation involves the major activities of a whole lab, especially if the application concerns an Advanced Grant. Although the grant, in principle, does not cover all lab activities, in practice it is closely interconnected with the major research lines in a lab. For example, a grant can enable the pursuit of a research line that continues previous experimentation, which can now be carried out more systematically and with a larger resource base. This implies that the proposed research cannot be radically different, and in this regard, there is a conservative element in the selection process. As a result, so-called 'Gyro Gearloose' researchers[8] are extremely rare.

The panellists said that in some of their funding decisions, they believed that the risk that the planned research objectives would not be achieved was quite high, but nevertheless, they consciously decided to take this risk. With high risk, there might be high gain, and this gain would involve funding an original idea for which the outcome could not necessarily be predicted.

7.2.2 *Interdisciplinary research and peer review.* As noted above, some panellists equated inter- or multi-disciplinary research with frontier or groundbreaking research by definition, although the majority of the panellists did not do so. Despite this perception of interdisciplinary research, such proposals bring new dynamics to the judgment of frontier research. There are no interpretative and customary rules about the judgment of interdisciplinary proposals, and according to the ERC peer review system, these proposals are subject to a separate evaluation according to the criteria of each discipline to which they are related (including methods and definition of phenomena) and are evaluated by experts within each of these related areas (Lamont 2009; Huutoniemi 2010). Thus, the boundaries, criteria, and considerations of each established disciplines still determine the outcome in the evaluation of interdisciplinary proposals.

We cannot conclude from this that interdisciplinary proposals are exposed to harsher criticism than single-disciplinary proposals or that they cannot be highly ranked. One panellist remarked that her panel retained the most highly ranked interdisciplinary proposals on its main ranking list to guarantee their funding, sending the less highly ranked interdisciplinary proposals to the interdisciplinary final panel consisting of the panel chairs. This implied that the panel sent the second-best interdisciplinary proposals to this pool. This is an interesting finding, and it indicates that the 'best' interdisciplinary proposals are judged as such according to the criteria valid in the domain areas of the respective panels. It is also an example of 'strategic' behaviour in the ranking of proposals (Lamont 2009), which shows that the panellists become committed to the proposals that they regard as the very best and worth funding. The prevalence of such strategic behaviour is a topic for further study.

The ERC allows a member of an evaluation panel to serve up to four times, the panel chairs only twice with an exceptional third round, and according to many panellists, by the second time together the panels had learned to get to know each other's rating scales, manner of argumentation and so on, and had thus created a joint 'culture'. Most panellists regarded their panel as being inter- or multi-disciplinary because each panel included disciplines or domains of science that were quite different, for example, in terms of their research tradition, quality standards, and the importance of theory in guiding research design. The boundaries of disciplines or fields, however, are fluid, and thus, the definition of what constitutes one's own discipline or field is not self-evident. Joint panel work and the creation of panel culture entailed learning and respecting the different standards, quality criteria, and ways of thinking in other disciplines or areas included in a panel. Lamont and Huutoniemi (2011) called this type of understanding 'respecting disciplinary sovereignty', and it is one of the customary rules adopted in peer review 'for sustaining collective belief in the fairness of peer review' (Lamont and Huutoniemi 2011: p. 12) and avoiding open conflicts within the panel.

7.3 Deliberation within the panel

7.3.1 *Resolving disagreements.* Langfeldt (2006) drew attention to the fact that a thorough peer review process involving several peers is liable to promote uncontroversial and safe projects. The ERC peer review process is quite thorough: in addition to three panel members, who (are expected to) read an application carefully and rate it while the other members read the application less thoroughly, all second-stage applications are subject to a variable number of additional external referees (of whom normally two to three respond and submit a review). Normally, six experts read a proposal and rate it. Will this number of peers promote the acceptance of uncontroversial projects?

Given our interview data, one cannot draw direct conclusions. First, the panellists repeatedly stated that an early consensus with respect to an application's marks and evaluation statements is not necessary for it to be positioned highly on the ranking list in the end. A situation often occurs in which one of the experts on the panel evaluates a proposal differently from the rest. The panellists who most diverge from the others clarify their points of divergence and discuss their grounds for judgment. During these discussions, the panellists often admit that they change their views, as they might have overlooked an important point or have been too lenient or too severe regarding a proposal. Disagreement can be over feasibility, but also over the value of different approaches, methods etc. Though it varies depending on the panel, this may infrequently resort to a vote with the main goal of ranking projects on the boundary area and/or on the reserve list or if the discussion would otherwise take too long. After the vote, the losing side accepts the point of view of the winning side. Many panellists talked about defeats and the necessity for panellists to accept defeats during these situations. A chair of a panel which used to vote on the proposals in the 'grey' area between proposals that will be funded and those that will not said that 'voting calms the different opinions and everyone accepts democratic voting'. Thus, behaviour patterns used in other social contexts, such as politics, seem to supply an additional rule of deliberation. We might note that in questions related to scientific expertise, controversy, and unconventional ideas, seemingly 'democratic' voting may not be the most appropriate procedure, as it may simply support the position of conventional viewpoints and prevent heterodox ideas from gaining support. This conclusion, however, is only tentative; and even though it is difficult to study, it requires further research.

The panels devoted the most time to discussing the ranking of the proposals that were near the boundary line between being funded and not, and the biggest changes in position occurred in this ‘grey’ area. It could nevertheless happen, if relatively rarely, that a proposal originally ranked as among the top three or four proposals could experience a deep drop in its ranking.

7.3.2 *External reviewers.* The panellists deferred to one another’s expertise and disciplinary sovereignty in the decision process. The panels do not, however, include experts in all of the subject areas related to applications. Some panellists even went so far as to characterize the panel as a panel of generalists. Therefore, the use of external referees or reviewers plays an important if somewhat conflicting, and debatable, role in the evaluation of applications. The external reviewers, who evaluate applications remotely, are chosen for the specific expertise they bring with respect to the areas covered by the applications. They, in principle, represent the body of expertise most relevant to the applications, and as such, their comments can carry a great deal of weight in the panel deliberations. However, these external reviewers are not present at the meeting to defend their viewpoints if they are contested by the panellists; the panellists believed that this fact implied that the views of the external reviewers in practice carried less weight. Furthermore, because they are not present at the meetings where the panellists discuss their markings and achieve consensus in their numerical assessments, the numerical marks given by the external experts are generally assumed to diverge from those used by the panel. Thanks to the panel culture, there was much tacit knowledge amongst the panellists that the external reviewers did not have. As such, the panellists have learned to pay less attention to the numerical marks of the external evaluators in favour of their written statements. The reports by the external reviewers were, furthermore, found to be of variable quality. Nevertheless, if external reviewers agreed in their assessments (and were perceived to be of high quality) these could carry more weight within the panel.

7.3.3 *Interviewing the Starting Grant applicants.* Another interesting question relates to the significance of the Starting Grant applicant interviews in funding selection. These interviews could decisively affect the ranking of Starting Grant applicants not only for reasons related to the applicant’s capabilities and his or her demonstrated mastery of the research topic but also in regards to the research plan. The interviews are a sort of test to check that the applicant prepared the proposal him- or herself and was not simply pursuing the research agenda of his or her supervisors. The purpose of the grant is to nurture the independent career of young researchers and ‘to support up-and-coming research leaders who are about to establish or consolidate a proper research team and to start conducting independent research in Europe’.[9]

7.3.4 *Summary.* To summarize, the panels use different means to achieve a consensus. The panels usually achieve a consensus through deliberation and by contesting one another’s arguments. Different panels, however, have slightly different styles regarding deliberation and their inclination to use voting or not. Customary rules noted in other contexts were also found here (Lamont 2009; Lamont and Huutoniemi 2011).

The ranking of a proposal might undergo significant shifts relative to other proposals during the evaluation process, and many factors could influence this. The most debated area is the ‘grey’ boundary area around the line demarcating the proposals that will be funded and those that will not (Cole et al., 1981). Often though not always, the topmost proposals enjoy an early consensus that they are highly excellent proposals. We might presume that such proposals do not contain evident risks and are not very controversial. By contrast, the proposals in the grey zone might be highly original, but they contain risks or other features that raise questions within the panel. What will tip such proposals to one or the other side of the boundary may depend on contingent factors. It should also be noted that the customary rule of deferring to expertise implies that the fate of a proposal can depend on the views of only one or two experts.

Whether such a situation would favour cronyism, cognitive, or institutional is an important question because cognitive cronyism hinders scientific progress, while institutional cronyism creates inequality (Chubin and Hackett 1990; Travis and Collins 1991).[10] The interviews for this study did not provide much material on scientific feuds or cronyism. Some interviewees mentioned that the panel had representatives from the two major research traditions in one of the two major disciplines represented in the panel, but there was no difference in the way that these members treated proposals or applied evaluation criteria. It seems that deferring to expertise also meant deferring to research traditions. However, a panel member from an engineering field admitted that the procedure of nominating external reviewers created a potential bias for fair peer review in that the panel members tended to choose experts they knew to be close to their own school of thought. This practice should not be regarded as free from bias. Furthermore, one panel chair spoke of reviewer luck, saying that a dominant panellist may ‘single-handedly kill a proposal or raise it quite a lot’. However, as the chair, he emphasized his role in preventing this from happening.

In principle, the collective debate within a panel is expected to prevent crude forms of cronyism from impacting evaluation outcomes, and the panellists are expected to check each other and contend with one another's judgments and claims. However, as the panels must cover a wide area of research, each specific area is represented by only a few experts. Thus, the actual, quite possibly heated, debates on the value of a particular proposal do not involve all members. Most often, they only involve those panellists that represent the specific research areas closest to the given application. The rules of deferring to expertise and respecting disciplinary sovereignty (Lamont and Huutoniemi 2011) mean that panellists whose area of expertise is more remote from the area of the application in question tended to respect the views of the panellists closest to the application, often accepting what the most expert panellists saw as the merits and weaknesses of an application as compared with the opinions of the more generalist panel members.

Similarly, if external reviewers agreed with each other (and had prepared their reviews carefully), the likelihood that they persuaded the panellists was very high. Thus, there were possibilities for cronyism to take effect despite the checks and balances brought into the system, but it is impossible to assess the extent with any accuracy.

8. Conclusions: supporting conventional or controversial proposals?

This analysis has shown that the major aim of the ERC's peer review system is to promote both excellent and groundbreaking research and to provide funding opportunities for researchers with a potential for such achievements. To achieve this aim, the ERC's peer review panels apply customary interpretative rules to help them apply the general criteria to actual cases, especially in the evaluation of groundbreaking research. These criteria were aimed at sorting out purely speculative proposals from those offering a more probable delivery of results.

To achieve consensus, the panels applied some of the customary rules of deliberation observed by Lamont (2009) and Lamont and Huutoniemi (2011). These included deferring to expertise and deferring to disciplinary sovereignty. Further rules were also applied, such as voting and the use of seemingly 'democratic' procedures customary in other spheres of life to solve otherwise difficult decision situations. From the point of view of controversial proposals, these rules of deliberation had the potential to provide opportunities for cronyism and/or conservative decisions by giving the greatest decision power to the panellists with expertise closest to the application or, in the case of voting, to the average opinion. The rationale for these rules lied in the expectation that the panellists less knowledgeable about an application's topic of research were seen to have greater difficulty in detecting unrealistic expectations in a proposal or whether a proposal did really represent a novel idea. Some of these issues are related to the estimation of risk, especially the justifiable degree of risk in decision-making. Making decisions to fund controversial projects necessarily presupposes risk-taking. The panellists were well aware of their responsibility of avoiding unjustifiable risks in recommending projects for public funding. Thus, controlling for risks was a major overarching aim in the evaluation criteria of potentially groundbreaking research proposals.

Many of the factors limiting the chances of controversial proposals could perhaps be characterized as related to Kuhn's (1970: 175) understanding of paradigms, where a paradigm is 'the entire constellations of beliefs, values, techniques, and so on, shared by the members of a given community'. We can also refer to Knorr Cetina's (1999) concept of epistemic cultures as a characterization of the totality of social and cognitive factors that legitimate and constrain the ways in which scientific research is evaluated and pursued. The relevance of the research plans, the research design, and the framing of the research questions are often defined within existing dominant paradigms or epistemic cultures. However, ERC-funded research is expected to go beyond these and break through current research frontiers or barriers between and across research fields. Still, the yardsticks that are available for the peer review panellists come from dominant belief systems and paradigms. Nevertheless, there is, admittedly, a strong goal of promoting research that goes beyond existing paradigms and belief systems and provokes important changes in present research lines, and this is reflected in the criteria for evaluating applications.

The feasibility requirement is one of the important interpretative rules that must be applied during evaluation, and feasibility is strongly linked to the prevailing paradigms and research traditions. Assessments of experiment feasibility might be valid, notwithstanding the general theories or paradigms maintained by the peer reviewers This is because in the context of paradigmatic change, the techniques, methods, and experiments may stay the same, while the interpretation changes. An exception would be situations in which profound change starts from the invention or development of radically new measurement techniques or devices.

An overall conclusion from the findings of this study is that despite the ERC's aims, the peer review process in some ways constrains the promotion of truly innovative research. These constraints arise from the very essence of peer review, namely, its basic function of judging the value of proposed research against current knowledge boundaries. However, this does not necessarily mean that peer review prevents new openings, especially if such an aim is a central evaluation criterion. The ability of the ERC panels to take great risks in funding is a further limiting factor. One of the central observations of this article is that the control and management of risks in decision-making, while always present when funding decisions are made, plays a major role in an agency that purposefully aims to fund groundbreaking research, which is by nature risky and controversial.

This paper has not discussed the relative value of strategies that could identify truly innovative and risky research through the selection of projects versus individual researchers. An example of the latter is provided by the HHMI, which selects individuals with exceptional capabilities and potential for exceptional future achievements (largely, however, based on their exceptional past achievements) (Nedeva 2011). The ERC's evaluation pays attention to the individual Principal Investigator and his or her abilities. Nevertheless, the selection is equally dependent on identifying proposals that promise innovative and groundbreaking research projects. If attention is paid only to the individual and funding is decided without a detailed research plan, the extent of risk-taking is far greater than in the system that the ERC has adopted. To some extent, the ERC's peer review system is a compromise approach that balances extreme risk-taking with a wish to support exceptional research and researchers.

Another related question is whether and the extent to which it is even possible to promote, plan, or design truly radical research in a purposeful way. We may, for example, question whether radical discoveries happen in a planned project or through serendipity. A good example is provided by the aforementioned discovery by Konstantin Novoselov, the 2010 Nobel Prize winner in physics and the current Starting Grant recipient, who experimented on graphene with his more senior colleague Andre Geim after working hours. This is an example of the role that serendipity plays in scientific discoveries. It is worthwhile noting that the discovery was made by individual investigators, one of whom was (before his Nobel Prize) identified by the ERC as having the potential (given his research plan) to make fundamental discoveries. Furthermore, he was selected in the first Starting Grant 2007 call which attracted an avalanche of over 9000 applications. The ERC did not provide the facilities originally leading to the discovery but now provides the researcher(s) with the freedom to pursue and develop their revolutionary ideas further.

Acknowledgements

This article draws on material collected as part of a study on peer review with Maria Nedeva of the University of Manchester, which is supported by Riksbankens Jubileumsfond, Sweden. Earlier versions of this article were presented at the workshop entitled 'Peer Review Practices and Risk-Taking in Funding Part-Breaking Research,' organized by Stiftelsen Riksbankens Jubileumsfond, 1 April 2011, and at the Atlanta Conference on Science and Innovation Policy, 15–17 September 2011. I am grateful to Chris Caswill, Thomas König, Maria Nedeva, Helga Nowotny, and two anonymous referees for valuable comments on this article.

Notes

1. <http://erc.europa.eu/mission>. The ERC started its activities as part of the Seventh Framework Programme (2007–2013).
2. <http://erc.europa.eu/index.cfm?fuseaction=page.display&topicID=12> accessed 14 March 2011. The term '"frontier research" was coined for ERC activities because they will be directed towards fundamental advances at and beyond the "frontier" of knowledge' <http://erc.europa.eu/mission> accessed 12 January 2012.
3. The ERC has 25 panels per call and two alternating panels per grant scheme, which makes a total of 100 panels. This figure does not include the experts for the Synergy Grant, which will consist of 5 highly interdisciplinary panels. Up till the end of 2011 the ERC has funded approximately 2500 grantees and has received and processed more than 26 000 applications (source: information from the ERC).
4. The potential differences between terms, such as groundbreaking, path-breaking, or frontier research, are ignored here. They all refer to something that breaks through existing knowledge boundaries and is highly innovative. Note that this article mostly uses the term groundbreaking to refer to this type of research.
5. The ERC Starting Independent Researcher Grants (ERC Starting Grants). The objective is to provide critical and adequate support to the independent careers of excellent researchers, whatever their nationality, located in or moving to the Member States and Associated countries, who are at the stage of starting or consolidating their own independent research team or, depending on the field, their independent research programme. The ERC Advanced Investigator Grants (ERC Advanced Grants). The objective is to encourage and support excellent, innovative investigator-initiated research projects by leading advanced investigators across the Member States and Associated countries. This funding stream complements the Starting Grant scheme by targeting the population of researchers who have already established themselves as being independent research leaders in their own right. Source: ERC Work Programme 2010. As of 2011, the ERC has an ERC Proof of Concept scheme, which is implemented through a Coordination and Support Action, that provides additional funding to establish proof of concept and identify a development path and an intellectual property rights (IPR) strategy for ideas arising from an ERC-funded project. The objective is to provide funds to enable ERC-funded ideas to be brought to a pre-demonstration stage where potential commercialisation opportunities have been identified. Furthermore, as of the 2012 Work Programme, the

ERC Synergy Grant will enable a small group of Principal Investigators and their teams to bring together complementary skills, knowledge, and resources in new ways to jointly address research problems.

6. The interviews were carried out by the author of this article and Dr Maria Nedeva, who is the partner on the project.
7. Originally, the aim was to observe peer review panels in action; however, the European Commission's data protection rules did not allow for such observation.
8. A fictional character, a creation of the Walt Disney Company, and a friend of Donald Duck, Scrooge and other characters associated with them. He is Duckburg's most famous and exceptionally prolific inventor, whose inventions do not always work the way he wants them to.
9. <http://erc.europa.eu/starting-grants> accessed 20 December 2011.
10. The ERC has elaborate rules to prevent conflicts of interest (CoI), and if an expert is in a CoI with a proposal s/he cannot participate in the evaluation in any way and has even to leave the room when detailed discussions or decisions are taking place on such a proposal. These rules define conflicts of interest as significant collaborative, conflictual or ongoing mentor/mentee relationship plus other close ties (family, personal, financial, administrative, collegial). Cognitive or institutional cronyism is a grey area not easily registered by formal procedures.

References

Braben, D. W. (2004) *Pioneering Research: A risk worth taking*. Hoboken, New Jersey: Wiley-Interscience.

Chubin, D. E. and Hackett, E. J. (1990) *Peerless Science: Peer Review and U.S. Science Policy*. Albany: State University of New York Press.

Cole, S., Cole, J.R. and Simon, G.A. (1981) 'Chance and Consensus in Peer Review', *Science*, 214: 881–885.

Grant, J. and Allen, L. (1999) 'Evaluating High Risk research: an Assessment of the Wellcome Trust's Sir Henry Wellcome Commemorative Awards for Innovative Research', *Research Evaluation*, 8: 201–204.

Hacking, I. (1985) 'Styles of Reasoning'. In: Rajchman, J. and West, C. (eds) *Post-Analytic Philosophy*, pp. 145–165. New York: Columbia University Press.

Heinze, T. (2008) 'How to Sponsor Ground-Breaking Research: a Comparison of Funding Schemes', *Science & Public Policy*, 35: 302–318.

Heinze, T., Shapira, P., Rogers, J. D. and Senker, J. M. (2009) 'Organizational and Institutional Influences on Creativity in Scientific Research', *Research Policy*, 38: 610–623.

Huutoniemi, K. (2010) 'Evaluating Interdisciplinary Research'. In: Frodeman, R., Klein, J. T. and Mitcham, C. (eds) *Oxford Handbook of Interdisciplinarity*, pp. 309–320. Oxford: Oxford University Press.

Frontier Research: The European Challenge. (2005) *High-Level Expert Group Report*. Brussels: European Commission.

Knorr, C. K. (1999) *Epistemic Cultures: How the Sciences make Knowledge*. Cambridge, MA: Harvard University Press.

Kuhn, T. S. (1970) *The Structure of Scientific Revolution*, 2nd edn, Enlarged, International Encyclopaedia of Unified Science, Vol. II, Nr 2. Chicago and London: The University of Chicago Press.

Kusch, M. (1991) *Foucault's Strata and Fields. An Investigation into Archaeological and Genealogical Science Studies*. Dordrecht/Boston/London: Kluwer Academic Publishers.

Lamont, M. (2009) *How Professors Think: Inside the Curious World of Academic Judgment*. Cambridge, MA: Harvard University Press.

Lamont, M. and Huutoniemi, K. (2011) 'Comparing Customary Rules of Fairness: Evidence of Evaluative Practices in Peer Review Panels. Peer Review Practices and Risk-taking in Funding Path-breaking Research'. Stockholm, Sweden: Research Workshop, 1 April 2011.

Langfeldt, L. (2001) 'The Decision-Making Constraints and Processes of Grant Peer Review, and their Effects on the Review Outcomes', *Social Studies of Science*, 31/6: 820–841.

——. (2006) 'The Policy Challenges of Peer Review: Managing Bias, Conflict of Interests and Interdisciplinary Assessments', *Research Evaluation*, 15/1: 31–41.

Langfeldt, L. and Kyvik, S. (2010) 'Researchers as Evaluators: Tasks, Tensions and Politics', *Higher Education*, published online 7 October 2010.

Laudan, L. (1977) *Progress and Its Problems: Towards a Theory of Scientific Growth*. London and Henley: Routledge and Kegan Paul.

Laudel, G. (2006) 'The Art of Getting Funded: how Scientists Adapt to their Funding Conditions', *Science and Public Policy*, 33/7: 489–504.

Luukkonen, T. (2010) 'The European Research Council in the European Research Area', <http://www.eurecia-erc.net/resource-centre/official-documents/> accessed 27 January 2012.

Nedeva, M. (2011) 'Selection Practices of Research Funding Organisations and Path-Breaking Research – Towards a Comparison', Manuscript.

OECD. (2010) *The Impacts of Nanotechnology on Companies, Policy Insights from Case Studies*. Paris: OECD.

Toulmin, S. (1972) *Human Understanding*, Vol. 1, General Introduction and Part I, Oxford: Clarendon Press.

Travis, G.D.L. and Collins, H.M. (1991) 'New Light on Old Boys: Cognitive and Institutional Particularism in the Peer Review System', *Science, Technology, & Human Values*, 16/3: 322–341.