Working Paper
Evaluation practices in the selection of ground-breaking research proposals

August 13, 2015

Terttu Luukkonen[1], Michael Stampfer[2], Michael Strassnig[3]

Abstract

The review empirically examines the proposal selection practices of seven funding organisations in schemes that explicitly aim to support ground-breaking research and focuses on the organisational practices of these funding organisations in their attempts to overcome the limitations and criticisms of conservatism in peer review, as copiously argued in debates on peer review. The review uses data from the websites of the relevant organisations and interviews with programme officers of each scheme and draws on the data that previous studies collected on some of these schemes.
The paper notes that, with one exception, the schemes use peer review but attempt to apply it in a manner to overcome some of its limitations by using specific evaluation criteria, designing panel composition, requiring sketch research plans and emphasising both ideas and people. Simultaneously, the applied procedures are somewhat elaborate and, in most cases, involve several phases. Furthermore, they are consensual and, thus, introduce elements that can hinder the selection of ground-breaking ideas. There is limited data on outcomes, but some of these data indicate that such schemes can succeed in promoting innovative and/or high-impact research.

Key words: peer review, proposal evaluation, ground-breaking research, funding organisations

1. Introduction

'Ground-breaking research' — or, alternatively, 'transformative' or 'frontier' research — is deemed to have profound impacts on science and, in the long run, society and the economy through radical innovation (e.g., European Commission 2005). This assessment has led both policy makers and scientists to be concerned about the present funding mechanisms and to question whether they are able to select potentially ground-breaking ideas. These concerns have been instigated by the fact that an increasing share of research resources derives from project-based funding that requires careful planning and may be detrimental to serendipitous discoveries. Consequently, some research funding organisations have launched specific schemes for ground-breaking research, and new funding arrangements, such as the European Research Council (ERC) and the Human Frontier Science Programme (HFSP), have been established.

A major challenge for funding organisations and funding programmes addressing ground-breaking research is overcoming the inherent limitations of peer review. Peer review typically denotes a review by generalists or specialist experts in the field of the proposal to be evaluated, that is, a review by people with competence within the area of the work to be evaluated.[4] In recent years, peer review has spread, and it has been modified into various forms of expert review in the impact assessment of research programmes, research funding and performing institutions, and national innovation systems. However, in the current paper, we limit our attention to the selection of research proposals.

The origins of peer review as a mechanism for allocating research funds can be traced to the construction of the US science support administration in the 1940s and 1950s, especially the NIH and NSF, although it was used as early as the 1930s in the National Advisory Cancer Council (Chubin and Hackett 1990). Doubts have frequently been expressed concerning the effectiveness and neutrality of peer review and its ability to select the best and most innovative proposals (e.g., Roy 1985; Chubin and Hackett 1990; Ismail et al. 2009; Nuffield Foundation, 2014). Questions concerning time lags, costs, outcomes, and impacts on the scientific community have been raised (Science 2014; 2015). Concerns about potential bias and problems in peer review, e.g., its fairness in selecting proposals from underprivileged groups and less well-known institutions or in making consistent selection decisions, have been raised as early as the 1970s and 1980s (e.g., Roy 1985) and addressed in studies (Cole et al. 1978; Cole et al. 1981; Travis and Collins 1991). Because public research funding organisations are especially faced with demands for accountability and effectiveness in their basic function — the selection of the research that is to be funded — they are keenly interested in and, in some cases, experiment with new methods to select and apply peer review.[5]

In recent years, a new strand of criticism of peer review has included a concern about the ability of peer review to select high-risk, exploratory research proposals (Wagner and Alexander 2013). This concern partially stems from the research literature on peer review that has claimed that peer reviewers are reluctant to support radical new departures for a number of reasons, including cronyism, scientific feuds, and institutional or cognitive particularism (old boyism) (Travis and Collins 1991). In the US in particular, Chubin and Hackett's book Peerless Science (1990) has been influential in noting gaps in the peer review process and concluding that research traditions, personal commitments, and other interests affect peer review, resulting in a conservative bias on panels that consider funding for grant proposals (Chubin and Hackett 1990).

In this review, we will analyse how research funding organisations address these challenges by examining the proposal selection practices of seven funding organisations or arrangements — the Howard Hughes Foundation (HHMI),ational Institutes of Health (NIH), National Science Foundation (NSF), Economic and Social Research Council (ESRC), Human Frontier Science Programme Organisation (HFSP), Volkswagen Foundation, and European Research Council (ERC) — in terms of schemes that explicitly aim to support ground-breaking research, although different terms may be used to describe the purposes of these schemes.[6]

We will focus on the organisational practices of these funding organisations in their attempts to overcome the limitations of peer review. We will use the findings of studies that attempt to assess the outcomes of these funding schemes (if such studies exist). Our purpose is to draw conclusions about the salient features that are part of the schemes that, by some criteria, are able to fund at least some ground-breaking proposals, as far as evidence is available, and the contextual factors that might be important for selection success.

2. What is ground-breaking research?

The concept of ground-breaking research owes much to Kuhn's (1962) concept of paradigm shifts (scientific revolutions), although there are many other types of profound discontinuities in science that are based on methodological or other discoveries (Luukkonen 2012). These discontinuities include the discovery of novel phenomena (serendipitously or otherwise), the creation of new methods or techniques that become enablers of genuinely novel measurements that lead to new branches of knowledge, access to new data (often but not only through the new methods), and the creation of general explanations, paradigmatic and other. In an interview study with Finnish and UK scientists,[7] Luukkonen (2014a) found that methods enabling radically novel departures in research were the most prominent category (over 60% of the interviewees), while the discovery of a new phenomenon (20%), new explanations (paradigms) (10%) and new data (4%) were less frequently considered by scientists. In the social sciences and humanities, ground-breaking research can take the shape of solving 'big questions' by using an exceptionally broad theoretical, methodological and/or empirical basis (Laudel and Gläser 2014).

Researchers in peer review use a variety of characterisations to describe ground-breaking research, such as highly innovative and original, risky[8], adventurous, and controversial (in the sense that scientists can have widely different views on the scientific value of the proposed research). Furthermore, they consider that the research/researchers evidence a high degree of creativity and that the results are often unexpected (Luukkonen 2012; Travis and Collins 1991; Grant and Allen 1999; Heinze 2008; Heinze et al. 2009; Laudel 2006).

Funding agencies and bodies use different terms to designate ground-breaking research. For example, the ERC uses the term '*frontier research*', which was originally suggested by a high-level expert group. Frontier research is defined as research that is at the forefront of creating new knowledge and developing new understandings, intrinsically risky, concerned with both new knowledge about the world and generating potentially useful knowledge, and cuts across established disciplinary boundaries (European Communities 2005).

A private charity, the Volkswagen Foundation, which is one of our examples, uses the term '*unconventional research'*. Furthermore, especially US funding agencies, but also the UK ESRC, use the term '*transformative*' research. The NSF's National Science Board defines transformative research as "research that has the capacity to revolutionize existing fields,

create new sub-fields, cause paradigm shifts, support discovery, and lead to radically new technologies." This type of research requires researchers to take high risks.[9] The UK ESRC scheme for the social sciences also employs the term 'transformative' and supports research that attracts an 'element of risk' — which seems novel for funding schemes in the social sciences.

Despite the different terms, funding organisations aim to support a roughly similar type of research that is expected to have profound consequences. The emphasis is on the impacts of ground-breaking research (which the term 'ground-breaking' also implies).

3. Policy rationales for promoting ground-breaking research

We can trace slightly different reasons for why ground-breaking etc. research has attracted attention from research funding organisations. For example, Wagner and Alexander (2013) reported that the trend in the US government in the 1990s and early 2000s to increase support for 'transformative' (exploratory, high-risk, high-reward) research "was partly motivated by a concern that peer-reviewed research results were biased against high risk or exploratory research" (Wagner and Alexander 2013, p. 188). The concerns were expressed at high levels in the governmental administration, for example, by the Office of Management, and the budget Memorandum of August 4, 2009 drew attention to increasing support for high-risk research (M-09-27; Wagner and Alexander 2013). Science studies and science policy analysis have provided some of the reasons for this trend (e.g., Chubin and Hackett 1990). As a consequence of critical studies on peer review, agencies have established exploratory schemes that use different methods to select funded projects.[10]

In the UK, a March 2006 discussion paper by several government departments voiced similar concerns about whether "the existing framework for supporting science and innovation enables an appropriate level of risk-taking, and if not, suggestions of how any gaps might be addressed" (Science and Innovation Investment Framework 2004-2014, 2006, p. 16). The concern stemmed from the realisation of the importance of interdisciplinary research for innovation. These concerns led to experimentation with new approaches to project selection, such as the Engineering and Physical Sciences Research Council's (ESPRC's) 'sandpit' residential workshops[11] and the ESRC's Pitch to Peer Workshops, which are described below.

The two concerns listed above highlight the impacts on the development of science and society when the funding systems do not support high-risk research at an appropriate level, whatever that level might be. These concerns have become stronger under conditions in which an increasing share of funding for university research derives from external sources and through planned projects that may leave less space for serendipitous discoveries or unconventional ideas.

The establishment of the ERC in 2007 provides an example of a case in which high-risk research, designated by the term 'frontier research', provided an important argument in

support of the funding of basic or fundamental research. In the debate on the need and legitimacy of the ERC, the term 'frontier research' was used in a manner similar to the long-standing uses of the concept of 'basic' or 'fundamental' research. The concept of fundamental research originally arose in fields with an explicit application orientation and with a promise of eventually producing useful knowledge, although in such a way as to manage expectations that may become far too high (Schauz 2014, p. 287; Godin 2006). The high-level expert group that the Commission set up in mid-2004 revived the old basic research concept under the term 'frontier research' (European Commission 2005: p. 18). The concept of frontier research was needed for the justification of the ERC, which was in many ways based on principles that differ from those in the EU framework programme for research in general (Luukkonen 2014b). This judgement is strengthened by the fact that in stabilising its position, the ERC has dropped the criterion of 'ground-breaking' research and only uses 'excellence' in its review guidelines for peer reviewers in the Horizon 2020 (8th framework programme running 2014-2020).

To summarise, the emergence of a concern with funding transformative, ground-breaking etc. research is based on many factors, including concerns that peer review selection mechanisms may not select exploratory and high-risk proposals, a general need to secure the renewal of the scientific knowledge base, and a belief in the long-term usefulness of basic research and the need to justify the funding of basic (fundamental) research. Public and private funders are clearly in a different position in terms of their needs to justify their choices to different audiences.

4. Research on peer review and the selection of ground-breaking research

Even in hindsight, it is not always easy to pinpoint the ground-breaking event in a particular research domain; it may take a long time for the impacts of this event to unfold, and these impacts may require further research and discoveries. The evaluation of research proposals *ex-ante* is especially difficult because the salient features of ground-breaking research include its unexpected, original, risky, and controversial nature. Furthermore, a funding organisation cannot expect that all or nearly all of the ground-breaking grants will lead to the expected results or impacts because of the risky and uncertain nature of the research in principle. For example, Wagner and Alexander (2013) claimed that the 10% success rate of the former NSF programme Small Grants for Exploratory Research (SGER), sponsored in the period of 1990-2006, was overly high, suggesting that NSF had been too risk averse in its programme support. However, because the ground-breaking nature of research results is not immediately evident, it raises the question of the appropriate time frame for assessing the success of a project — and what is labelled as 'success'.

Many studies on peer review have painted a negative picture of the ability of peer reviewers to select innovative and risky research proposals. Conservatism, as is maintained, is the

impact of cronyism and scientific schools of thought (Chubin and Hackett 1990, p. 62) as well as cognitive and institutional particularism in the selection process, whereby reviewers prefer proposals that are similar to their own work or come from organisations that are similar to their own organisations (Travis and Collins 1991). Reviewers have been observed to be reluctant to support unconventional research ideas or proposals that entail high risks of failure. These views are also in accord with the opinions of scientists, as indicated by surveys (Chubin and Hackett 1990). Aside from the studies by Cole et al. (1978; 1981), which used an experimental design, very few empirical studies exist. One of the few studies was conducted by Travis and Collins (1991), who used material drawn from the observation of grant-awarding committees of the UK Science and Engineering Research Council. An important portion of the criticism is based on individual observations and views.

However, there is a branch of research literature on peer review that focuses on the manner in which it is organised and the relationship between organisational practices and outcomes, including the support of controversial versus safe projects. Langfeldt (2006: 33) noted that attempts to be thorough in the evaluation process may be counterproductive because having a large number of reviewers will increase the likelihood that at least one of them is critical, and under conditions of tough competition, a consensus among the experts is required for a proposal to be ranked high on a short list of funded projects. There are also organisational studies that draw attention to programme characteristics and their relationship with outcomes (Heinze 2008).

Lamont (2009) and Lamont and Huutoniemi (2011) drew attention to the social, in addition to the cognitive, aspects of peer review, such as the creation of trust through problem solving, dialogue, and learning in interdisciplinary contexts. They especially elaborated the customary rules of deliberation that facilitate the achievement of consensus within a panel of evaluators. Luukkonen (2012) elaborated some of the customary rules and noted the importance of the degree to which the grant applications were judged feasible and whether the risks were justifiable. Her study concerned peer review processes in the 2010 calls of the ERC. She concluded that the ERC peer review process, despite its aims to promote frontier and ground-breaking research, constrained the promotion of truly innovative research. She stated that such limitation occurred because peer reviewers could only judge the value of proposed research against current knowledge boundaries and because, despite the fact that ground-breaking research was a central evaluation criterion, the ERC panels' ability to take great risks in funding was limited.

Current studies on peer review often attempt to assess the outcomes of the process. If the promotion of ground-breaking research is the major aim, then the studies aim to gauge the degree to which this aim has been achieved. Some studies use multiple methods and data sources, but bibliometrics is used most often and is an important element in most studies (see, e.g., Neufeld et al. 2013). Such studies have provided either unclear evidence of the *ex-ante* or *ex-post* performance of grant recipients or some evidence of higher performance after the grant (Lal et al. 2012; Azoulay et al. 2011). However, an underlying assumption in the use of bibliometrics in this connection is that highly influential and radically innovative research

becomes highly cited quickly and that highly innovative researchers consistently publish highly cited papers. The extent to which these assumptions are true cannot be established with the available evidence. A counter example is provided by, for instance, the phenomenon of 'Sleeping Beauties', i.e., discoveries that are ahead of their time and initially unnoticed — even up to thirty-four years in the case of Gregor Mendel's discoveries on plant genetics (1865; according to van Raan 2004) — or quickly dying scientific fashions. There is some evidence of the former, which has been found to be an infrequent phenomenon (van Raan 2004), but to date, there is no evidence of the frequency of the latter.

5. Questions in focus

Because peer review itself is questioned, our first interest in this review is whether and the degree to which funding schemes that are dedicated to supporting ground-breaking research use peer review as the basic selection mechanism. If they do not, then the question becomes what mechanisms research funding organisations employ to select projects for funding.

In principle, different combinations of panel review and external, remote reviewers are used in most peer review practices. In a panel review, the panel members read and deliberate on the merits and weaknesses of the proposals and make a ranking or rating of the proposals. In order for the panel members to arrive at a list of fundable projects, they typically must reach a consensus. As noted above, the requirement of consensus can be detrimental to supporting unconventional research ideas.

Remote reviewers alone or as a complement to a panel review are used to a great extent in peer review. Remote reviewers are typically selected to provide more specific expertise because a panel cannot be so large that it does not allow for debates and discussions. Thus, by definition, panellists are often generalist. When only remote reviewers are used, a party such as a programme officer or a committee, such as the Research Council, its subcommittee or an evaluation panel, must relate the separate reviews and their markings to each other and take into account their different assessments to arrive at a joint ranking list. In remote review, each reviewer typically has few proposals, sometimes even only one, to consider; thus, the reviewers do not have an opportunity to make broader comparisons among the proposals, which a panel can do.

If peer review is used, a relevant question concerns the type of changes or modifications that the funding bodies have made to avoid a potential conservative or other bias in peer review. To what extent do they use panel reviews or do they use remote reviewers as a complement? Do they use specific means to ensure that the reviewers provide recommendations to fund unconventional proposals? Is consensus required, and what are the means to achieving a consensus? We further presume that evaluation criteria are important and focus on them. Finally, the question of who makes the final selection or determines the final rank list is pertinent.

6. The data

This paper will focus on schemes with the stated aim to promote ground-breaking research, or research that uses related terms, such as frontier research, path-breaking research, transformative research, highly innovative, risky, or controversial research, or creativity. We sampled funding schemes in several countries and internationally and used the purpose of the scheme as the selection criterion. Interdisciplinarity is, by definition, not the same as ground-breaking research, and if the term 'interdisciplinary research' was the only term noted in the aims of the scheme, then it was not included in the review. However, some funding arrangements such as the ERC view interdisciplinary research as a corollary term to ground-breaking research. Furthermore, schemes to promote the careers of especially early stage researchers or to promote talent were not included unless they simultaneously emphasised some of the aspects of ground-breaking research as outlined above. In addition, a sole reference to "excellence" was also insufficient for inclusion in the case studies, despite the fact that the divide between excellence and frontier research is not clear-cut (cf. Luukkonen 2012).

The data were obtained through the websites of the relevant organisations, by interviewing one or two programme officers of each scheme, and using the data collected by Luukkonen (2012) on the ERC peer review[12] and further information on the US schemes from Maria Nedeva (University of Manchester).

The funding organisations and schemes for this review are listed in Table 1.

Table 1. List of the studied funding schemes

Funding organisation	Programme/scheme	Country/ public vs. private	Thematic coverage	Year of the establ. of the scheme
Howard Hughes Foundation (HHMI)	HHMI Investigator Program	US Private	Life Sciences	N/A
National Institutes of Health (NIH)	NIH Director's New Innovator Award	US Public	Life Sciences	2007
National Science Foundation (NSF)	EAGER Early-concept Grants for Exploratory Research	US Public	All fields	2009
Economic and Social Research Council (ESRC)	Transformative Research	UK Public	Social Sciences	2012/13
Human Frontier Science Programme Organisation (HFSP)	Human Frontier Science Programme (programme grants and young investigators) ("main scheme")	International, Intergovernmental, Public	Life Sciences	1989
Volkswagen Foundation	Freigeist Fellowships	Germany Private	All fields	2013
European Research Council (ERC)	Starting Grants and Advanced Grants	European Public	All fields	2007

The reviewed cases vary in terms of their organisational aspects. Three of the schemes are the major schemes of the respective organisations, specifically, the HHMI, HFSP, and ERC,

while the remainder — in particular, national public funding organisations — have a separate, often small, scheme for ground-breaking research. These small schemes incorporate higher levels of risk-taking than do the mainstream activities and aim to support frontier research as a way to promote the sustainability — continued renewal — of knowledge production (NIH, NSF, ESRC). Three of the schemes are intended for the life sciences, three are for all fields and one for the social sciences only. Two are private, while the rest are public. The private foundations (HHMI from the US and Volkswagen/Freigeist from Germany) have more leeway in defining their ambitions and practices than the public funding organisations. The HHMI channels most of its funding through the Investigator Program, which promotes frontier research. The Volkswagen Foundation is well known as a first mover and risk taker in the German funding landscape.

One of our cases is intergovernmental, and another case is an EU scheme. Some (in particular, HFSP and HHMI) have been well established and well embedded in the research system over many years, while others (Freigeist, ESRC transformative) are running pilots, with the ERC being a fairly recent (2007) addition to the EU Framework Programme for Research and Development (now Innovation).

It is important to highlight that evaluation processes evolve over time when a funding scheme is continued over several years. Thus, what we report here represents the current situation. In the case of the ERC, our data were collected in 2011 and concerned the 2010 call (Luukkonen 2012). The processes may, and are likely to, change (or have changed) when a funding scheme has been or will be in existence over a number of years.

The schemes (with the exception of the ESRC) typically give the applicants a free hand in the selection of their topics if they are appropriate in terms of the schemes, exploring 'unproven avenues, embracing the unknown', 'highly innovative' ideas and exceptionally creative investigators etc. The only restriction concerns the field of research in the schemes that are intended for specific fields. The only funding organisation that restricts topic selection is the ESRC, which states that 'transforming' social science applications must address one (or more) of the ESRC's three priority areas: 1) economic performance and sustainable growth, 2) influencing behaviour and informing interventions, and 3) a vibrant and fair society. However, these strategic priorities are defined widely enough to allow for a diversity of themes to be funded, although there is a potential fundamental tension between ground-breaking and mission-oriented schemes.

The schemes generally target early to mid-career researchers[13] who have the ability to pursue independent research. Researchers at these career stages are less well established as independent researchers than senior scientists and are most likely judged to be most in need of special support for unconventional research ideas. The schemes do not target very junior researchers because there must be some proof of previous achievements to reduce some of the risks inherent in highly innovative research. If expected results will not be reached, then engagement in more risky research at an early career stage might also have more serious implications for the career track of junior researchers than for the career track of well-

established researchers, and such considerations could play a role in funding organisations' design of schemes.

The US organisations (and the ESRC in its pilot of the scheme in 2012[14]) define which research organisations are eligible to have applicants obtain their funding. The ESRC further restricts the number of proposals per research organisation. Thus, in the ESRC calls, the research organisation plays a strong role in pre-selecting the applicants and assuming a — albeit moral — responsibility that the applicant as proposed by the research organisation is able to conduct the suggested research. The implication is that the universities and their hierarchies effectively define the ideas and researchers to be supported.[15]

Only the HFSPO aims to explicitly promote research collaboration, both new collaborations and 'novel forms of collaboration'. This is not surprising given that it is an intergovernmental organisation. All of the other schemes allow, but do not require, collaboration.

7. Findings

7.1. Peer review or alternatives?

First, we will focus on the use of peer review mechanisms in the selection process. Table 2 shows that peer review remains the major mechanism for selecting funded projects even in these schemes: only one scheme does not use peer review at all, and one scheme uses a rather unconventional form of it.

Table 2. Summary of the selection mechanisms used in the funding schemes

Funding organisation	Programme/scheme	Use of peer review
Howard Hughes Foundation (HHMI)	HHMI Investigator Program	Yes, elaborate
National Institutes of Health (NIH)	NIH Director's New Innovator Award	Yes
National Science Foundation (NSF)	EAGER Early-concept Grants for Exploratory Research	No; scientific officers
Economic and Social Research Council (ESRC)	'Transforming' Social Science - Transformative Research Call	No; however, peer group procedure
Human Frontier Science Programme Organisation (HFSP)	Human Frontier Science Programme (programme grants and young investigators) ("main scheme")	Yes
Volkswagen Foundation	Freigeist Fellowships	Yes
The European Research Council (ERC)	Starting Grants and Advanced Grants	Yes

The NSF EAGER scheme is intended for early explorations of new topics. The grant is small (max. $300 000) and intended for a two-year project at maximum. The review procedure

represents a clear non-peer review case: scientific officers make the selection, although in exceptional cases, they can solicit external reviews. The scientific officers have a scientific background and often a career in science. Nevertheless, the exclusive use of in-house scientific officers is not typically called peer review.

The UK ESRC transformative scheme applies the most atypical peer review procedure, called "Pitch to Peer Workshop" (see Box 1).

Box 1. ESRC "Pitch to Peer Workshop"

The ESRC does not employ classical remote review by sending the applications to external experts; rather, it organises a workshop that involves all shortlisted applicants and provides a forum where the proposals are discussed and rated by the applicants as peers (in two to three subgroups). The subgroups are supervised by members of the "Commissioning Panel", an expert panel invited by ESRC programme management, which shortlisted the proposals and invited the shortlisted applicants to a "Pitch to Peer Workshop".

During this workshop, the applicant Principal Investigators must be present. They are randomly split into three groups. Within these groups, each applicant has 7 minutes to present his/her idea, then the proposal is scored (1-10 points) by the other applicants that are in the groups and by some members of the Commissioning Panel. The participants' scores are taken into account in the decision making of the Commissioning Panel to inform decisions when the panel struggles to reach consensus.

This procedure combines remote review with a peer review panel and a hearing in one move. Yet, because the programme has operated for only a short period of time, the merits and problems of this process have not been evaluated. A comprehensive evaluation is planned after round 3, which will be completed by 2016.

The remaining schemes all use peer review, and most use an elaborate form of peer review.

7.2. Salient features of decision-making processes of ground-breaking research funding schemes

Table 3, which summarises the form of peer review that the funding organisations use for these schemes, shows that the peer review mechanisms used are all more or less conventional variants of peer review. However, these mechanisms are highly elaborate and have from two to several stages, and some panels use remote reviewers in addition to panels of experts who meet, debate, and justify their scoring. The NIH, the HFSP, and the ERC use remote reviewers. When remote reviewers are used, overall, the proposal is reviewed by 4-6 (or even more) experts. However, the HHMI, which does not use external remote reviewers, can have a total of 10 to 12 reviews in several rounds in each case.

Table 3. Peer review forms in the funding schemes

Funding organisation/scheme	Form of peer review	Peer Panel	Remote reviewers	Interviews/hearings
HHMI/ Investigator Program	Multiple steps: 1) internal review by SOs; 2) peer review	Yes	No	A brief presentation at a symposium attended

	by four HHMI investigators; 3) SOs match top-rated applications with current research portfolio and further reviewers within HHMI that work in the same area; 4) full review panel that is advisory; 5) institute top leadership makes final decision.			by HHMI scientific leadership and the final advisory panel.
NIH/New Innovator AWARD	1) SRG checks first; 2) external mail review; 3) review panel; 4) SRO; 5) final decision by Director's office.	Yes	Yes	No interviews
NSF/EAGER	Only internal review by POs required; external reviews are rarely sought.	No	No	No interviews
ESRC/Trans-formative Research	1) Commissioning Panel shortlists the applications; 2) Pitch to Peer Workshop where proposals are reviewed by the applicants (under the supervision of a Commissioning Panel); 3) no external peer review: Commissioning Panel makes funding recommendation.	Yes	No	Applicants present their proposal to each other (and to the present Commissioning Panel).
HFSP/main scheme	1) short letter of intent reviewed by a review committee and then a smaller Selection Committee that selects the applicants for the second stage; 2) full proposals are reviewed by external reviewers and the Review Committee; 3) the final decision is made by Council of Scientists and Board of Trustees.	Yes	Yes	No interviews
Volkswagen Foundation/Freigeist	Two-stage selection: 1) Review Committee selects candidates for 2) an interview at the second stage; 3) Review Committee makes the final funding decision	Yes	No	Hearings with the applicants in the second stage
ERC	In each stage, applications are reviewed by three members of one of the 25 panels per call. 1st stage) only an extended synopsis is evaluated, while in 2nd stage) both the synopsis and the full research proposal are evaluated. In this stage, the panel members evaluate the proposal again. The synopsis and the proposal are sent to six or seven external reviewers, the panel typically receives only two or three of these external remote reviews back. The	Yes	Yes	Hearings with starting grantees in the second stage.

	domain-specific panels meet and discuss all the reviews and develop a ranking list, which is typically followed. 3rd) formal decision is made in the agency.			

SO=Scientific Officer; SRO=Scientific Review Officer; SRG=Scientific Review Group (review unit within the NIH); PO=Program Officer

At each stage, applications are filtered such that only a small share of original applications is reviewed. For example, the review procedure of the HHMI and the filtering of applications at each stage are described in Box 2.

Box 2. HHMI review process

First, two scientific officers of the HHMI read each application. Applications are categorised in three different groups; only the applications in the first group (appr. 20%) advance to the next stage of selection.

In the second stage, each application is sent to four reviewers (existing HHMI investigators and other experts), who conduct a second deeper review (rate them). At this stage, the number is further reduced to approximately 30% of the original number.

In the third stage, HHMI scientific officers examine the top-rated applications and see how these match with their already supported research; and scientists in the same area in the institute examine the applications. The scientific officers assign the applications to the reviewers, and they produce an approximate rank order after this round of reviews.

In the fourth phase, the final full review panel consisting of distinguished scientists from around the world cuts the number down to approximately half. This panel is advisory and meets at the HHMI campus. The panel members are asked to send their scores in advance (1 through 5). The scores are calculated to rank the applications. The panel removes the top 10-12 and the bottom 20, and all applications in the area in between are discussed.

After the discussion, there is a vote on the score. Face-to-face discussions help to resolve potential differences among the panellists.

The panel advises the top leadership of the institute, who make the final choices. This choice is prepared by the scientific officers, who rank the applications on the basis of the advisory panel review.

.

The final stage of the selection process is the most thorough, and interviews or hearings take place at this stage, if at all. Four organisations use hearings or interviews with the applicants. These interviews or hearings can take place in the form of a scientific symposium (HHMI), a workshop (ESRC) or a classical hearing (Freigeist). The ERC uses hearings for the more junior grant applications. Hearings take place with a panel or review committee, which can then have better grounds for making its decisions on the final ranking or rating of the proposals.

7.2.1 Panel decision making

As noted above, the requirement of achieving a consensus in the review panel is a mechanism that can filter out unconventional research ideas. However, not all the organisations/schemes require the panel to reach consensus. This leeway applies, for example, to the NIH scheme, in which the panellists are not required to reach a consensus. However, regarding the creation of a ranking list, the NIH scientific officers have considerable influence on the preparation of the final ranking list for the institute leadership.

Table 4. Use of a panel and its decision making

Funding organisation	Programme/scheme	Panel	Decision Principle
Howard Hughes Foundation (HHMI)	HHMI Investigator Program	Yes	Consensus / Can use voting
National Institutes of Health (NIH)	NIH Director's New Innovator Award	Yes	Can use voting, although consensus is not required
National Science Foundation (NSF)	EAGER Early-concept Grants for Exploratory Research	No	N/A
Economic and Social Research Council (ESRC)	Transformative Research	Pitch to Peer + Commission panel (gives funding recommendation)	Consensus
Human Frontier Science Programme Organisation (HFSP)	Human Frontier Science Programme (programme grants and young investigators) ("main scheme")	Yes	Consensus
Volkswagen Foundation	Freigeist Fellowships	Yes	Consensus
The European Research Council (ERC)	Starting Grants and Advanced Grants	Yes	Consensus / different means to achieve

Although discussions and debates are the major means of achieving consensus in a panel (Lamont 2009; Luukkonen 2012), panels may use voting to place the scores of applicants in a ranking order. Voting is used at least by the HHMI and NIH panels (even though they are not required to reach a consensus). The HFSP basically employs a common consensus-oriented system in which two panel members take the lead; however, other panellists are also advised to participate in the rating of proposals that finally leads to a ranking list (which is also then discussed again). Because the NSF does not have panels, this is not an issue. The "Pitch to Peer Workshop" of the ESRC is not based on consensus because the workshop splits applicants into subgroups that only interact through the participating members of the Commissioning Panel. Participants can give individual votes (which, in fact, are often quite diverse). However, after the workshop, the decision making of the Commissioning Panel is consensus-based.

All of the review panels play a role in providing advice, but they do not make decisions about the ranking of the applicants. The final decision is typically made by an authoritative higher body in the organisation. However, to our knowledge, the authoritative higher body typically makes a formal decision, following the recommendations of the review panels or the scientific staff of the organisation.

7.2.2. Measures taken to enhance the review process: Background of the panellists

The cases covered by this review tend to appoint peer review panels that are interdisciplinary ('generalist' panels). Interdisciplinary panels are expected to understand and appreciate highly

innovative ideas and be less limited in terms of the questions or approaches that are not mainstream in their own fields (Nedeva, unpublished). Furthermore, the panellists are highly distinguished scientists. Freigeist (Volkswagen Foundation), the HHMI and the HFSP aim to staff the committees or juries with scientists with a strong 'interdisciplinary' or 'risk-taking' record and give them considerable freedom. At the NIH, the panellists are chosen for their breadth of knowledge and expertise. The HHMI also uses its own scientists as reviewers at one point in the selection, but the full review panel consists of experts who are recruited globally. With 25 panels for each main scheme over the entire spectrum of scientific fields, the ERC has the most elaborate panel system. The panellists are distinguished and active researchers in their respective fields and, despite the number of panels, to some extent, represent generalists.

The NIH Scientific Review Officers train new panellists through teleconferences on the nature of the programme and the much more sketch documentation that is required in the review. This practice highlights the fact that being a panellist in a ground-breaking scheme is regarded as different from the more conventional review panels and, thus, requires special preparation.

7.2.3. Role of Scientific Officers

Scientific officers of the funding organisations play a significant role in the selection process that is not exclusively technical. The US research funding organisations give their scientific officers more influence — or at least acknowledge it — in the selection process. In general, scientific officers conduct the first screening and filtering of the applications and assess the appropriateness and basic quality of the applications for the scheme. They can select the internal or external reviewers for the applications, although in some schemes, many parties participate in the process of suggesting reviewers, including the applicants (HFSP). In later stages, the scientific officers summarise the comments and views of the panel and help to prepare the final ranking list of the applications to be funded. In doing so, they typically examine the portfolio of projects already funded and attempt to avoid overconcentration on specific areas of research. We do not have sufficient information on the background of the officers of all the organisations, but the scientific officers of the HHMI are appointed for fixed terms and have a separate scientific career. Similarly, the NIH and NSF officers have a background in science and/or a scientific career. At the Volkswagen Foundation and the ESRC, programme officers typically have a scientific education (PhDs) but have not necessarily pursued a long scientific career before becoming programme officers.

In the NSF, the Programme Directors can also be proactive and solicit EAGER proposals from researchers based on their knowledge of the research domain and the researchers who could potentially contribute to new developments.

7.3. Criteria: Research plan versus PIs

In their selection criteria, most research funding organisations emphasise the research idea and the applicant's potential for creative and innovative research, merits or the quality of the

work previously performed, or qualifications to conduct the proposed research. The HHMI places a greater emphasis on people than on projects. The HFSP is the only scheme that requires collaboration and international collaboration, and thus, selection focuses on the innovativeness of the research idea and interdisciplinarity of the approach. Freigeist is another funding scheme in which interdisciplinarity is specifically noted as an indication of highly innovative research. The NSF seems to emphasise ideas more than people and has not specified the selection criteria for principal investigators.

Table 5. Evaluation criteria and documentation needed

Funding organisation	Programme/scheme	People vs. research ideas	Length of research plan
Howard Hughes Foundation (HHMI)	HHMI Investigator Program	People	upper limit 3000 words
National Institutes of Health (NIH)	NIH Director's New Innovator Award	Ideas and people	10-page essay
National Science Foundation (NSF)	EAGER Early-concept Grants for Exploratory Research	Ideas	5-8-page project description
Economic and Social Research Council (ESRC)	Transformative Research	Ideas	Upper limit two pages of A4
Human Frontier Science Programme Organisation (HFSP)	Human Frontier Science Programme (programme grants and young investigators) ("main scheme")	Ideas	In the second stage, full proposal
Volkswagen Foundation	Freigeist Fellowships	Ideas and people	Full proposal
The European Research Council (ERC)	Starting Grants and Advanced Grants	Ideas and people	In the second stage, full proposal

To emphasise the highly innovative and unconventional nature of the research to be funded, in most cases, the applicants are required to submit only a short vision paper or an essay on the suggested research, even at the second stage of the process. Exceptions include the HFSP, Volkswagen Foundation, and the ERC, which require full research proposals at the second stage. The short vision paper emphasises the nature of the schemes as frontier research that is uncertain and risky. The rationale for this is the fact that as more emphasis is placed on details in the research plan, the scheme becomes less likely to promote frontier research; detailed research plans emphasise the elements that are known in advance and often present preliminary data. For example, the HHMI requests only a 3000-word vision document from the applicants. The ESRC transformative research proposals are remarkably short (2 pages) for a social science funding scheme, and a budget is not even required at the stage of selection.[16]

The funding organisations emphasise not only novelty, innovativeness and the importance of the suggested research but also the applicant's potential for creative and innovative research. Both play an important and decisive role in the selection, and because competition for funding is tough, the selection process can be demanding in both respects. The past achievements of

the applicant play a role in lending credibility to the suggested new research ideas and providing proof that the applicant can complete the project. Schemes such as the Freigeist and ESRC (must) rely strongly on the 'promises' of the applicants.

Attention to the background of the applicants is related to the aim of supporting creativity, which most of these schemes emphasise. For instance, the HHMI emphasises that it funds people, not projects. Thus, the HHMI pays special attention to the individual applicants and their past performance in terms of their capacity for creative thinking and radically innovative ideas or great impact on the development of the field. Among other things, applicants are requested to provide a complete bibliography and an overview of their most significant research achievements. However, the selection is based not only on the individual and her/his merits and proof of earlier achievements but also, equally importantly, on the suggested vision of the new research. HHMI officers state that they conduct a 'deletion test', that is, an assessment of the effect on science if the individual does not conduct the suggested research. If there would be no effect, then the person drops down the ranking list even though the research might be excellent. This emphasis must be relaxed when more junior researchers are under consideration.
While the ESRC is also first and foremost seeking to fund ideas it takes almost an opposite strategy as compared with the HHMI; namely, in the first round, it anonymises the applicants and only seeks to review the ideas in the proposal. It thus does not attempt to find individuals who have shown especial indications of creativity. The rationale of the ESRC is to prevent the seniority or the reputation of the applicants influencing the evaluation in an adverse manner and to allow less well-known applicants to have an equal chance. The UK EPSRC (Engineering and Physical Sciences Research Council) applies the same procedure in its Big Pitch Bright Ideas Award.[17] It appears that there is difference between institutional policies and views of the ways how best to select promising transformative research. Since studies of the impacts of the more recent practices in the UK Research Councils are not available it is difficult to judge the outcomes of these different policies.[18]

It is noteworthy that the funding schemes considered here do not support researchers who have recently received their PhDs; for example, the HHMI requires that applicants have 5-15 years of professional experience after the PhD. Nevertheless, even this range of professional experience can make it difficult to compare the applicants.

In summary, both the research idea and the applicant's profile are important, but more detailed conclusions about how they are treated by reviewers in their judgement are difficult to draw without interviewing individual reviewers.

7.6. Major differences between selection procedures concerning frontier research and mainstream schemes

Three of our cases studies, the HHMI, HFSP, and ERC, support frontier research as their main activity. In all other examples, frontier research support constitutes a fairly small share of overall funding activities. When describing the selection procedures of these schemes compared with the mainstream schemes, one of the major differences seems to be a shorter and more essay-like research proposal document that emphasises the novelty and breakthrough nature of the proposed activities. In such a document, detailed research plans can be counterproductive. Typically, peer review procedures are used in mainstream and frontier schemes, but the choice of peer review panel members in frontier cases highlights experience in selecting frontier or unconventional research. However, in both types of cases, panel members are expected to have broad knowledge and expertise in the research area to be evaluated. Remote reviewers, when used, represent more specific expertise.

An interesting example is provided by the NSF, which uses peer review procedures for the selection of its main supported research, while the EAGER scheme proposals are normally only evaluated by scientific programme officers. The ESRC employs traditional review processes for its regular schemes but has created the "Pitch to Peer Workshop" for the transformative research scheme. There is also some difference in that selection criteria for frontier research emphasise the breakthrough and potentially unconventional nature of the research and/or the review criteria are less detailed than in the mainstream activity.

At times, frontier schemes appear to be test sites for new and innovative evaluation procedures for the research funding organisations to experiment with new means of selection: non-traditional research must be selected by non-traditional methods. For example, the Freigeist and ESRC transformative research schemes employ such methods. The Freigeist does not use written reviews, and the HHMI does not require that the reviewers write written comments but requests that they rate the applications. The ESRC transformative research scheme uses the novel workshop idea and involves all the shortlisted applicants in the process. Its selection procedures differ greatly from those normally used in ESRC selection processes. It is interesting in that it does not use the elements that are regularly present in normal schemes, e.g., specified budgets in the application stage, external remote peer review, the CVs of key researchers. The idea of using applicants in the selection process raises the question of how independently the workshop participants review the applications of their rivals. Because this practice is very new, we do not have any monitoring data on its outcomes or specific problems. Furthermore, the manner in which experiments such as these affect mainstream schemes remains to be seen.

7.7. Evaluation of the outcomes of the schemes

Typically, the reviewed schemes do not use specific monitoring or *ex-post* evaluation procedures. For example, with the HHMI, *ex-post* evaluation occurs in the reappointment process. The HHMI allows the reappointment of the five-year investigator grants, and 80% of

the investigators are reappointed. Even when they are not reappointed, there is a soft landing with an additional 2-5 years of support. The performance of the investigators is thoroughly evaluated for reappointment through processes that are similar to the original appointment. However, the quality, not the quantity, of publications and achievements are important for reappointment, with achievement meaning that the investigators have opened up new lines of research, have helped to move the field in a positive direction etc.

In other organisations, the funded investigators must in some cases, make an annual report and in most cases, provide a final report on the research conducted. Data from such reports may be used for institutional review (HFSP) and can provide other publicity material (HFSP 'success stories').

Some funding organisations have commissioned specific evaluation studies. The NIH commissioned an outcome evaluation of its NIH Director's Pioneer Award scheme (initiated in 2004) and NIH Director's New Innovator Award, which are intended for highly innovative research, albeit for more senior researchers (Lal et al. 2012). The study compared the Pioneer Award with traditional NIH support and the HHMI at the level of investigators and programmes using publication data and expert assessments of performance measurement.

According to the publication analysis, the overall summary was that NIH Pioneers outperformed the more traditional scheme awardees and were similar to the HHMI investigators in many respects, but when there was a difference, the HHMI investigators outperformed the Pioneer awardees.

According to the expert assessments, some forms of impact on science were more commonly cited among the HHMI investigators and Pioneers than the matched NIH awardees, while translational or clinical potential was more commonly cited among the Pioneers and matched NIH awardees than the HHMI investigators. Innovation in research was cited most often among the HHMI investigators, followed by the NIH Pioneering awardees and the matched NIH awardees. On average, the differences between the Pioneers and HHMI investigators were not statistically significant, while the traditional NIH awardees differed from these two groups significantly. A careful analysis also showed that some of the noted differences were attributable to principal investigator differences (selection) and differences in the programme characteristics.

This study drew on the scientific production of the awardees in the period from FY 2004-2006 to the end of 2011, implying a five-year time lag after the end of the award period. However, breakthrough research can take longer to materialise than the interval used.

Further research has been conducted on the impact of different funding schemes on creativity, such as Azoulay et al.'s (2011) study on HHMI investigators and 'traditional' NIH grantees, which also used citation-based and key word analysis. Some of the findings are interesting: the number of 'hit' articles at the baseline is highly predictive of HHMI appointments; HHMI investigators fail more often (fall in the bottom quartile of citations); "the punctiliousness of the NIH peer-review process [traditional scheme] crowds out scientific exploration" (p. 548); and HHMI investigators tackle more novel topics. These findings imply that, compared with

traditional NIH grant schemes, the HHMI is more successful in promoting or selecting innovative and exploratory research themes and performers.

In the ESRC scheme, all projects last 18 months with no extension. The rationale for this guideline is that, if projects perform well and become catalysts for transformative change, then the ESRC offers additional funding to such projects based on the input and review of the Commissioning Panel. However, because of the tight budgetary situation of UK public research funding, no funds are available for that purpose. Hence, projects worth subsequent funding must be evaluated in established schemes — including all of the risks of being filtered out for being unconventional. Currently, after round 3 of the scheme, the entire programme is evaluated, but this evaluation will not be completed until 2016.

The HFSP was evaluated in 2010 (Edler et al. 2010) using different surveys coupled with bibliometric analysis. The portfolio during the evaluation period included long- and short-term fellowships: career development awards and Programme Grants and Young Investigator Grants (YIG). The latter two programmes were a case in our exercise because they provide larger grants for frontier research. The survey analyses of the two programmes showed a high degree of additionality, in part due to the specific requirements of transcontinental, interdisciplinary teams. The surveys showed that the principal investigators (PIs) could obtain funding for risky research and could move into a new field without a prior track record. The bibliometric study revealed very high citation impact compared with the global baseline. Overall, the evaluation of the HFSP programme grant and YIG provided some evidence of additionality, high-risk and interdisciplinary research and the high impact of publications from HFSP-funded projects. The question of whether HFSP-funded projects represent transformative/frontier research could only be answered partially using these criteria.

In summary, the evaluations provide some data on the higher performance of especially the HHMI scheme but also the NIH ‘transformative’ scheme when compared with the ‘regular’ scheme. Furthermore, the HFSP scheme showed evidence of higher performance. However, most of this evidence comes from bibliometric studies. As noted above, such studies provide evidence of the short-term visibility of the research funded through these schemes or, in some instances, of the ability of the schemes (particularly, the HHMI) to select highly performing and innovative scientists for their funding, but they do not provide firm evidence of the ground-breaking nature of the research funded by the schemes.

8. Conclusions

The different schemes use different terms to describe the nature of research that they seek to fund, although their aim is similar: research involving high risks but promising high impacts on science, society, and the economy. All the funding schemes emphasise the purpose of promoting frontier research and apply specific selection criteria that aim to identify and support such research. The selection procedures used in these schemes vary. Nevertheless, there are clear differences from the procedures used in schemes that do not specifically aim to

promote frontier research, and thus, the nature of the funded research has been taken into account in the portrayed schemes.

All except one funding organisation use peer review, and one organisation uses it in an unconventional manner. The funding organisations, however, focus on the ways in which they use the peer review procedures to counteract its potential bias and conservatism.

Although some of the schemes use remote reviewers, they play an assisting role, and the main body of evaluation is a multidisciplinary panel of experts or, rather, generalists. Most of the funding organisations in this review use review panels or committees of experts that deliberate on the relative merits of the applicants and applications, and the members of such panels are selected on the basis of their broad — often interdisciplinary — expertise, experience in evaluating frontier research proposals and scientific/scholarly reputation.

The following list encapsulates the features that, in light of the research on peer review, can be conducive to the selection of ground-breaking ideas and are used in most of our examples:

- generalist (often interdisciplinary) panels with panellists who have experience in evaluating ground-breaking ideas
- criteria emphasising the innovative and unconventional nature of the research to be funded
- requiring short essay-like vision or research idea papers rather than proper research plans, especially in the first stage but in many schemes overall
- emphasising both ideas and people or only people and their creativity and ability to conduct radically innovative research.

In nearly every aspect, there is a range in the degree to which different features are employed. This range applies to the degree of detail concerning the planned research required in applications, although these schemes generally require considerably less detailed research plans than is normally required and sometimes do not even require actual research plans but, rather, a short vision document concerning the research to be funded to emphasise the unexpected and uncertain nature of frontier research. The schemes also vary in the extent to which they emphasise the selection of people versus projects. However, both aspects play a role in the selection procedures.

There were also features in our cases that might potentially hinder the selection of ground-breaking ideas:

- many rounds of selection; each round can screen out potentially radically innovative ideas
- in most cases, the requirement of consensus within the panel.

Multistage filtering/selection processes are a common feature of these schemes, enabling a better focus on those proposals deemed to have the most potential for frontier research. The implication is that a potential frontier proposal can be rejected at any stage in the selection process because one cannot assume that the selection procedures are fault-free. It is more likely that such proposals are within the boundary area where their funding decision is

debated: a potential frontier proposal has merits, but there are aspects of the proposal that make it debatable.

The requirement of panel consensus, however, is not universal, and there are means to achieve a final verdict in the absence or presence of the requirement of a consensus. If the requirement of consensus in a panel potentially prevents the selection of ground-breaking ideas, then the strongly modified version of peer review, the "Pitch to Peer Workshop" used in the ESRC scheme, may be an even more consensual type of group process. In the absence of studies or evaluations of this procedure, we do not know whether this procedure advances or hinders the funding of out-of-the-box ideas.

The HHMI, which has been in existence for the longest period, has been studied more than the other schemes, and it has been shown to have a strong record in promoting frontier research. Most of the schemes studied here are somewhat recent, and their track record cannot be proven or disproven for some time to come, although there are some indications of positive achievements. Providing evidence of the success of a frontier scheme is particularly difficult because most of the available methods assess the outcomes and impacts in a fairly short term, when the impacts cannot yet be seen. Furthermore, it is noteworthy that it is not only the selection procedures but also the overall terms, the amount of funding and the time period of funding that influence the outcomes of a funding scheme. Some of the schemes are intended to be the first funding for testing new ideas, with the next funding input expected to derive from more conventional sources. Whether and the degree to which this occurs were outside the scope of this review. As organisation's high reputation as a funder of frontier research influences the self-selection of applicants and the ideas that they propose to the funding scheme, which can be an additional success factor.

To conclude, on the basis of our review, there is variation in the mechanisms that the studied schemes and organisations use for the selection of ground-breaking research, and there is no single dominant pattern. Nevertheless, peer review seems to be thriving even in the schemes that attempt to promote unconventional, high-risk and highly innovative research, and only one scheme uses peer review as an exception. A final observation is that the funding organisations that are responsible for these schemes make a serious attempt to modify peer review procedures to ensure that they are better suited for the purpose, although there are inherent limitations that are difficult to overcome.

Acknowledgements
This paper draws on the review we prepared for the European Research Council Expert Group for Programme Monitoring and Evaluation. We would like to thank our informants at the different research funding organisations who generously shared their knowledge with us, in particular, Guntram Bauer (HFSPO), Henrike Hartmann (Volkswagen Foundation), Rajiv Kumar (NIH), James W. Mack (NIH), Jeremy Neathey (ESRC), and Geoffrey Richards (HFSPO). Furthermore, we thank Maria Nedeva (University of Manchester) for providing information on the US schemes and Joanne S. Tornow (NSF) for information on the NSF.

9. References

Azoulay, P., Zivin, J.S.G. , Manso, G. (2011) Incentives and creativity: evidence from the academic life sciences. RAND Journal of Economics, Vol. 42, No. 3, Fall 2011, 527-554.

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

Cole, S., Cole, J. R. and Simon, G. A. (1981): Chance and Consensus in Peer Review, Science, 20 November 1981, Volume 215, Number 4531, 344-348.

Cole, S., Rubin, L. and Cole, J.R. (1978) Peer Review in the National Science Foundation: Phase One of a Study. Washington, D.C.: National Academy of Sciences Press.

Edler, J. Rigby, J., Jones, B. (2010) Review of Human Frontiers Science Program. http://www.hfsp.org/sites/www.hfsp.org/files/webfm/Executive (accessed 6 April 2015).

European Commission (2005) Frontier Research: The European Challenge, High-Level Expert Group Report, February 2005, Brussels. http://ec.europa.eu/research/future/pdf/hleg_fullreport_frontier_research_april2005.pdf (accessed 6 April 2015).

Godin, B. (2006) 'The Linear Model of Innovation. The Historical Construction of an Analytical Framework. Science, Technology, & Human Values, 31/6, 639-667.

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.

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.

Ismail, S., Farrands, A. Wooding, S. (2009) Evaluating Grant Peer Review in the Health Sciences - A review of the literature, RAND Corporation, Santa Monica, CA, Arlington, VA , Pittsburgh, PA, Cambridge, United Kingdom.

Kuhn, T. S. (1962). The Structure of Scientific Revolutions. Chicago: University of Chicago Press.

Lal, B. Wilson, A., Jones, S., Lee, E., Richards, A., Peña, V. (2012) An Outcome Evaluation of the National Institutes of Health (NIH) Director's Pioneer Award (NDPA) Program, FY 2004-2006. IDA Paper P-4899, Washington, DC.

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: Evaluative practices in various types of peer review panels. In: Camic, C., Gross, N. and Lamont, M. (eds) *Social Knowledge in the Making*. Chicago: University of Chicago Press.

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

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

Laudel, G. and Gläser, J. (2014) Beyond Breakthrough Research: Epistemic properties of research and their consequences for research funding. Research Policy 43, 1204-1216.

Luukkonen, T. (2012) 'Conservatisms and risk-taking in peer review: Emerging ERC practices', Research Evaluation 21, 48-60.

Luukkonen, T. (2014a), Peer Review and Knowledge Dynamics, ERC Workshop Monitoring the performance and quality of peer review systems 28-29 November, 2013, Brussels

Luukkonen, T. (2014b) The European Research Council and the European Research Funding Landscape, Science and Public Policy 41, 29-43.

Nuffield Council on Bioethics. 2014. The Culture of Scientific Research in the UK. December 2014.

Neufeld, J., Huber. N., and Wegner, A. (2013) Peer review-based selection decisions in individual research funding, applicants' publication strategies and performance: The case of the ERC Starting Grants, Research Evaluation 22, 237-247.

Report to the National Science Board on the National Science Foundation's Merit Review Process Fiscal Year 2013, May 12, 2014.

Roy, R. (1985) Funding Science: The *Real* Defects of Peer Review and an Alternative to it. Science, Technology, & Human Values, 10/3, 73-81.

Schauz, D. (2014) 'What is Basic Research? Insights from Historical Semantics', Minerva 52: 273-328.

Science and Innovation Investment Framework 2004-2014: Next steps (March 2006) HM Treasury, DTI, Department for Education and Skills.

Science (2014) 'Does journal peer review miss best and brightest?' Available at http://news.sciencemag.org/scientific-community/2014/12/does-journal-peer-review-miss-best-and-brightest (accessed 6 April 2015).

Science (2015) 'Peering into Peer Review'. 343, 596-598.

Stokes, D. E. (1997) Pasteur's Quadrant: Basic Science and Technological Innovation. Brookings Press, Washington D.C.

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.

Van Raan, A.F.J. (2004) Sleeping Beauties in Science, Scientometrics 59/3, 461-466.

Wagner, C.S. and Alexander, J. (2013) Evaluating transformative research programmes: A case study of the NSF Small Grants for Exploratory Research programme, Research Evaluation, 22, 187-197.

Endnotes

[1] ETLA, the Research Institute of the Finnish Economy

[2] Vienna Science and Technology Fund WWTF

[3] Vienna Science and Technology Fund WWTF

[4] Peer review is widely used in the selection of articles for publication in scientific and scholarly journals, and this is the area in which it originated, i.e., when the Royal Society founded the Philosophical Transactions in 1665, publication in the Transactions was authorized by the review of some members of the Society (Chubin and Hackett, 1990).

[5] For example, the UK ESPRC's 'sandpit' residential workshops.

[6] In this paper, we use the term 'ground-breaking' as a generic term indicating high-risk, uncertain, unconventional, transformative etc. research. As noted below, many terms are used to refer to this type of research.

[7] The fields were computer science, chemistry, cancer research, energy research, urban studies, and archaeology. The study was an interview study with 80 researchers in Finland and the UK (see Luukkonen, 2014a).

[8] Risk in this connection typically refers to uncertainty in terms of the research/project's achieving what it set out to achieve for a number of reasons: whether the underlying assumptions of the research are tenable, whether it is technically feasible to conduct the research as planned, whether the findings are as expected etc.

[9] See http://www.nsf.gov/about/transformative_research/ accessed 4 April, 2014.

[10] For example, the NSF established the Small Grants for Exploratory Research (SGER) programme, which operated from 1990 to 2006 and was a predecessor to the EAGER, which we study in this paper, and its sister programme, called RAPID.

[11] Sandpits are residential interactive workshops conducted over five days involving 20-30 participants: the director, a team of expert mentors, and a number of independent stakeholders. "Sandpits have a highly multidisciplinary mix of participants, some active researchers and others potential users of research outcomes, to drive lateral thinking and radical approaches to address research challenges." (http://www.epsrc.ac.uk/funding/howtoapply/routes/network/ideas/whatisasandpit/, accessed on April 6, 2015). Sandpits define and clarify specific potential research topics and develop them into research ideas and proposals, and the proposals are shortlisted and ranked in priority order before a final recommendation by the Director and mentors to the EPSRC.

[12] At the time of that study, the ERC only had two grant schemes, Starting Grants and Advanced Grants. This study reports on these findings. It is noteworthy that the ERC has since established Consolidator Grants (for researchers between the former groups) and two small grant schemes: ERC Proof of Concept and ERC Synergy Grant. See http://erc.europa.eu/funding-and-grants/funding-schemes (accessed on 6 January, 2015).

[13] The ESRC scheme does not have different pillars for different career stages — all applicants compete against each other. However, in round 1, applicants at the professorial level were much more successful. After creating a level playing field in round 2 (by including early stage researchers, ESRs, in the Commissioning Panel), ESRs performed much better than in round 1. While contributing 27% of the total applications, 54% of those who received funding were ESRs. However, the scheme has no agenda in terms of what career stage should be supported. The HFSP's Programme Grant addresses teams of independent researchers at all career stages, but given that there is a special scheme for Young Investigators, the "normal" schemes tend to favour more senior researchers.

[14] Only those eligible research organisations that received more than £100 000 of ESRC funding in the previous year were able to respond to the pilot call. The purpose of this restriction was to enable the ESRC to manage demand in the pilot phase. Since round 2, all eligible institutions (= recognized research organizations) can submit at least one proposal, and Top 11 institutions can submit two. In the future, all eligible institutions will be allowed to submit two proposals.

[15] This is part of a larger movement in the UK and its Research Councils to shift selection processes to the research organisations to reduce administration costs (at the Research Councils).

[16] It needs to be included *after* being selected for funding; however, it is noteworthy that the maximum size of a grant is not large, i.e., £300 000 in total.

[17] [17] https://www.epsrc.ac.uk/funding/howtoapply/routes/network/ideas/brightideas/

[18] Another great difference that would affect any findings of the effectiveness of the different policies is the fact that the HHMI grants generous support over a sizeable number of years while the ESRC scheme is a small grant for 1,5 years to test the potentially transformative ideas.