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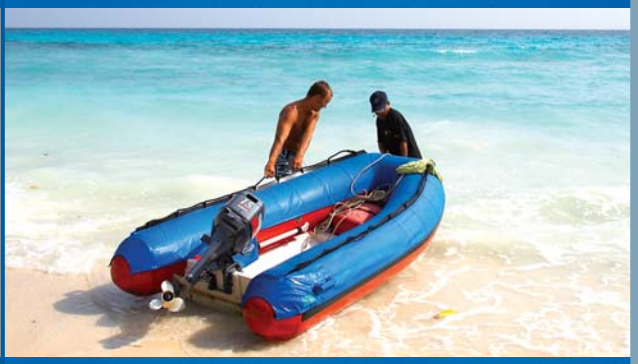
Funding for high-risk research at the Academy of Finland



Maunu Häyrynen

Academy of Finland
Research funding and expertise

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Tiivistelmä	Uudet tieteelliset avaukset, joihin viitataan myös riskirahoitus-nimityksellä, ovat uutta luovia, kunnianhimoisia, tiedettä uudistavia ja rajoja rikkovia tutkimushankkeita. Niiden merkitys voi perustua poikkeuksellisen mittavien ja monimutkaisten tutkimuskysymysten vastausyrityksiin, vallitsevien teorioiden ja tieteellisten paradigmojen haastamiseen, tutkimusmenetelmien radikaalisti uudenlaisiin sovellustapoihin tai eri tieteenalanäkökulmien ennakkoluulottomaan yhdistelyyn ja tieteidenväliseen integrointiin. Uusilla avauksilla on poikkeuksellinen epäonnistumisen riski. Raportin taustaosuuksissa tarkastellaan riskin käsitettä yleisesti ja tutkimuksen piirissä sekä erilaisia näkökulmia uusiin tieteellisiin avauksiin tieteen uusiutumiskehityksen osatekijöinä. Muiden maiden tutkimusrahoittajien uusien avausten määrittelytapoihin sekä keinoihin niiden tunnistamiseksi, arvioimiseksi ja rahoittamiseksi luodaan katsaus, samoin Suomen Akatemian aikaisempiin selvityksiin uusista avauksista. Varsinainen selvitys muodostuu kahdesta osasta, Akatemian tieteelliset toimikunnat ja hallintoviraston tutkimuksen yksiköt sekä muut tutkimusarvioinnin ja -rahoituksen avainhenkilöt käsittäneestä keskustelukierroksesta sekä vuoden 2005 yleisten tutkimusmäärärahojen haun seulonnasta eri toimikuntia edustaneilla tutkimusaloilla. Keskustelukierroksella todettiin uusien tieteellisten avausten kaipaavan lisähuomiota, mutta myös käsitteistön täsmentämistä niiden tunnistamisen ja arvioinnin tueksi. Vuoden 2005 rahoitushakemusten ja -arvioiden perusteella Akatemia myöntää rahoitusta myös merkittäviä riskejä sisältäville tutkimushankkeille, mikäli ne ovat tieteellisesti innovatiivisia ja muuten laadultaan korkeatasoisia. Kymmenesosa kaikista ko. alojen hakemuksista ja viidennes rahoitetuista hankkeista tunnistettiin uusiksi avauksiksi, kriteereinä erityinen tieteellinen innovatiivisuus, tietoinen riskinotto ja läpimurron mahdollisuus. Tunnistetuista uusista avauksista noin puolet rahoitettiin, joten niiden läpimenoprosentti oli muita hakemuksia suurempi. Uusia avauksia rahoitettiin viidellä tarkastelluista kahdeksasta tutkimusalasta. Akatemian hallitus on päätynyt tukemaan uusia tieteellisiä avauksia valtavirtaistamisen eli läpäisyperiaatteen avulla. Johtopäätöksissä todetaan tämän merkitsevän tutkijoiden rohkaisemista tutkimusalasta, iästä ja sukupuolesta riippumatta uusien riskejä sisältävien tutkimusideoiden esittämiseen rahoitushakemuksissa sekä hakemusarvioinnin ja hankeraportoinnin kehittämistä uudet avaukset entistä paremmin huomioon ottaviksi. Uusien avausten arvioinnin pääpainon on oltava tutkimussuunnitelmien sisällössä. Akatemian tulee kannustaa uusiin avauksiin myös viestinnällään. Uusien avausten rahoituksen tulee tukea tieteellistä monimuotoisuutta eikä sitä pidä sitoa ennakoinnin ja tutkimusindikaattorien osoittamiin painopistealueisiin.	
Asiasanat	Uudet tieteelliset avaukset, riskinotto, riskirahoitus, vertaisarviointi, tutkimusrahoitus	
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Description

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Abstract	Breakthrough research, also known as high-risk research, is transformative, ambitious and mould-breaking research. Its significance may be based on tackling exceptionally wide and complex research problems, on challenging established theories and scientific paradigms, on radically new ways of using methods as well as on unprejudiced combination and interdisciplinary integration of different research perspectives. Breakthrough research is characterised by an exceptional risk of failure. In the background sections of the report the concept of risk is addressed in general and in the context of research. Various views on breakthrough research as an element of scientific progress are studied. An overview of the means of identifying, evaluating and funding breakthrough by research funding organisations in different countries is presented, as well as a summary of earlier discussion on the subject at the Academy of Finland. The report proper consists of two main parts, firstly an account of a discussion round entailing the Research Councils, the corresponding research units and other key persons as regards research reviewing and funding at the Academy. The second part is formed by a survey of the 2005 call for general research grants in fields of research representing each Research Council. During the discussion round breakthrough research was held to merit more attention but also a sharper conceptual framework to help in identifying and evaluating such projects. Based on the survey of the proposals and their reviews in the 2005 call the Academy can be said to fund projects containing significant risks, provided they are deemed scientifically innovative and otherwise high-quality. A tenth of all proposals and a fifth of the projects receiving funding could be identified as breakthrough research, particular scientific innovativeness, conscious risk-taking and breakthrough potential used as criteria. Of identified breakthrough research proposals about half were funded, their likelihood to receive funding being much higher than that of an average proposal. Breakthrough research was funded in five of the eight fields under study. The Board of the Academy ended up supporting breakthrough research by mainstreaming. In the conclusions this is understood to entail encouraging researchers to present novel and risky research ideas regardless of their field of research, age or gender. Efforts should be made to better accommodate breakthrough research in the reviewing and funding processes, the main emphasis being on the content of the research plans. The Academy should also promote breakthrough research by its communications. The ultimate aim in funding breakthrough research is to enhance scientific diversity and it should thus not be applied restrictively to the strategic areas highlighted by foresight exercises and bibliometrics.

Key words	Breakthrough research, risks, high-risk research, peer review, research funding	
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Foreword

Decision-making about the funding of research is, in principle, a straightforward business. The applicants are rank-ordered by outside experts according to the scientific quality of their proposals. The funds available are then apportioned in this order until they have been expended. In some disciplines it is thought that this rank-ordering can be produced directly on the basis of citation indices and impact factors, perhaps with the additional adjustment of the applicant's academic age.

However, this is too simplistic an approach to research funding. The goals of the research and the priorities set out in the research plan must also be taken into account. In some fields the assessments may be based on the expected practical benefits of the research. It is also possible to prioritise a specific group of applicants, such as young researchers or women. This is often called science policy, but science policy cannot challenge the primacy of scientific quality as a funding criterion.

Decision-making that relies on mechanical criteria no doubt produces high-quality science, but it does not necessarily lead to important scientific breakthroughs. Routinisation is a real risk in research as well, and that must be resisted with open-mindedness. I remember Erik Allardt once saying that if the model of explanation doesn't work, then the addition of new explanatory factors is hardly going to solve the problem. What you need in this situation is a new theory or a new interpretation of the old theory. At least in the social sciences this is quite commonplace, and it occasionally leads to heated disputes and debate on methodological and theoretical questions.

The problem of funding scientifically innovative, high-risk research is not a new one, but is the subject of ongoing discussion in virtually all organisations that finance basic research, including the Academy of Finland. This report by Dr Maunu Häyrynen provides an overview and assessment of international developments in breakthrough research and reviews the discussion on the subject at the Academy of Finland. As the report shows, there is no consensus in the international academia on the problem and how it should be resolved.

Some take the view that the Academy should create a separate funding instrument for breakthrough research, others believe that the current system of allocating funding within the existing structure of disciplines is good enough to identify potential breakthroughs. The difficulty with the latter approach, it is pointed out, is that especially multidisciplinary and interdisciplinary research that involve greater than usual risks, would be sidelined in funding decisions. Indeed, reviews commissioned by the Academy indicate that the proportion of these kinds of proposals has increased, but they do receive a fair and equal treatment nevertheless. It is imperative that the concept of breakthrough research remains clear. Funding must not be granted to research plans that promise a breakthrough effort if it is clear that those plans are not viable in the first place. In other words, the research plan must have credible breakthrough potential, even if the project eventually does fail to achieve its targets. Decisions to fund these kinds of projects may be complicated by widely differing expert opinions on their quality; is it possible in this situation to rely on the views of the one expert who rates the project most highly?

It is sometimes thought that funding for high-risk research is intended primarily for bold young researchers whose thinking is out-of-the-box. Senior researchers may

be more likely than their younger colleagues to be caught up in their old familiar ways and interests, but this does not have to be the case. Senior researchers can secure funding for an innovative, high-risk project by orienting themselves to a whole new field of research or by radically departing from their old perspectives. The purpose of funding for high-risk research is to support and promote the renewal of science and increase its innovativeness, not to serve any other science policy objectives.

The analysis by Häyrynen shows that it is indeed possible to identify and set apart breakthrough projects from the large bulk of proposals and that these projects are of a higher than average quality. If the funding agency's review and decision-making system works properly, then a separate funding instrument is not necessarily needed to identify innovative research projects for funding. Hopefully, the views and results presented in this report will help the Academy and other funding bodies to develop their own mechanisms and procedures to support breakthrough research.

Finally, it is good to remember that funding for high-risk research is not about making choices between different funding instruments; the key issue is the overall level of funding. The diversity of research and funding for high-risk research can best be guaranteed when adequate resources are available to provide funding for all high-quality research projects. In the situation today, the relative scarcity of competitive research funding also hampers the funding of breakthrough research.

Raimo Väyrynen
President

Preface

In recent years there has been much discussion and debate not only within the Academy of Finland but internationally on scientific innovativeness, risks and their relationship to other funding criteria for research. In early 2006, I was commissioned by the management of the Academy of Finland to conduct an inquiry into the nature of breakthrough research, the need for funding it and its funding criteria. To this end, I have held discussions with senior management at the Academy, the heads of Research Units at the Academy's Administration Office and other key personnel. I also arranged a round of discussions with the Research Units and Research Councils. Furthermore, I overviewed the international debate on high-risk research and its funding and undertook a detailed analysis of proposals submitted to the Academy of Finland for general research grants in 2005 in selected fields of research under each of the four Research Councils. In this I was assisted by the Research Units' presenting officials and by research assistant Laura Valkeasuo. The results of this work were discussed by the Academy's Board at its evening session on 3 October 2006, where the decision was reached to support breakthrough research by means of mainstreaming throughout the review and funding process. The results have also been presented to the Executive Committee of the Finnish Council of University Rectors.

Comments on drafts of this text were provided by Raimo Väyrynen, President of the Academy; Anneli Pauli, Vice President (Research); Anne Heinänen and Jarmo Laine, Senior Science Counsels; Tuomas Parkkari, Planning Manager; and Jaana Lehtimäki, Science Adviser. Outside of the Academy I have received valuable comments and feedback from Director Antti Hautamäki (Sitra, Finnish Innovation Fund), Researcher Katri Huutoniemi (Helsinki University of Technology), Director Markus Koskenlinna (Tekes, Finnish Funding Agency for Technology and Innovation), Professor Heikki Patomäki (Helsinki Collegium for Advanced Studies) and Professor Emeritus Y-P Häyrynen. I wish to thank them all.

Maunu Häyrynen
Senior Adviser

"High-risk research is the lifeblood of science."

Ian Diamond, Chair, Research Councils UK.[1]

1 Introduction

A major new focus of concern in the area of science policy in the past few years has been with high-risk or breakthrough research. The reasons for this concern lie both in the escalation of international competition among different research systems, and on the other hand in the growing difficulties to obtain competitive research funding. As the competition has continued to stiffen and the criteria for funding have become more stringent, the prospects of proposals for high-risk breakthrough research look set to deteriorate. If this happens, the potential for renewal and reform in science in general may well be jeopardised. If new scientific ideas were to dry up, that would obviously wreak havoc on the competitiveness of national research systems.

As an area of science policy debate, breakthrough research is not an easy subject to address. There exists no internationally accepted definition of breakthrough research and no coherent terminology in the same way as there is for inter-disciplinarity, for instance.[2] These difficulties are further complicated by differences in opinion about the progress of science and its steerability. A distinction is often made between radical and routine innovation, scientific breakthroughs challenging existing ways of thinking and recognised methods of doing science in the former. However, these category boundaries are fluid.

Breakthrough research is characterised not only by exceptional innovation, but also by the conscious taking of risks in the choice of its research subjects and methods and by its ambitious goal-setting. However, these are defined and understood in different ways according to discipline. A scientific breakthrough may mean completely different things in theoretical physics, multidisciplinary environmental research, experimental nutrition research or constructivist media studies. Likewise, the failure of research appears in different ways in the varying context of disciplines.

The main concern in this report is with high-risk breakthrough research. Its purpose is to gauge attitudes to breakthrough research internationally and at the Academy of Finland; to see how breakthrough research can be identified in the project proposal review process; and to see how research funding should encourage the propagation of new ideas and the taking of risks.

1)The Guardian 20 July 2006.

2) Interdisciplinarity refers to approaches that integrate datasets, methods, tools, concepts and theories from different disciplines, as distinct from multidisciplinarity where the perspectives of each discipline remain apart and from transdisciplinarity, which cannot be reduced to individual disciplines; Bruun et al. 2005.

2 The Concept of Risk

For reasons of space it is not possible here to provide an in-depth discussion of the study of risks and risk analysis. Dictionary definitions of risk say that it is the "possibility, threat or danger of loss, injury or other disadvantage". Colloquially, risk is often taken to refer to the possibility that something unpleasant or undesirable may happen. For purposes of scientific research, risk is defined more closely and rigorously. Most typically, it is considered in terms of the likelihood of some undesirable event or outcome, with that likelihood being at least roughly estimatable. It is distinguished from uncertainty, where the likelihood of the undesirable event cannot be described; and on the other hand from threat, which is the low-likelihood possibility of a serious accident or injury.

Many spheres of life such as medicine, engineering, business and the security sector have their own specific definitions of risks in which risk is often approached from an objectivist stance, in terms of mathematical probabilities based on measurable indicators. Even when they are perceived and understood as real, risks cannot be considered as absolute, but they are always relative to their context.

According to the subjectivist stance, probabilities or risks are not objective, but always based on subjective assessments of reality. Financial author Glyn Holton points out that risk must always involve both a perceived uncertainty by the individual concerned, and exposure to that uncertainty. From this vantage-point, risk is defined as "exposure to a proposition of which one is uncertain" (Holton 2004, 22). The implication here is that risks are always reduced to the points of view of individual agents. Risks are assessed in different socio-cultural frameworks that may persuade people to emphasise certain risks and to bypass others. Furthermore, a distinction can be made between gambling and hedging approaches. (Holton 2004.)

Psychological research on performance motivation and risk-taking has sought to explore how goal-directed activity is influenced by the desire for success and on the other hand by fear of failure. The relationship between target level and risk avoidance varies from one type of person to another, but both very low and very high probability of success will lower performance motivation (Atkinson 1966).

The debate on risk management and the so-called risk society (Beck 1990) provides a wider context for a consideration of risks. Professor of Social Policy Risto Eräsaari says that risk management today is based on collectively produced threat perceptions in which risks are represented using a logic of measurability and probability. This way of thinking is reinforced by a growing consciousness of risks, and it is aimed at gaining a better understanding and control over the uncertainty caused by the constant changes in society. Uncertainty is described either in terms of the absence of alternatives, or in terms of a new opportunity to get the risks under control. However, risks have continued to become more and more complicated, and in reality they are impossible to eliminate. (Eräsaari 2002.)

3 Risks and Scientific Research

It is generally recognised and accepted that basic scientific research is an inherently uncertain exercise, i.e. that it will not necessarily produce the results expected. Research projects may fail to get off the ground at all, or fail to produce any publications, degrees or patents. On the other hand, basic research often produces results that are different from those originally intended, and a study that was initially ignored altogether may turn out to be a major scientific breakthrough. Whether or not a particular piece of research becomes recognised as a breakthrough depends on the development of science and society more generally and is very difficult, if not impossible to predict.

It is also possible to identify unequivocal risk factors in research and to estimate their probability in one way or another. This provides the foundation for the risk assessments in the Tekes (Finnish Funding Agency for Technology and Innovation) funding instrument "strategic basic research", for instance. Tekes uses four risk variables: 1) human resources and competencies, 2) company financial status, 3) discovery of a technological solution and 4) targeted markets. Each variable is assessed on a five-point scale from low risk (20%) via significant, large and very large risk to intolerable risk (100%).

In the case of human resources and competencies, for example, an intolerable risks means that the project or the business company is not thought to possess any skills or experience in relation to the content area concerned; and in the case of discovering a technological solution, that the solution pursued is not considered technologically viable. Risk assessment is an integral part of the overall process of reviewing project viability. Another aspect of this assessment concerns the project's innovativeness. (Director Markus Koskenlinna, 7 Sept 2006.)

The perceived risks of research are liable to change at different stages of the research process, and they appear in a different light depending on the angle from which they are considered. For the individual researcher, the risk-taking begins with the choice of research topic: an overly ambitious set of targets and the complete avoidance of risks may both prove to be problematic (cf. Chapter 2). Risks at the start-up stage include those of failing to secure sufficient funding and competent research personnel. The research work itself may involve risks that have to do with the researchers themselves and with outsiders (fieldwork in crisis areas, laboratory work with hazardous substances, medical in vivo experiments, etc.).

In research plans, risks of failure are particularly conspicuous in the case of high-budget research projects that nevertheless cannot guarantee tangible result. One example is provided by research in fusion energy, where a breakthrough would revolutionise science and society, but no such breakthrough can be predicted with certainty.

Another example of a "high-risk project" is alternative research that challenges prevailing theories and research practices and that may lead to paradigm changes in theoretical thinking, but equally to a complete dead-end. Higher than normal risks may also be involved in research projects that apply methods or datasets that have not been tried in that particular area before, as well as in interdisciplinary projects. Individual research projects may fall into one or more of these categories.

If and when a research project does come up with results, the risk here is that they fail to elicit any response or that they are rejected by the academia. In applied research the risk may be that the intended technology cannot be produced, or that it fails to meet the market expectations (cf. Tekes). There are also various indirect risks. For individual researchers, the orientation to a certain line of research, indeed to research in general may constitute a risk to their careers. Seen from the funding agency's or research environment's point of view, taking the wrong strategic choice constitutes an even greater risk than the failure of an individual research project; this risk may materialise if a field of research that has been sponsored fails to live up to its quality and impact expectations. One final type of risk that warrants mention are the social risks or even threats presented by research and its applications, for instance in the case of nuclear or gene technology (Beck 1990, 178–185; *Creative System Disruption* 2005).

4 The Concept of Breakthrough Research

The term "risk research" is used in several different meanings. It may be used to refer to research that is concerned to explore risks, or to studies that are considered in themselves to involve risks. The latter may have to do with external risks associated with the research itself (cf. above), with the high probability of failure that is built into the research design itself, or simply with the poor standards of research work.

In the field of international research funding, the concept of high-risk research has recently been adopted to refer in positive sense to radical new research approaches that involve a distinct and pronounced risk of failure. Discussions at the Academy of Finland have tended to use the term "high-risk funding" (riskirahoitus).

The thinking that informs these discussions is that if research systems are to retain and strengthen their international competitiveness through scientific renewal and reform, then it is imperative to lower the threshold for new ideas that can potentially lead to scientific breakthroughs. It must be accepted that this kind of research involves an exceptional risk of failure. It is important to bear in mind that failure may mean very different things in different disciplines: the "big science" requiring an expensive research infrastructure, the meaning of failure is inevitably different from its meaning in multidisciplinary projects in the humanities or social sciences.

At the National Science Foundation (NSF), the problems involved in identifying and financing innovative and other high-risk projects have been an ongoing concern since 1999. The Advisory Committee for GPRA[3] Performance Assessment, which monitors the NSF's work, has observed that there exist no clear operational criteria with which these projects can be set apart from other research funded by the NSF, which in principle should always involve scientific innovativeness risks and a potential of discoveries. Both the Advisory Committee and the NSF's own board, the National Science Board (NSB), opted to describe the area of science concerned by the term "transformative research" (AC/GPA 2005; NSB 2004; NSB 2007).

3) U.S. Government Performance and Results Act 1993.

Research Councils UK (RCUK), for its part, makes a distinction between high potential, high impact research on the one hand and new innovative interdisciplinary areas of research on the other although it deals with these two areas in parallel. High potential, high impact research is "adventurous, speculative, innovative, exciting, creative, radical, groundbreaking, precedence setting, unconventional, visionary, challenging, ambitious, uncertain, mould-breaking or revolutionary". This kind of research can be interdisciplinary, but it can also represent established disciplines. Research Councils UK is reluctant to use the term high-risk research because this may carry negative associations with methodologically weak research. (RCUK 2006.)

	Researcher	**Reviewer**	**Decision-maker**
"High ambitions"	Applies unconventional ideas and aims for significant results	Rewards innovative research plans and accepts shortcomings in proposals	Looks to achieve scientific breakthroughs with funding and tolerates failures, aims for dynamic science and research
"Fear of risks"	Conservative choice of subject designed to secure funding	Emphasises scientific quality, merits and viability	Aims for certain, measurable results, strategic goals and predictable changes

The Natural Sciences and Engineering Research Council of Canada (NSERC) has also provided funding for "high-risk research" (see p. 21). In a review of its research funding programme, NSERC defined the concept of risk on a relative and individual basis, high-risk research assuming different meanings depending on the field of research. Generally, however, its characteristics were thought to include unconventionality and an uncertainty of results. Attitudes to risk-taking depend just as much on the cultures prevailing within each discipline as on the attitudes of science funding agencies. (NSERC 2003.)

In the examples above, the risks of research are approached from the point of view of research funding bodies. However, the attitudes taken by a certain funding body or agency to risks cannot be considered as monolithic. Following Glyn Holton (2004), risks are divided and concentrated in different ways in the review and decision-making processes, and they will be influenced by the roles of different agents and by their expectations. The attitudes to risks may be illustrated as in the table above.

The way that risks are taken into account in the organisation depends on the specific combination of different attitudes to risk-taking within that organisations. Scientists may come forward with radically new ideas and research questions, but the outside reviewers may not necessarily be impressed. On the other hand, an overly cautious approach on the part of researchers may detract from the scientific innovativeness of proposals, even though the reviewers and decision-makers might be prepared to support it. Even if a proposal with breakthrough potential gets an excellent review, that will not necessarily guarantee funding because its high costs, for instance, may entail too great a risk. The positive or negative attitudes to risks in one single group of people is thus reflected in the whole process.

The chart above illustrates two extreme opposite mindsets to risk-taking: on the one hand a daring drive to achieve high ambitions, and on the other hand an overly

cautious stance to avoid any and all risks. In real life it is reasonable to assume that researchers will be inclined to avoid these extreme positions and steer towards the middle of the road. Furthermore, as was pointed out above, the individual's propensity to take risks may vary depending on the specific context.

Given these different concepts and different attitudes to risks, it is problematic to try and define breakthrough research by reference to risks alone. If there is a strong negative climate of attitudes towards risks, then the label of a "high-risk researcher" (or an agency that finances high-risk research) may well be considered a burden. The terms used by funding bodies in North America and the UK reflect a positive attitude to risks in that they refer to the potential impacts and outcomes of a scientific breakthrough. For this reason, instead of talking about high-risk research, I have chosen here to use the term "breakthrough research" to refer in a positive sense to scientific research that promises significant results and that may be regarded as exceptionally creative, but that at the same time involves pronounced uncertainties. Breakthrough potential may be identifiable even in the research plan, whereas a scientific breakthrough may only be detected afterwards. The reformative or innovative nature of research, its potential significance and its risks are determined in the processes of interaction among the various actors involved.

5 Breakthrough Research and the Scientific Progress

Breakthrough research ties in closely with the broader debate and discussion on the nature of scientific innovation and the progress of science, which can only be touched on briefly here. In research, novelty and originality are regarded as key values. Breakthrough research at the cutting edge of science is thought to stimulate and promote the scientific progress and guarantee its diversity. However, novelty and originality cannot be ends in themselves in research, but new ideas must always be tested against earlier knowledge and subjected to scientific critique, or as Karl Popper would have it, falsified, before they can be accepted as scientific (Popper 1992).

The production of scientific ideas, understood in broader terms than just development and problem-solving, is based in part on unpredictable processes and advances through shifts and leaps of understanding. It therefore always involves an element of tension between trends working to maintain the old balance and those working to expand upon new departures. New problems may initially appear as unspecific "weak signals". These can only be tackled if one has a sound command of the earlier knowledge, but also the ability to depart from it and from immediate cost-benefit thinking. Creative processes are inherently self-directed, but they are also slow and beset by uncertainty. Scientific creativity always requires an element of risk-taking that cannot be accurately modelled. Ultimately, the meaning of creativity will be determined in and by the social context, and in individual instances it is possible that it can only be ascertained ex-post. (Häyrynen 1994.)

The word "innovation" has many different meanings, but it is mainly understood now as referring to research-based technological development and its commercial

application. Scientific innovations, then, refer more specifically to those that advance the progress of science, to new insights that reshape the foundations of research. A scientific breakthrough refers to success in attempts to resolve a significant and extensively researched problem, thus highlighting the nature of science as an exercise in problem-solving.

Another common concept is scientific discovery, which hints at the disclosure of some significant aspect of reality. The most wide-ranging form of scientific reform is a scientific revolution, which completely transforms established ways of thinking (see below). The terms used reflect differences not only in the depth and intensity of scientific breakthroughs, but also in their user's concept of science.

According to sociologist of science Robert Merton, the reward system in science emphasises the novelty value of research results. Originality is the key value of science on which the scientist's reputation rests and on the basis of which they are rewarded. However, this is not unfettered but constraints are imposed by what Merton describes as the general norms of science, such as universalism and scientific communism (Merton 1973; 1962). Originality is a flexible concept and it varies in degree and intensity; it may refer to the creation of new theories, to the improvement of an existing theory or to providing a better description of a known phenomenon (Gulbrandsen 2000).

Original new ideas sanctioned by the scientific elite are common assets for the academia, and the breakthroughs produced by those ideas will enjoy appreciation and gain rewards from both within and outside that community (Bourdieu 1988). Social networks and political considerations may also come into play in the definition of high-level scientific breakthroughs, such as Nobel prizes (Hakala 2002). Another aspect of the reward system of science is what Merton describes as the Matthew effect, i.e. the accumulation of advantage to prominent and recognised scientists, which may make it much harder for young scholars, women or researchers in marginal areas to gain recognition for their ideas.

The production of scientific ideas may be problematic from the point of view of prevailing research paradigms and norms. Paradigmatic or "normal science", to use Thomas Kuhn's (1969) well-known term, shares the paradigm's norms and underlying assumptions and is therefore more compatible with other research within that paradigm than work that radically challenges earlier science. Research within one paradigm may appear as wholly unscientific from another paradigm's point of view. Kuhn maintains that both normal science and scientific breakthroughs and revolutions leading to paradigm shifts have an important, mutually complementary role within the system of science. Kuhn's model of scientific revolutions is heavily simplified and firmly grounded in the natural sciences.

From a sociology of science point of view, radical breakthroughs in research challenge existing power structures within the science institution and therefore are liable to be criticised or rejected (Kuhn 1969; Bourdieu 1988). Tony Becher (1989) describes the academic fear of novelty as well as the tendency to defend positions that are based on the scientific community's existing hierarchies; this is reflected in peer reviewing, for instance. Becher points out that any researcher who ventures to tackle new problems is gambling with his or her career.

Scientific creativity and risk-tasking may also be suppressed by intolerance, poor management and bureaucracy (Gulbrandsen 2000). The habiti or "intellectual styles"

of different disciplines that depend on their social connections (Bourdieu 1988; Allardt 2002) may be reflected in their conservative or liberal attitudes to scientific risk-taking.

John Ziman (1994; 2000) makes a distinction between routine and radical originality, the latter of which calls into question the very foundations of doing science. In Ziman's view, originality works to sustain the diversity of research and to open up new paths of development. He refers to blind variation or serendipity as an integral part of the scientific process, which he claims to be evolutionary. Key to achieving this variation is originality, which in its most radical form may, however, cause the researcher to be stigmatised. The punishment of failure and the rejection of strange ideas reduce variation and thus deter the evolution of science.

Ziman argues that the "post-academic", relevance-driven science model and science policy threaten the very regeneration of basic research, forcing the progress of science down a one-way street and a commitment to goals that are predetermined from the outside. So while previously major breakthroughs in research may have been prevented by the internal rigidities of the science institution and its opposition to change, the obstacles now derive increasingly from mounting calls for immediate impact and effectiveness and from a path dependency created by strategic goal-setting.

Top-down science policy or market-driven control takes an instrumental approach to research from above and may therefore disregard significant breakthroughs emerging within the scientific community if these occur in areas considered unproductive or if their time horizon is too long (Ziman 1994; Ziman 2000; cf. Gulbrandsen 2000; Patomäki 2005). It is contrary to the market logic to invest in high-risk research that is riddled with uncertainties (Miettinen et al. 2006). Forceful external intervention can ultimately lead to "epistemic drift" (Elzinga 1997).

A common criticism against the research model that is based on foresight and top-down science policy control is precisely its avoidance of risks. Ilkka Patomäki, for instance, has pointed out that true risk-taking is possible only under conditions where science can retain its autonomy and where mistakes and errors are accepted. "It is paramount that there is the opportunity to try out paths of research that may perhaps never yield results." (Patomäki 2005, 160).

6 Breakthrough Research in International Research Funding

In the past few years, not only scientists and researchers but also research funding organisations have begun to discuss and debate the significance of breakthrough research to science and to look into ways in which they could support it. As the competition in research and among research systems has continued to intensify, so the need for turnover and regeneration in science and for new breakthroughs has become more and more apparent. There is a strong body of opinion that one of the key obstacles to achieving these breakthroughs lies in what is regarded as an overly

conservative peer reviewing system. This chapter sums up the discussions that have been going on among and inside research funding organisations.

North America

In 2004, the National Science Board (NSB) observed that there existed no criteria for the identification of what it called transformative research. This kind of research was considered to require exceptional timetables, acceptance of a higher than normal failure rate and different kinds of funding principles than research that was known certainly to deliver results.

For the NSB, the key was to revise and develop its merit review process so that it was more accommodating towards transformative research. It took the view that this research could be supported among other things by shifting the emphasis from project reviews to the evaluation of applicants, by setting up dedicated merit review panels, by increasing interaction and exchange with researchers in the review process, by increasing the proportion of funding allocated to transformative research in all disciplines, by improving the skills of Program Officers to identify transformative research, by communicating to the scientific community that the funding agency was interested in supporting new breakthroughs and by developing tools and methods for assessing the short and long-term impact and effectiveness of new breakthroughs. (NSB 2004.)

The National Science Foundation (NSF) has approached the problem by a funding instrument called Small Grants for Exploratory Research (SGER; the maximum grant is currently USD 200,000). Individual panels have been authorised to allocate up to 5 per cent of their total funding to SGER projects, but in practice no more than 0.4 per cent of their budgets have gone to this purpose.

In its internal assessment of innovative and high-risk research, the NSF took stock of all the projects it had funded earlier to identify scientific breakthroughs ("nuggets") and to develop new criteria for the allocation of special funding to transformative research. This work led to a draft set of criteria on the basis of which the projects reviewed were grouped into three categories: 1) new and untested ideas; 2) projects showing great promise but also involving a high technology risk and a high risk of failure; and 3) innovative projects running counter to the prevailing theory or paradigm. (AC/GPA 2005.)

The most difficult questions remained the same: the identification of transformative research, the financing of that work and the conservatism of the evaluation process. It was concluded that scientific breakthroughs could only be identified after extended periods and that this required some kind of yardstick anyway.

In spite of the inherent complexity of the problem, the solution proposed was a simple and indeed effective definition of transformative research. The most crucial role in applying this definition would be played by Program Officers, who therefore would require special guidance and training. It was separately stressed that budget constraints should not be allowed too heavily to influence funding decisions concerning these kinds of projects.

The evaluation group proposed the following definitions for the identification of transformative research:

Innovation. Significantly more forefront, novel, and transformative than other proposals in the field.

Risk. The proposal may have significant technical risk (including risk to the PI safety), a high probability of failure, it may be untried, and/or it may be contrary to current theory. If there is significant risk to society that a result might be harmful (e.g. health, environmental, safety, etc.), this should be explicitly documented, and flagged for a policy-level decision prior to funding.

Reward. The proposal has significant economic, intellectual, societal, etc. return on investment.

Overall Rating. A proposal would be considered "innovative-high risk" if it is clearly innovative beyond other proposals in the field, might have substantial risk, and offers the potential for outsized returns on investment.

The group recommended that transformative research be supported by increasing the use of the SGER funding instrument. The promotion of this research is a prominent element in the NSF's latest strategy (NSF 2006).

The latest evaluation report on the NSF's operation says that the recommendations issued in the previous report have increased the proportion of transformative projects in its funding, but not to a significant extent. According to the report, the process of project evaluation allows for the funding of new transformative projects above all at the discretion of Program Officers. The evaluation group recommends that for future purposes the NSF adopts a "risk portfolio" with a balanced mix of high-risk, transformative research, important but less risky projects and innovative research projects that contribute to strengthening national competitiveness. As the group explains: "The first two are converting dollars into knowledge and the third converting knowledge into dollars." (AC/GPA 2006.)

In its 2007 draft report the National Science Board says the main problem in financing new transformative projects is the lack of faith on the part of applicants. It recommends that the National Science Foundation adopt a new funding instrument under the heading of *Transformative Research Initiative*, which should help to get the message across that the Foundation is indeed committed. (NSB 2007.)

A bill (National Innovation Act 2006) has been introduced in the US Congress according to which federal agencies funding research in science and technology are to allocate 3 per cent of their budgets to technologically challenging, multidisciplinary frontier projects under an innovation acceleration grants programme. The bill remains in negotiation and it is unclear whether it will be enacted into law.

Among the other science funding agencies in the United States, the National Institutes of Health (NIH) has launched its own *NIH Director's Pioneer Award* in 2004, while the Defense Advanced Research Projects Agency (DARPA) under the US Defense Department has also channelled a small part of its funds to high-risk interdisciplinary research.

The US Department of Energy, which has extensive research operations, has acknowledged in its strategy the need to support new frontier projects in the field of energy research with a view to securing the country's competitive edge in science

(DoE 2006). The Juvenile Diabetes Research Foundation International (JDRF) awards special innovative grants to research proposals that have submitted novel and innovative hypotheses and that cannot provide the research evidence that would secure them conventional funding (JDRF 2007).

The Natural Sciences and Engineering Research Council of Canada launched in 2002 a funding initiative called the *Innovative Ideas Program*. Three-year funding is made available to new breakthrough projects on the criteria of innovativeness and high risks associated with the project (NSERC 2002). In 2006, it has started up a new, more flexible funding programme called *Special Research Opportunities*. Based on the criteria of risk-taking and the possibility of breakthroughs, the purpose of this programme is to support new research subjects or new kinds of research cooperation (NSERC 2006).

United Kingdom

Following the examples set by the United States, the UK Treasury proposes in its science and innovation investment framework for 2004–2014 that funding be stepped up for interdisciplinary as well as "high-risk" research. A major obstacle to the latter, according to the framework programme, is the peer review system that avoids risks and that places too much weight on scientific merits at the expense of the potential applications of research. Interdisciplinary research, for its part, is hampered by a funding and review system that is based on a rigid classification of disciplines (HM Treasury 2006).

In its statement on the framework plan, Research Councils UK (RCUK) points out that funding agencies in the country do in fact sponsor "adventurous" research, although it admits that researchers are overly cautious with their funding proposals. Reviewers, too, show some tendency to conservatism, which is further encouraged by the relative scarcity of funding. RCUK does not believe that a greater emphasis on the utility and applicability of research or special funding programmes for focal areas of research would encourage an increased amount of high potential, high impact research.

Some Research Councils in the UK have allocated dedicated funding to novel research projects, but RCUK suggests that they should be promoted on the mainstreaming principle in all fields of research and in all types of funding. This should include the possibility of flexible long-term funding for research pursuing new innovative directions. It is important that applicants are fully aware and informed of the funding opportunities available and of the willingness of funding organisations to take risks when the research offers significant new potential. This should be made clear in the application guidelines.

The peer review process should give closer attention to the risks involved in the proposed research projects and to risk management. It is important here to make a distinction between "honourable failure of risky research", such as the failure to resolve a difficult research problem in spite of resilient efforts, and failure that is due to poor organisation and delivery.

RCUK does not accept that encouraging creativity and innovation in research proposals is the sole responsibility of funding agencies. A major source of difficulty, it maintains, is the concern of young researchers about their career prospects and their tendency therefore to lean towards the safety of mainstream. More radical research

consequently remains the preserve of more well-established scholars. Another cultural factor that is thought to inhibit innovation is the emphasis in the review process on track records in publication.

With respect to the relationship between breakthrough research and interdisciplinary research, RCUK insists that the automatic assumption cannot be made that multidisciplinary or interdisciplinary fields of research are more innovative than more established fields, but even these fields can and do produce great advances once they reach maturity and critical mass. Breakthrough research in all fields and new and emerging fields of research are, however, parallel issues and constrained by similar factors. They should also be encouraged by similar means, such as flexible long-term funding programmes. The key difference is that whereas interdisciplinary research should set out its strategic challenges and commitments in advance, breakthrough research should remain open in this respect. (RCUK 2006.)

Sweden

In Sweden, the Foundation for Strategic Environmental Research (Mistra) has indicated that in addition to major interdisciplinary projects, it is committed to supporting "new ideas and high-risk projects" (*Svensk forskning* 2006, 17). In 2006, the Swedish Foundation for Strategic Research, the Swedish Research Council and VINNOVA (Research and Innovation for Sustainable Growth) have supported research in the field of medical engineering, with a special emphasis on "high-risk projects" and "new, bold ideas" (Swedish Research Council 2006). Furthermore, Formas (Swedish Research Council for Environment, Agricultural Sciences and Spatial Planning) sponsors interdisciplinary research as well as creative and innovative projects that cut across disciplinary boundaries (Formas 2006).

European Union

In 2004, the DG Research of the European Commission appointed a high-level working group to monitor implementation of the recommendations of the Lisbon strategy. In its report entitled *Creative System Disruption* (2005), the working group observes that one of the main problems for the European R&D system is the cultural tendency in Europe to avoid risks (as compared to the United States). On the other hand, European research and development places too much weight on the immediate utility of research results at the expense of longer-term impacts, interdisciplinarity and diversity.

Breakthrough research was not separately included in the working group's remit, but the group did recommend that long-term funding for basic research be stepped up and that attitudes to risk-taking be revised particularly in those fields of technology where benefits can only be expected in the distant future and where it is impossible accurately to predict the future. It placed the main emphasis on disruptive areas of research where European R&D can be expected to achieve a competitive edge over its rivals. The report and its conclusions have subsequently been used in the report of an independent expert group on *Creating an Innovative Europe* (2006) under the chairmanship of Esko Aho.

The challenge of securing more funding for European basic research has now been taken up by the European Research Council (ERC), which under the Seventh Framework Programme's Specific Programme of Ideas is committed to promoting

researcher-driven "frontier research". This will be achieved by supporting high-risk interdisciplinary basic research whose results cannot be accurately predicted. One of the overarching principles in the ERC's peer review process is to aim for true and fundamental transformation in European science:

"The ERC has been set up with high hopes and great ambitions, and has set amongst its goals the instigation of *transformative changes* in the European research landscape. The Scientific Council aims to set new examples and standards by sending forceful signals for such transformative changes that track and support changes in the sciences themselves." (ERC 2006.)

According to the ERC's programme, this will be achieved by encouraging interdisciplinarity, proposals in new emerging fields and by high-risk, high-gain proposals (ERC 2007).

Conclusions

Breakthrough research and the risks it involves have recently been propelled to the centre of discussion in various countries at the same time. There is a broad consensus about the importance of this work to the reform and development of science and by the same token to innovation. Likewise, it is widely felt that conventional evaluation criteria and peer review processes do not fully and properly accommodate high-risk research in today's intensely competitive market for research funding. Most funding bodies take the view that the consideration of breakthrough research should not be confined to predefined priority areas. However, there is as yet no agreement on how to identify breakthrough research and how it should be encouraged by means of research funding. It seems the main options are as follows:

Allocation. Breakthrough research is encouraged by the allocation of dedicated or general programme funding to research issues of special current interest or to strategic priority areas (NSF, NIH, NSERC, RCUK in part, Formas, Swedish Research Council, etc.).

Separate-lane system. Breakthrough research is given separate treatment from other research funding regardless of the discipline, applying a separate set of criteria that favours innovation and tolerates risks (NSF, JDRF, Mistra).

*Rewards***.** Breakthrough research, both successful and failures, is afforded adequate recognition in the assessment of scientific merits and scientific quality (RCUK).

Mainstreaming. The criteria for research funding are modified so that they favour scientific innovativeness and are more tolerant of risks (ERC, RCUK).

The relationship of breakthrough research to interdisciplinarity is also dualistic. On the one hand, the two are to a large extent identified with each other, but on the other hand it is admitted that new breakthroughs may also happen in well established fields of science.

7 The Academy of Finland and Breakthrough Research

7.1 Earlier reviews and position statements

Discussion and debate on the funding of high-risk research has been an ongoing process at the Academy of Finland for some time now. Erik Allardt, Chair of the Academy's Central Board of Research Councils in 1988–1991, was concerned about the tendency to err on the side of caution in funding decisions: "But I couldn't help fearing that what if among the one hundred lunatics there was one genius!" (Allardt 2000). In recent years the debate on breakthrough research has taken place under the concept of high-risk research.

The debate on risk-taking gathered momentum in 2004 with the publication of the latest international evaluation of the Academy of Finland. Here, suspicions were voiced that the peer review process may discriminate against innovative or adventurous research that does not fit within the present boundaries of the disciplinary structure or that is otherwise too far removed from the current consensus (Gibbons et al. 2004, 38–39). Since the publication of this report, breakthrough research has been discussed in various contexts both within the Academy and in published reviews and position papers.

Audit by Audiator Oy

In 2004, the Academy management commissioned an auditing company Audiator Oy to conduct an internal audit focusing on practices of high-risk research. The brief report that came out of this audit looked at questions of how to define breakthrough research, funding criteria, the impacts achieved with this funding vis-à-vis the Academy's existing funding instruments, and at ways of assessing and measuring impacts (Audiator 24 Jan 2005). The audit was carried out by interviewing the Academy's President, Vice Presidents (Research and Administration), and Board members.

For the purposes of this audit high-risk research was defined as consisting of projects that involve "a high potential for innovative, new and even revolutionary scientific ideas, but also a greater than usual threat of failure" (2). The interviewees thought that basic research contained an inherent element of risk, but some of them nonetheless believed it was necessary to have a dedicated funding instrument to support scientific innovation, young researchers and research at the interface of different disciplines.

The primary criteria for this kind of funding would be its scientific innovativeness and sound research idea. On the other hand, less than usual weight would be placed on the flawless presentation of research objectives, on the researchers' earlier merits and on the application fitting unequivocally into the field of research concerned. Some suggested that if a new funding instrument is indeed created, a separate panel should be set up to assess the risks involved.

There was some support among the interviewees for the allocation of breakthrough funding to strategic priority areas and interdisciplinary research. This would effectively restrict the number of projects financed because otherwise there

would not be enough money to go around to individual projects. One of the ideas floated was a "high-risk research portfolio", with projects singled out for separate consideration from mainstream proposals.

According to the interviews, breakthrough research would consist of exceptionally innovative basic research. Other possible hallmarks of breakthrough research listed by Audiator Oy were as follows:

1) Researcher's or research team's lack of experience or merits
2) A well-established researcher or research team moving to a new field of inquiry where they have no earlier merits to their name
3) The application of a set of methods from one discipline to a completely different field
4) The development of new methods and technologies that may not lead to applications until far in the future
5) Research aimed at overthrowing prevailing theory
6) Globally new and untested idea
7) Research that cannot be completed with the resources of just one discipline in Finland; research that requires the collaboration of researchers from different disciplines.

Most of the interviewees expressed the opinion that breakthrough research called for a peer review process, in spite of the latter's tendency to avoid risks and to overemphasise the importance of merits. It was thought that the value added from financing breakthrough research was based on financing such high-level proposals that otherwise would stand little chance in the highly competitive funding situation. Consideration was to be given to both interdisciplinary proposals and those confined to one single discipline. Breakthrough research would be expected to meet greater than normal reporting requirements.

The audit conducted by Audiator Oy is a tentative and preliminary overview of the subject that has no connection with the broad international debate that has been going on about breakthrough and high-risk research. It served mainly to canvass opinions among Academy management. In the discussions below, the results of this audit, as well as those of other earlier surveys commissioned by the Academy, are used as background material rather than as firm starting-points.

Report by the Funding Instrument Working Group

Chaired by the Academy's Vice President (Research), Dr Anneli Pauli, the Funding Instrument Working Group looked in its report (2005) at the possibility of creating a new funding instrument for breakthrough research. Discussing the criteria for breakthrough research, the Committee observed:

"Risk cannot be a funding criterion in and of itself; the criterion has to be that the funding is expected to generate completely new knowledge and to open up new scientific or technical horizons. These criteria must be defined clearly and transparently so that valid grounds can be offered for all funding decisions made" (*Tutkimusrahoitusinstrumenttien kehittäminen* 2005, 30, "Developing research funding instruments").

The Working Group offered the following rationale for the funding of breakthrough research:

"If funding is only made available to research that is rated in a peer review process as being of the highest quality, this is just playing it safe and going along with commonly accepted rules and culture. This may leave projects without funding that promote the progress of science, even though they do not in advance meet the criteria of cutting edge science. The funding of multidisciplinary, interdisciplinary or transdisciplinary research plans may also involve risks. Another instance of high-risk research is the funding of young researchers who do not yet have enough merits to warrant being funded." (*Tutkimusrahoitusinstrumenttien kehittäminen* 2005, 31, "Developing research funding instruments").

The Working Group also proposed an extension of funding for postdoctoral researchers so that monies are made available not only for the payment of their salary, but also to support implementation of their research plan. This proposal was put into effect in 2006 in conjunction with a broader reform of the Academy's funding instruments: the so-called postdoctoral researcher's project was specifically designed to help and support the most talented postdoctoral researchers on their way to independence and where necessary to make it easier for them to set up their own research teams.

The Working Group took the view that more monies should be made available to breakthrough research in all funding instruments so that the risks taken on by the funding agency are determined by the content of each specific type of support. On the other hand, it was not considered necessary to create a separate new funding instrument for this purpose. The Working Group noted that in order to give breakthrough research better exposure, peer reviewers should be instructed to assess proposals with a view to identifying any "forward-looking new ventures or risks potentially contained in the research plan" (31).

Based on the foregoing, the document adopted by the Academy's Board "Foundations for research funding 2007" also included the following addition:

"Research projects with high ambitions and aimed at new breakthroughs always involve a risk of failure. This shall not constitute an obstacle to a favourable decision in reviewing proposals or in preparing and making funding decisions." "Foundations for research funding" 2007.)

Furthermore, the Funding Instrument Working Group took the view that funding for breakthrough research could also be provided for specific themes or subject areas in conjunction with calls for general research grants. On this basis, the Working Group proposed that the Academy Board or its Research Councils could allocate funding to specified subject areas in order to support projects involving a particularly high risk.

Report "Promoting Interdisciplinary Research"

Breakthrough research was also among the themes covered in the report of a research team (Docent Henrik Bruun, Professor Janne Hukkinen, Research Fellow Katri Huutoniemi, and Professor Julie Thompson Klein; see Bruun et al. 2005), commissioned by the Academy to look into the promotion of interdisciplinary research in Finland. According to the working group, many major scientific and technological breakthroughs have an interdisciplinary background. Interdisciplinary

interaction is also crucial to the development of new lines of research inquiry and by the same token to the renewal of science.

The working group proposed an alternative to the prevailing hierarchy of disciplines in what it described as a "rhizome model", which by producing greater variation and diversity in research would make science more adaptable to change. Interdisciplinary, networked peer reviews were thought to have a potentially central role in identifying breakthrough research. However, interdisciplinary research was considered to involve a whole range of problems, such as the difficulty of providing a fair and just assessment, the researcher's fears of marginalisation in the field of science and possible territorial disputes.

Action and financial plan, research impact reports

In its Action and Financial Plan for 2007–2010 (5 Oct 2005), the Academy of Finland announced that by the end of the planning period it would be drafting a strategy and action plan on "how the Academy could provide more support to research that has significant potential to create something entirely new but that at the same time involves a high risk of failure" (10). Funding would be allocated to high-level research projects that are thought to be particularly relevant to the development of science and technology and to promoting change in society, which in many cases means interdisciplinary research.

In its assessment of the impact of Academy research funding (2006), the panel of experts under the chairmanship of Professor Jussi Huttunen recommended that funding for breakthrough research be organised in line with the proposals put forward in the action and financial plan so that its targets were selected on the basis of "independent surveys and earlier as well as ongoing foresight projects", i.e. in practice on the basis of strategic research needs. A report on the impacts of natural sciences and engineering research funded by the Academy (2006), on the other hand, emphasised the significance of multidisciplinary high-risk projects and new emerging groups to the renewal of science and research.

Summary

A lively and diverse debate has been going on at the Academy on breakthrough research. The reasoning as to why this research needs to receive greater prominence has been understood more or less consistently and in largely the same way as in other funding agencies. The definition of breakthrough research, the ways in which it should be identified and the processes of reviewing and making funding decisions, by contrast, have proved to be a rather difficult task, as indeed they have elsewhere. The audit conducted by Auditor Oy pointed at a strong body of opinion in favour of separate funding based on strategic allocation, while the Funding Instrument Working Group decided to recommend the mainstreaming principle complemented by allocated funding. The observations of the Working Group on the promotion of interdisciplinary research regarding the problems and possibilities of research are largely applicable to breakthrough research as well.

Prior to 2006, the funding of breakthrough research has also been discussed at the Academy's Research Councils, and a conscious effort has been made to give closer consideration to this matter in connection with reviewing proposals for general research grants. Following the completion of the present review the Academy's Board

has clarified its position on the funding of breakthrough research at its evening session on 3 October 2006; more on this later.

7.2 Discussion round on breakthrough research

The first stage in my own inquiries consisted of a round of discussions with the Academy's Research Councils and the Administration Office's Research Units, the Academy's review and evaluation development group (ARVI), the team having responsibility for coordinating the promotion of research careers at the Academy (TUTURA) and with the Academy's information management unit. These discussions were structured around the following questions:

List of questions

1. Should the applicant's merits be emphasised in the funding of breakthrough research?
2. Should projects involving applied research be considered as breakthrough research?
3. Should funding for breakthrough research be allocated to specific thematic areas identified in advance?
4. To what extent should the quality of the research environment be emphasised in the case of breakthrough research?
5. Is there any scope for flexibility with scientific quality criteria in the case of breakthrough research; in what respects?
6. Allocation of funding or a completely new funding instrument?
7. What is the ideal funding period for breakthrough research? (1–4 yrs)
8. Should reviews and decisions be made within the Research Councils or jointly in the subcommittee of the Academy's Board?
9. Individual dedicated reviewers or a special panel?
10. Normal monitoring of finances and final report, or more interaction; what kind?

Preparations for this round of discussions were conducted jointly among others with Vice-President (Research) Anneli Pauli; the then heads of Academy Research Units, Arja Kallio, Susan Linko, Riitta Mustonen and Liisa Savunen; ARVI leader Annamaija Lehvo; TUTURA team leaders Risto Andberg and Merja Kärkkäinen; and the then leader of the research programme coordination group Ritva Dammert. The results of these discussions are reported below.

Identification

The discussants took the view that there existed no unequivocal criteria or timetable for potential scientific breakthroughs. It was thought that their occurrence and financing was largely dependent on the discipline or field of research. In several discussions reference was made to the role of interdisciplinarity as an important substratum for breakthrough research.

New research questions and an innovative research design were considered crucial to the identification of breakthrough research, but research plans were also expected

to show conceptual and argumentative clarity and credibility in general. In order that reviewers can identify new research questions, they must have a solid knowledge and understanding of the research field in question and a keen eye to detect "weak signals". Many of the discussants pointed out that new breakthroughs had a tendency to provoke contradictory reactions among reviewers.

The risks of breakthrough research were thought to lie, on the one hand, in the formulation of new research questions and in the setting of ambitious goals; and on the other hand, in the very feasibility of the research project. As well as sharing the same risks that are inherent in cutting-edge research at the highest level, breakthrough research was thought to represent an alternative line of inquiry that loosely and freely applied new perspectives and explanation models and that even could contain "crazy ideas". Risks may be associated with different stages of research (start-up, fieldwork, results) and with its different objectives (scientific and other impacts).

Opinions differed on the question as to who was thought to be the best equipped to identify potential new breakthroughs and to assess their risks: dedicated reviewers, Research Councils or specially appointed "risk panels" across Council boundaries. One strategy of identification mentioned in the discussions was to ask the applicant's self-assessment as to whether their application should be considered as high-risk, breakthrough research.

Review process

A tendency to err on the side of caution in reviewing proposals and to emphasise their details were widely thought to work against the prospects of breakthrough research. Potential breakthrough proposals, the discussants believed, were easily sidelined in a conventional review process, for they belonged to no established field of research or would be rejected as probable failures. Assessments of these proposals were often thought to be contradictory, and given the intense competition had little chance of receiving the go-ahead from the Council concerned. Some aspects of new breakthrough projects may actually be contraindications for conventional project reviews.

It was also pointed out that because of the perceived risks, the Academy had turned down projects that subsequently had become success stories with funding from other sources. On the other hand, reference was also made to research projects that the Academy had decided to fund in spite of the considerable risks and that had never produced results of any scientific value. It was not thought that the review process by default excluded high-risk research ideas, but nevertheless changes would be needed if these ideas were to be better accommodated.

The discussants did not feel that differences between disciplines or schools of thought were a serious obstacle to breakthrough research, for any such contradictions were neutralised by the use of international experts in the review process and by the general emphasis on interdisciplinarity. Some discussants did feel, however, that paradigm differences might impact the assessment of proposals in some disciplines.

The majority view was that reviewers should be encouraged to take a more positive attitude towards conscious risk-taking in research plans. Greater flexibility should be applied with respect to the formal and technical application criteria, and more emphasis placed on bold research plans at the expense of earlier scientific achievement. The view was also expressed that weaknesses in the proposals

themselves should not be seen as risks. The evaluation of other than scientific impact was not considered necessary in the assessment of breakthrough research, nor was the question of whether this research fell within the scope of strategic priority areas.

Judging by these discussions, certain minimum requirements need to be defined for potential new breakthroughs in research, not only with respect to their content, but also the qualifications of applicants and the standards of the research environment. Applicants should definitely have completed their PhD and have a sound understanding of their field of research. Research environments and research communities should support the successful implementation of the research plan. Apart from the actual research location, the discussants also emphasised the importance of domestic and international networking in research.

To improve the funding prospects of breakthrough research, it was considered a minimum requirement that the existing guidelines for reviewers be updated and revised. There was broad support for the inclusion in these guidelines of a specific "risk criterion" (scientific innovativeness is already mentioned in existing guidelines). Furthermore, in the selection and appointment of reviewers more attention should be given to their openness to new research ideas and risk-taking.

The following alternative models were suggested for the review process:

1. *Normal review procedure in Research Councils*. Interdisciplinary projects jointly reviewed by the relevant Research Councils. This is based in the recognition that breakthrough research is defined indifferently in different disciplines and fields of research. Will require more detailed guidelines for reviewers and a separate set of criteria. Potential new breakthroughs would be taken into account across the board in reviews for all forms of funding. Additional statements would be solicited in the event of disagreement. One possible problem might be that support for breakthrough research is seen simply as an extension to ordinary research funding. The model would not necessarily imply any major changes to existing practices.

2. *Separate review in Research Councils.* Potential new breakthroughs would be singled out at least from amongst proposals for general research grants and placed in a separate portfolio for consideration by a review panel handling other than mainstream proposals. This would serve to underscore the distinctive nature of breakthrough research and might support the development of the ordinary review process so as to encourage greater innovativeness.

3. *International dedicated reviewers.* These reviewers would complement the scientific expertise of the Academy's Research Councils in identifying new research ideas and allow for greater independence from the national science context. Among the problems mentioned were the difficulty of recruiting such experts, the costs involved and the possibility of information leaks. Some discussants also called for interdisciplinary generalists to work alongside specialist experts.

4. *Joint review by Research Councils.* A joint panel representing all Academy Research Councils would reduce dependence on the traditional classification of disciplines and prevailing paradigms. An altogether separate review and granting process might also give clear clarity when compared to separate decisions taken by

individual Research Councils. Problems here would include at least the extra work and costs entailed by organising the panels and the difficulty of recruiting the necessary expertise from different fields of research. The selection of projects for consideration by the joint panel might also prove problematic.

Mainstreaming, the principle that has since been adopted by the Academy Board for encouraging breakthrough research, comes closest to the first of the three alternatives outlined above.

Another question that was discussed was whether the risks involved in proposed research projects should be assessed separately from the research review process. This would be easier if separate risk descriptions were included in project proposals. The search for ground-breaking new research from amongst weaker proposals was not generally considered very meaningful. Instead, that search should look at high-level proposals that have failed to secure funding and on the other hand at the middle ground in-between these two categories of research (which was the strategy adopted in screening proposals for general research grants in 2005, see later).

Funding model

The funding of breakthrough research was also discussed against the general principles of research funding. It was generally agreed that funding for breakthrough research should not be tied to specific priority areas that are defined in advance, although there was also some support for foresighting and the definition of priority areas on that basis. The funding of breakthrough research was considered important for the Academy's external image, but there were also those who thought that the Academy should not go out of its way to encourage researchers to come up with ground-breaking new ideas, but these should emerge spontaneously from within the research community.

Opinions differed sharply on the preferred target groups for funding. Especially in the natural sciences it was thought that the main driving force of breakthrough research would have to be well-established researchers who were expanding into new areas of study: these were the people who would have both the experience and the audacity to tackle unconventional research subjects. On the other hand, there was also a strong body of opinion which argued that younger researchers were inherently more innovative and unprejudiced than senior scholars, at the same time as it was thought that funding would naturally gravitate towards the best-known scientists who nevertheless were past their prime.

It was pointed out that separate funding instruments existed for younger researchers – funding for postdoctoral researchers and posts for Academy Fellows – and that young age was not as such a guarantee for breakthrough research. Nonetheless, breakthroughs and risk-taking should be given more weight in the assessment of these types of funding as well. Most discussions arrived at the conclusion that breakthrough funding should be awarded to researchers of different ages, but the same kind of merits should not be required of younger researchers as of their senior colleagues.

Discussions on the type of funding revolved around three alternatives: mainstreaming (cf. Chapters 6 and 7.1 above), the allocation of funding, and a separate, dedicated instrument for breakthrough research. Most discussants were in

favour of the allocation of funding in connection with the issuing of general research grants on the basis of Council reviews, coupled with the introduction of a separate risk criterion and additional guidelines in other funding instruments. However, the risk with the allocation option was that it might simply be used as an extension to ordinary research funding. As for mainstreaming, there were concerns that this might remain little more than a lip service that would have little true impact on the scientific innovativeness and risk-taking in Academy-funded research.

Some support was expressed in the discussions for a two-stage application process, if a separate funding instrument were created for breakthrough research. Some discussants were prepared to consider short-term incubator or pilot funding schemes for new research ideas and related "sparring", but overall the sense was that breakthrough research needed first and foremost comprehensive, long-term funding. If no new funding instrument would be created, then according to one discussant breakthrough research should nevertheless be considered separately from other funding proposals so that there is enough time to weigh their relative merits and risks.

It was felt that breakthrough research involved a greater need for interaction and monitoring than ordinary research projects. Interaction could mean more in-depth interim reporting than ordinarily, more frequent feedback to researchers and possibly an appointed mentor or even a separate steering group. Personal presentations by researchers and project teams at the Academy were also suggested.

With some projects at least it would be important to have follow-up mechanisms in place so that information could be gleaned on scientific breakthroughs or failures and their reasons. However, there were also many of those who felt that interaction and follow-up were unnecessary and too laborious and expensive. Some suggestions were made that funded breakthrough projects should be encouraged to interact and work closely together in the same way as research programmes.

Among the international funding models for breakthrough research (see Chapter 6), it seemed that the Research Councils and Research Units voiced the most support for some kind of separate-lane system, i.e. one where breakthrough research is separately reviewed and assessed yet without the creation of a separate funding instrument.

7.3 Survey of proposals for general research grants in 2005

Proposals submitted to the Academy's Research Councils for general research grants in 2005 were screened to assess their innovativeness and risk-taking and to see what kind of treatment they received in the review process and in decision-making. This survey was done with the kind assistance of presenting officials at the respective Research Units: Jaana Lehtimäki and Sanna-Maija Miettinen (Biosciences and Environment); Raija Matikainen (Culture and Society); Anna Kalliomäki and Pekka Katila (Natural Sciences and Engineering); and Saara Leppinen and Aki Salo (Health).

The Academy's Research Units under the Administration Office contributed to selecting two fields of research or project categories for closer analysis that were considered to be representative of breakthrough research in each Council (see Table on page 40). These fields of research were as follows:

- *Research Council for Biosciences and Environment*: Biosciences/multidisciplinary proposals and Biosciences and Environment/Culture and Society social and environmental research

- *Research Council for Health*: Pharmacy and public health

- *Research Council for Culture and Society*: Linguistics and media studies

- *Research Council for Natural Sciences and Engineering*: Physics and information technology

The examination focused on proposals for research grants and on the reviews they received from experts. The aim was to identify projects that advanced essentially new perspectives that had exceptionally ambitious targets, that took conscious risks or that promised extraordinarily significant potential results. It should be repeated that the meaning of these criteria varies in different disciplines and that there are marked differences in the intensity with which they are exhibited. It may be questioned whether there are such disciplines within Finnish science where a single research project could revolutionise international science, or conversely whether it is possible to have a scientific breakthrough on the Finnish national scale.

It was not possible within the confines of this analysis to study the actual novelty value of individual projects or to assess their potential impacts in the fields concerned, or indeed to explore the background of the criteria applied by the reviewers. For this reason, the survey could not be limited to projects with potential for an international breakthrough. The definition of what was to be included had to be more flexible, but on the other hand it could not just take in all basic research that generated new knowledge and that involved a risk of failure.

As in the US National Science Foundation's review, it was impossible here to put forward any exact definition of breakthrough research; we had to content ourselves with just a loose set of criteria. Following the NSF example, three different aspects were emphasised: first of all how novel, unconventional and ambitious the research question was; secondly the risks involved in the research; and thirdly the possibility of significant and far-reaching results.

Special attention was given to how the innovativeness, risks and potential significance of the research were presented and how they were weighted against each other in the expert statements and how they were taken into account in the funding decisions. This provided the platform for us to try and identify the preferences and priorities in each field of research with respect to these three criteria and their mutual relationship.

The review covered all proposals in 2005 for general research grants in the selected fields of research, the final statements from panels and experts and in some cases preliminary statements as well. In these statements, special consideration was given to those projects that were described by the reviewers as exceptionally innovative and as involving the potential for significant results as well as specific risks. The aim was to identify both projects that the reviewers clearly regarded as breakthrough research but that had not been funded, and on the other hand those projects that despite the risks mentioned by the reviewers were given the go-ahead.

Based on the first screening it was clear that it could not be inferred from proposals that received weak reviews (a grade of 1–2 on a scale from 1 to 5) or from the expert opinions on those proposals whether or not the proposed project represented breakthrough research. Most poor reviews were due to a flawed research plan which did not give a clear enough picture of the intended project. Therefore, the final analysis only included those projects that received a grade of 3–5. Special focus was given to proposals that received conflicting preliminary opinions or final expert statements, that received much lower grades in the final panel evaluation than they did in the preliminary opinions, or that were not funded even though they received good reviews.

Among the terms used by the reviewers to describe what they regarded as exceptionally innovative projects were "original, novel, ambitious, innovative, exciting, unique, forefront, transformative, cutting edge" or "breaking science".

The terms "original, novel, forefront" and "unique" were used to position the research plan vis-à-vis the current situation in the broader field of research. On these criteria the project is clearly distinguished from standard research and is more advanced. The term "ambitious" was used to describe a research plan or applicant that was considered intellectually more audacious than usual. The terms "transformative, cutting edge" and "breaking science", then, hinted most directly at the possibility of scientific breakthrough. Even though scientific innovativeness is explicitly mentioned as a review criterion, the term "innovative" was used rather loosely in the reviews to describe both breakthrough research and proposals that were thought to have significant application potential.

The term "original" was used in both a positive and a negative sense. "Novel" means something that is of a new kind or quality, but does still not imply a scientifically revolutionary perspective. The term "ambitious" may also be used to refer to the high ambitions of the research question, while the research plan otherwise may not necessarily live up to the same standards. The exact meaning of different terms can only be deduced in their respective contexts.

The reviewers used subjectively valued terms such as "exciting, unique", etc. when they wanted to set a research project apart from ordinary, incremental research in the field.

The statements were also searched for references to the risks involved in research. If the flaws and shortcomings of the research plan itself are not counted as a risk, a distinction can be made between seven different risk categories:

1. Risk related to the *research objectives*, such as whether the objectives are realistic and attainable in the first place or whether failure is very likely

2. Risk related to the *research methods*, such as the use of an untried method, a dataset that is poorly fitted with the method or the wrong kind of research tools

3. Risk related to the *field of research*, such as the sense that the subject is too marginal or (in Finland) in an orphan situation, and on the other hand that the field is too crowded

4. Risk related to *personnel*, such as the lack of scientific merits or the anticipated weakness of the manager's role

5. *Ethical* risks related to the research, such as data protection issues

6. The risk connected with *interdisciplinarity*, i.e. weak links between researchers or participating projects representing different fields of science in interdisciplinary or multidisciplinary programmes

7. Risk related to *resources*, i.e. the research cannot be completed with the resources projected in the research plan or on timetable.

The shortcomings of the research plan may have been due simply to lack of experience on the part of the applicant or time pressures. According to the presenting officials there are certain fields where applicants prefer to play their cards close to their chest for the fear of information leaks, and consequently their research plans may remain rather vague. In the case of major consortia, restrictions on the number of pages may also cause important details to be left out. Differences between disciplines or paradigms might influence the assessment of research plans and their standard for instance via the reviewers' expectations of how their methods ought to be described.

Based on the reviews they received, the proposals were placed into a four-cell matrix where the horizontal axis consisted of their acknowledged innovativeness and the vertical axis of the presumed risks involved. Summaries were prepared of the written statements and their grounds and reasoning regarding the innovativeness and risks of the proposed projects.

Later on the material was re-examined together with the respective presenting officials in 2005. In this connection the interpretations of the innovativeness and risks of the research plans were revised on the basis of the background information received from the presenting official about the field of research, about the applicant or about the Research Council's funding decisions. At the same time, a profile was created of breakthrough research typical of each field.

Research Council for Biosciences and Environment

Based on the assessments of the presenting officials in the BIO panel, one of the categories selected for closer scrutiny among the proposals for general research grants submitted to the Research Council for Biosciences and Environment in 2005 was that of genuinely multidisciplinary proposals. The second example came under both the Research Council for Biosciences and Environment and the Research Council for Culture and Society, for it consisted of proposals to their joint ENV&SOC panel. According to the respective Research Units the proposals selected for review by this panel were those that in both Research Councils addressed topics at the interface of environmental and social sciences and that were often multidisciplinary by their nature. The majority of these proposals were addressed to the Research Council for Biosciences and Environment.

The BIO panel had assessed eight multidisciplinary proposals. One of these proposals was a consortium of four component projects, which were reviewed as a single proposal. The reviewers regarded the consortium proposal as ambitious, but according to the presenting official it was difficult to assess its scientific innovativeness on account of a rather general research plan. In spite of getting the highest mark (5), the consortium remained without funding.

Among the other projects reviewed by the BIO panel, one was identified as a scientifically high quality proposal and as involving risks. The reviewers had praised the project for its careful planning and strategic insights, and they regarded the applicant as competent. The risks of the project were associated with its ambitious goals and the inadequate description of its methods. The final grade given to the project was a three, and it did not receive funding approval.

Of the two multidisciplinary projects that were funded in the biosciences, one was rated as exceptionally innovative and the applicants were regarded as competent, even though the project leader was young and did not yet have a very impressive list of publications. The other potential risk factors had to do with the proposed methods and interdisciplinary cooperation. The project involved primarily applied research and it received a four.

If the consortium proposal is excluded from this examination as a borderline case, the multidisciplinary proposals reviewed by the BIO panel can with certainty be said to have included two potential high-risk, breakthrough projects, one of which was funded and the other not. The total number of funding decisions in this category of proposals was two, meaning that the approval rate for breakthrough proposals was 50 per cent and for other proposals 17 per cent.

The joint ENV&SOC panel processed a total of 30 proposals, 18 of which had been addressed to the Research Council for Biosciences and Environment and 12 to the Research Council for Culture and Society. Among the former, two were identified as potential breakthrough projects, one of which had been submitted by a researcher who in the presenting official's view was very much underrated. This researcher's application was innovative and of a high quality, but according to the reviewer suffered from lack of integration, a typical weakness in multidisciplinary proposals. There were also problems with respect to time use and methodology. The project received a grade of four and it was financed.

The other high-risk project was also thought to promise great potential, although it failed to provide accurate enough documentation of the spending of its funds. The applicant was young but nonetheless considered competent. The project was graded as a four, but it did not receive funding approval because it had existing Academy funding.

Among the ENV&SOC proposals addressed to the Research Council for Culture and Society, three were regarded as potential breakthrough projects. All three were considered exceptionally innovative. One of the applicants was a young researcher whose exceptionally high-quality proposal included a methodological risk. According to the review this risk only added to the scientific value of the presentation. In spite of getting a five, the proposal did not receive funding approval. This proposal was for a follow-up of a project that had previously been funded by the Academy.

The second applicant was competent and had published extensively. The project proposal was of a high quality, but the data presentation was rather limited and the methodological description incomplete; therefore it did not receive funding approval. The third proposal was also from a competent researcher, but again there were shortcomings in the description of methods. However, this project received a four and was funded.

Among the 14 ENV&SOC proposals filed with the Research Council for Biosciences and Environment, two may be identified as potential high-risk, breakthrough research, and one of them received funding approval. Three of the eight

proposals addressed to the Research Council for Culture and Society were potential breakthrough projects, and one of them was funded. In other words, five of the total of 30 proposals submitted to the joint panel were exceptionally promising but high-risk ventures, and two of them were funded, while the total number of funded projects was four. The approval rate for breakthrough research proposals reviewed by the joint ENV&SOC panel was 40 per cent, for other projects 8 per cent, so breakthrough research had a very strong position indeed in this pronouncedly interdisciplinary panel.

Research Council for Health

The two fields of research that were reviewed under the Research Council for Health were pharmacy and public health. In the case of public health research, the examination focused on proposals reviewed in panel number four (social medicine, epidemiology, nutrition research and psychiatry). Among these 24 projects, only one was identified as showing potential for a scientific breakthrough. This project was rated as scientifically highly significant, but its problems were the lack of national and international cooperation and comparative data. The project was given a three and it was not funded.

The examination additionally looked at a borderline case where a well-established research team had submitted a proposal on an interesting and disputed subject. However, the research plan failed to make a clear enough statement of the project's objectives, and the presenting official did not consider the application to represent breakthrough research. Among the public health proposals reviewed, none of the breakthrough proposals were funded, giving them an approval rate of 0, while among other proposals four received funding approval at an overall approval rate of 17 per cent.

Among the 11 proposals in the field of pharmacy there was likewise one potential breakthrough project. The project was regarded as highly ambitious and innovative but also as involving high risks, because its objectives may not have been attainable. The applicant in question worked on the borderline of two fields of research and had had variable success with previous funding proposals. The project was marked as a three and it did not receive funding approval. Among the proposals filed in this discipline, three other projects were given the go-ahead, giving them an approval rate of 30 per cent. On this basis it may be suggested that pharmacy was the least supportive of breakthrough research among all the disciplines included in this survey.

Research Council for Culture and Society

The two examples taken under scrutiny for the Research Council for Culture and Society were media studies and linguistics. Two of the thirteen proposals in media studies were classified as potential breakthrough projects. One of them promised a new approach to a well-established research problem and significant potential for new knowledge. This was a multidisciplinary project and its risks were associated with the integration of its component projects. The project was marked as a four and it received a favourable funding decision.

The other project concerned was a multidisciplinary undertaking that cut across two areas of research. The reviewer was concerned about the feasibility of the project in view of the vast amount of ground it proposed to cover. The project received a four

and was not funded. The number of funding approvals in the whole field of research was three: the approval rate for breakthrough research in media studies was thus 50, for other projects 18 per cent.

Two of the 18 proposals in linguistics were identified as potential breakthrough research. The research plan in the first of these proposals was rated as high-quality and innovative, but there were problems with data sufficiency, the small size of the research group and its international contacts. This was a new move by an established, senior researcher that was given a rating of four and that did not receive funding approval.

The other project was a young researcher's proposal in a field of study that involves major inherent risks. The research plan was described as multidisciplinary, transdisciplinary and international. Marked as a four, this project was given the go-ahead together with two other linguistics projects. Therefore the approval rate for breakthrough research in linguistics, too, was 50 per cent, while for other proposals the figure was 13 per cent.

Research Council for Natural Sciences and Engineering

The two disciplines reviewed for the Research Council for Natural Sciences and Engineering were physics and information technology, which at once received the largest portion of proposals in this survey. Physics received 31 proposals. Six of these proposals were identified as potential breakthroughs, albeit two of them with reservations. The first borderline case was an innovative project outside of the mainstream, but it involved no risks. The second was an ambitious and expensive project that filed a flawed proposal. Both of these projects received a four and they were not funded.

One of the other high-risk projects dealt with an important and disputed area of research. The research plan and the applicants were both rated as excellent, but they had no earlier merits in this field of research. The methods involved risks as well. The topic of the other project was also a significant and well-researched subject. This project proposed to develop a new method and it involved a particularly high probability of failure. The third project was described as interdisciplinary and innovative. This high-quality research plan was submitted by a competent team of researchers and involved extensive risks. The fourth project was part of a long-term and ambitious research effort by a highly experienced team working in an excellent research environment. Given its high level of ambitions, the risk of this project was that it might fail to reach its goals.

All four projects received the highest possible rating of five and they all received funding approval, i.e. their approval rate was 100 per cent. In addition, funding was awarded in this field to two other projects that were given a four. In other words, breakthrough research accounted for two-thirds of the six funding approvals. The approval rate for other funding proposals was 7 per cent, so amongst the disciplines included in this survey, physics gave the most favourable reviews of and the largest number of funding approvals to potential breakthrough proposals.

Information technology was by far the biggest of the disciplines included in this survey, receiving no less than 71 proposals. Three of these were identified as potential breakthrough research. The first of these proposals represented highly ambitious cutting edge research that aimed at (applied) innovations. However, the research plan did not provide a detailed enough account of the technical aspects of the proposed project, and it was marked as a three.

The second proposal was aimed at developing new methods in a theoretically important field, but the group of applicants was considered weak and the project overall received a rating of just three. The third proposed project had set itself the target of a significant technological innovation, the achievement of which was considered potentially risky. The team behind the proposal was a high-quality group, but their research plan did not go into sufficient technical detail. The proposal received a three. Therefore, funding approval was not given to any proposals in this field.

Among the proposals submitted to information technology, a few stood out that showed hints of breakthrough research but that were hard to assess for their risks or innovativeness because the proposals were not detailed enough. According to the presenting official, this was due to fears of information leaks in the fields of research concerned. Some of these proposals had received funding approval. All in all 16 of the proposals filed in this field of research were funded, giving a relatively high approval rate of 24 per cent.

Summary of results

The survey covered a total of 206 proposals (see the Table below).[4] Almost half of them or 102 were in the natural sciences and engineering fields, which must be borne in mind in interpreting the results. The total number of high-risk, breakthrough project proposals was 20, i.e. one-tenth of all the proposals received in the disciplines concerned, excluding borderline cases.

Funding approval was given to nine potential breakthrough projects in five of the eight fields of research included in the survey. Breakthrough research accounted for over one-fifth of all the funding approvals in these fields of research and were therefore overrepresented among the projects that were granted funding. A very high proportion, 45 per cent of all identified breakthrough proposals received funding approval; the average approval rate for other projects was the normal 17 per cent.

The data reviewed cover only a small part of the fields of research funded by the Academy, although the examples were selected with a specific view to representativity of each Research Council. Therefore, the observations made do not allow for any far-reaching conclusions. Nevertheless, proposals have been identified in each field under review that in the opinion of both the presenting officials and the reviewers were both exceptionally innovative, high-risk and promising significant results.

In most of the fields of research under study, these kinds of proposals have indeed received funding approval and their approval rates have been higher than for other proposals in the fields concerned. It seems that there are quite marked differences between these fields in terms of their profiles of funding of breakthrough research – at one extreme of the continuum is physics, at the other pharmacy. This seems to point at the existence of different kinds of "risk cultures" within different disciplines.

Based on this survey it seems that the Academy's existing review system is well capable of recognising and accommodating breakthrough projects. Profiling potential new breakthroughs is relatively straightforward, but only within the context of the respective field of research. It also seems that risks are defined according to their differing disciplinary contexts. Judging by their assessments, some reviewers are

4) Consortium proposals are considered here as single proposals that have been reviewed together.

Breakthrough research in 2005 call for general research grants

	Breakthrough research proposals	Funding approved	Funding not approved	Statements total	Total no. of funding approvals	Breakthrough projects as % of statements	Funded breakthrough projects as % of all funding approvals	Approval rate for breakthrough projects	Approval rate for other projects
B&E/multidisciplinary bioscience *	2	1	1	8	2	25 %	50 %	50 %	17 %
B&E/C&S / ENV&SOC **	5	2	3	30	4	17 %	50 %	40 %	8 %
Health/public health ***	1	0	1	24	4	4 %	0 %	0 %	17 %
Heath/pharmacy	1	0	1	11	3	9 %	0 %	0 %	30 %
C&S/media studies	2	1	1	13	3	15 %	33 %	50 %	18 %
C&S/linguistics	2	1	1	18	3	11 %	33 %	50 %	13 %
N&E/physics	4	4	0	31	6	13 %	67 %	100 %	7 %
N&E/information technolog	3	0	3	71	16	4 %	0 %	0 %	24 %
Total	20	9	11	206	41				
Percentage on average						10 %	22 %	45 %	17 %

* Among the proposals reviewed by the biosciences panel, the present survey focused on 11 genuinely multidisciplinary proposals as identified by the presenting officials.

** In the joint Biosciences/Culture & Society panel, the survey included proposals from both Research Councils on the interface between environmental and social sciences; these were often multi-disciplinary projects.

*** Panel 4 reviewed proposals from the fields of social medicine, epidemiology, nutrition research and psychiatry. The data reviewed consist primarily of proposals representing public health research; according to the presenting official's assessment there were 24 of them.

already inclined to reward scientific innovativeness and risk-taking, and the Research Councils are also prepared to provide the funding needed by these projects. Some proposals have also been turned down on grounds of their lack of scientific innovativeness.

Breakthrough proposals come both from younger researchers who have won their spurs and from senior scholars. The proportion of junior researchers, which has been debated in the Academy and elsewhere, showed no bias in either direction. The sample of proposals studied here includes both interdisciplinary proposals and projects anchored to one particular discipline. This is in line with the views of Research Councils UK, for instance, who say that high potential, high-risk research and interdisciplinarity are parallel phenomena but not analogous features of research. The sample includes a couple of research projects that are clearly applied in their orientation, but potential for application is recognised in quite a few proposals.

Women are vastly outnumbered by men among the breakthrough applicants: there is just one single woman among the applicants who received funding approval and among those who did not. Women also accounted for no more than five or 14 per cent of all funding approvals in the disciplines reviewed, while the corresponding proportion of approvals for general research grants in 2005 was 32 per cent.

When the weaker proposals were excluded from the analysis, the average rating for potential new breakthrough projects was 4.0; for those that received funding approval, the average was 4.4 (the ratings for funded projects were all within the range of 4–5).

The overall assessments of the *breakthrough projects that received funding approval* highlighted the general interest value of the research question, the ambitious research approach and the detected potential for significant new knowledge. The risks identified in these assessments were the prospects of completing the exceptionally demanding research projects, questions related to data and methodology, ability to satisfy the requirements of interdisciplinarity, practical problems with division of labour and management and difficulties with the practical applications of results. These aspects are closely in line with the characteristics of scientifically innovative and high-risk projects identified by the US National Science Foundation (AC/GPA 2005), although there may be differences in what in the Finnish or US context is regarded as exceptionally innovative or potentially rewarding.

The project plans were praised for their clarity and composition. All project leaders and research teams were regarded as competent, if not always as leading figures in their respective fields, and described as internationally well networked. This also applied to younger researchers. The projects' research environments and arrangements for postgraduate education were rated as first-class.

There were 11 *breakthrough projects that did not receive funding approval*, with ratings ranging from 3 to 5 (average 3.6). The overall assessments of the project proposals highlighted the novelty and importance of the proposed research questions, their ambitious research approaches and the potential for generating new knowledge. As for problems in the research plans, the reviewers drew attention to the lack of clarity about their objectives, problems with research methods and datasets and inadequate technical details. There was also some tendency for slackness in questions of resource allocation and coordination. Overall, however, the standard of proposals was very high.

Most of the applicants, but not all, had earlier scientific achievements to their name, extensive international contacts and experience of postgraduate training. All, however, were considered to have adequate qualifications and competencies to execute and complete the proposed projects. All projects involved postgraduate training, but the extent and level of that training varied.

Some reviews considered it a weakness of the proposed projects that their objects of study or background theories remained disputed, that their theoretical orientation was too one-sided or that no earlier research knowledge was available. Overcrowding in the field of research concerned was also mentioned as a risk factor, whereas in other cases reference was made to the lack of international contacts.

Decisions to withhold funding approval were thus motivated by one of two sets of reasons: either the information provided about the project was considered inadequate, or the proposal was located at the interface of established fields of research. Both of these factors were raised during the earlier round of discussions (see 7.2 above) as possible reasons influencing the decision not to finance break-through research, so they are certainly recognised within the Academy as well. The former may be due simply to the lack of accuracy and adequate detail in the proposal or to the lack of experience on the part of the applicants, but also to the research problem being exceptionally challenging or to fears of information leaks in connection with the review. In the latter case, the reviewers accepted that proposed projects were in principle very interesting, but they had doubts about the applicant's competence to tackle the demanding research questions or about the research

approach chosen. Here the ultimate question is whether the problem lays in the project itself, or whether its assessment was influenced by disciplinary or paradigmatic differences or simply by the rejection of novelty.

7.4 Conclusions

Although tentative, the results of our survey appear very interesting, particularly with respect to the review of the proposals in 2005 for general research grants. As in the corresponding overview by the US National Science Foundation (AC/GPA 2005), scientifically innovative high-risk projects were also identified among the research funded by the Academy of Finland. Other similarities between the Academy and the NSF are that breakthrough projects have received unequal exposure and opportunity in different disciplines; that all the means and resources available have not necessarily been applied to support these projects; and that these projects have not been considered as a consistent and systematic part of research funding as a whole. Both funding agencies have been hampered by their not having a workable definition and set of criteria ahead of the review process.

During the round of discussions with the Research Councils and Research Units at the Academy, the view was widely expressed that breakthrough research can be identified and set apart as a distinct category, even though there are no explicit and unambiguous criteria. The key distinctive characteristics of this research were thought to be its scientific innovativeness and credibility as well as risk-taking with regard to tackling new questions and general feasibility.

It was thought that the peer review process allowed for some flexibility especially with regard to the technical proposal criteria and the assessment of the applicant's achievements in the field of science concerned. The funding model that won the most support was the allocation of general research grants via the Research Councils to breakthrough research combined with a revision of assessment criteria and guidelines.

Among the proposed projects that won funding approval and those that were rejected in the 2005 proposals for general research grants, a category of projects clearly stood out that were rated as exceptionally innovative and ambitious and as involving high risks. The breakthrough potential of proposals and their types of risks were determined against the background of each discipline.

The large number of high-risk breakthrough projects as a proportion of funded proposals and their high approval rate suggest that in most of the fields assessed, the risks had paid off if the applicants were well established scholars working in high-quality research environments and if the risks were related to the objectives set out in the research plan and to the proposed research methods. For projects that did not receive funding approval, the problems identified by the reviewers had to do either with flaws in the submitted proposals or with their location at the interface of different disciplines.

The projects identified as potential breakthrough research included both interdisciplinary proposals and those representing traditional disciplines, and the applicants included both recent PhDs and senior researchers. The scarcity of women researchers may perhaps be explained by their overcautiousness in setting research goals, by latent biases in the review process or by the predominance of men in the research fields under study.

This analysis seems to provide no evidence to substantiate the doubts voiced in earlier discussions that the Academy's peer review process is inherently biased against risk-taking. High-risk, breakthrough projects are recognised at the Academy as a distinct problem area and there is a willingness in principle to finance breakthrough research. However, that support could be stepped up by showing greater tolerance to minor shortcomings in high-risk proposals and greater awareness of the differences between the risk cultures of different disciplines and fields of research, by recognising projects that fall in-between different fields of research and by paying greater attention to the data protection problems experiences in certain fields. However, given the extraordinary volume of proposals received each year by the Academy, this can only happen at the expense of conventional high-level research projects.

One of the overriding concerns in the debate on breakthrough research both internationally and at the Academy has been the prospect of losing valuable new research ideas and potential breakthroughs in a funding system that gives unreserved priority to the high quality of research (e.g. *Tutkimusrahoitusinstrumenttien kehittäminen* 2005, 31, "Developing research funding instruments"). If the Academy wants to support breakthrough research without compromising on the high scientific quality and formal criteria of the proposals, the findings of this survey suggest that the emphasis on scientific innovativeness and risk-taking primarily favours excellence in research. The likelihood of securing funding with middle-ground proposals does not essentially depend on whether they propose high-risk new ideas, whereby the

1 CAUTIOUS INCREASE IN EMPHASIS	
STRENGTHS	OPPORTUNITIES
– No new tasks for personnel – Established review practices – Based on current expertise	– Funding can be made available for viable breakthrough projects – Minor Academy investment provides high-visibility encouragement for risk-taking by researchers
– No improvement necessarily achieved in the identification of new breakthrough projects – Breakthrough projects may be overtaken by other priorities	– Reform remains cosmetic and Finnish research loses diversity and capacity for regeneration compared to the competition
WEAKNESSES	THREATS
2 RADICAL RISK-TAKING	
STRENGTHS	OPPORTUNITIES
– Separate assessment of breakthrough research makes for easier identification and risk management – New kinds of review practices are created – Unprejudiced funding for breakthrough research allows for effective implementation	– Academy provides extensive and visible funding for breakthrough research – Academy review processes become more supportive of innovativeness and risk-taking – Researchers show greater willingness for risk-taking in their proposals
– No unambiguous criteria are found for breakthrough research – Separate review and monitoring process is time-consuming and expensive, increases sporadic funding – Funding for breakthrough research may attract weak proposals	– Increased proportion of failures among funded projects – Increased criticism against Academy funding decisions – Additional funding for breakthrough research detracts from funding for other high-quality projects – In spite of the investment researchers are reluctant to take risks
WEAKNESSES	THREATS

additional funding for breakthrough research may be presumed to provide only little encouragement to other than top researchers. In other words, the need to support science-advancing projects that do not necessarily meet the outward criteria for cutting edge science, as highlighted by the Academy's Funding Instrument's Working Group, will continue to exist if funding is only made available to projects with the highest scientific quality ratings.

The SWOT tables below summarise the implications of the two main alternatives of cautiously increasing the weight given to breakthrough research in the Academy's research reviews and funding decisions, and on the other hand substantially increasing the risk-taking involved in new breakthrough projects.

8 Measures

Following its discussions on innovative breakthrough research at its evening session on 3 October 2006, the Board of the Academy of Finland decided to adopt the mainstreaming principle in supporting breakthrough research, i.e. to integrate it as part of the regular review and funding process. In practice this means that instructions and information given to applicants will need to be revised and the guidelines given to reviewers as well as the review form they use upgraded and updated so that promising high-risk projects with ambitious objectives are better taken into account at different stages of the review process. The funding made available to breakthrough research may be increased not only through general research grants, but also in new research programmes and centre of excellence funding.

In practice, the impacts of the mainstreaming decision taken by the Academy's Board are likely to be equivalent to those of the "cautious increase in emphasis" option in the SWOT table above. Revised review criteria, guidelines and information may increase the prospects of potential breakthrough proposals in the Academy's review and funding process, but in the last instance this will depend upon the attitudes taken by key groups of people, i.e. researchers, reviewers, presenting officials and those who make the final funding decisions.

To make sure that mainstreaming does not remain just a lip service, a crucial first step is to revise and upgrade the criteria for breakthrough research in different disciplines and fields of research and in different funding instruments. The funding of breakthrough research is first and foremost about *strengthening the diversity of research* by adjusting the tolerances of the review process. This will also require a need of bracing oneself to meet a probable increase in the rate of failures. The priorities established by the Research Councils and enforced in the review process, taking into account their specific risk cultures, will play a crucial role in this regard.

While continuing to support new breakthrough research, it is important for the Academy to monitor the development of international models used in reviewing high-risk, scientifically innovative research projects and to assess their applicability to the Finnish system of research funding.

Review practices

One of the Academy's key evaluation criteria in reviewing project proposals is their scientific innovativeness. The decision to support breakthrough research by means of mainstreaming means at the very least that the high-risk element has to be incorporated as part of the criterion of scientific quality and innovativeness. This will require the provision of more detailed instructions and guidelines to the reviewers and presenting officials. With respect to the application of the review criteria and the selection of reviewers, it is important that special consideration is given to differences in risk cultures so that breakthrough research receives equal consideration in different disciplines and fields of research. Based on the findings of this analysis, the review process should give special attention to possible gender differences in conceptions of scientific innovativeness and risk.

The primary starting-point for the review process has to be the application's *adequate scientific quality*, which is assessed on the basis of the applicant's competence and qualifications, the clarity of the research plan, the availability of appropriate research environments and networks of contacts. However, in order to ensure the diversity of research, it is important that the criteria for breakthrough research allow for greater flexibility with respect to the researcher's qualifications or the details of the research plan, and that funding is made available to projects in different fields of research and in different kinds of research environments. This will allow proposed breakthrough projects in the middle ground to compete on more equal terms with projects that in a conventional assessment are rated as excellent.

Project reviews should give consistently more positive consideration to *creative and original research plans, conscious risk-taking and the potential for scientific breakthroughs*. These should be considered against the background of each discipline or field of research, taking account of the types of risks that are typical in that field. Risk-taking cannot be the only criterion in assessing a breakthrough project, but risks must always be weighed against scientific innovativeness and the promise of significant rewards, in keeping with the example set by the US National Science Foundation (see Chapter 6). The risks taken in research must also be ethically sustainable.

It is also important that breakthrough research is consistently balanced against the Academy's other priorities with respect to research funding, such as interdisciplinarity, equality, considerations of research career and international cooperation. In view of this it would be useful to consider the applicability of so-called *portfolio management methods* for review purposes (e.g. Gustafsson & Salo 2005). The evaluation of breakthrough research is particularly important in the middle ground between excellent projects and weaker proposals where the relative preference of different projects may be determined by several alternative sets of criteria. Portfolio management would allow for the creation of a portfolio of projects on the basis of several parallel criteria in a transparent manner and against a variable set of background factors.

Mainstreaming and allocation

As was pointed out in the report by the Funding Instrument Working Group (2005), the application of the mainstreaming principle to support breakthrough research can be complemented by allocated research funding in different research instruments. The

allocation of general research grants by Research Council decisions is certainly a noteworthy option for the funding of breakthrough research. That would not tie up too much resources and would offer extra flexibility in identifying and funding research projects. However, these allocations must not be allowed simply to become extensions to ordinary research funding, a concern that was raised during the internal round of discussions.

In the case of *other funding instruments* there are again arguments for the further encouragement of breakthrough research and risk-taking. Research programmes should emphasise the significance of new breakthrough research as a matter of principle, but it might also be beneficial to single out themes where there is a particularly strong confluence of scientific innovativeness, breakthrough potential and risk-taking. In the case of postdoctoral projects and posts for Academy Research Fellows, young researchers in particular could be encouraged to take greater risks, as indeed they are to a certain extent already. Centres of Excellence, for their part, could be encouraged to set up spin-off research teams around new research ideas by providing dedicated funding.

Monitoring

Project reporting and monitoring requirements should be stepped up for projects that receive high-risk breakthrough funding, since the progress and results of these projects may be expected to have general interest both from the point of view of research in general and from that of the Academy's operation. Breakthrough research could serve as a testing ground for interactive monitoring, where there is closer contact and exchange than is currently the case between the Academy and researchers.

Failures in breakthrough projects that have received funding should be regarded as acceptable and reportable results whose reasons should be explored in the same way as successful scientific breakthroughs. Ex-post assessments of breakthroughs or failures can only be conducted after a sufficiently long time period, several years after the project has been completed. The present analysis provides an opportunity to return to the projects reviewed here and monitor their destinies at a later date.

Communications

Apart from the availability of funding, the willingness of researchers to take risks also depends on their career prospects and on the general climate of attitudes. In its communications the Academy should be prepared both to give exposure to scientific breakthroughs and to justify its decisions for funding projects that turned out to be failures. In spirit of the Research Councils UK recommendations, the Academy can encourage researchers to submit plans for high-risk projects in their proposals not only by providing funding, but also by taking a public stance in favour of risk-taking in research, by rewarding breakthrough research and by dispelling fears of failure.

Foresighting

As elsewhere, the Academy's possibilities to provide funding for breakthrough research are very much conditioned by growing demands for the use of foresight methods in research funding, such as the Government's decision in principle of 7 April 2005 on the development of the public research system. Foresighting is an important science policy tool with diverse uses, and it can provide valuable

information on changes happening in the field of research and on its weak signals (FinnSight 2015, 2006). However, if research funding is exclusively tied to areas identified by means of foresighting techniques, as suggested among others by the Academy's report on the impact of research funding, this may easily cause path dependency and probably leads to the rejection of breakthrough projects that divert from the most probable scenarios. Such policy can undermine the Finnish research system's international competitive edge when there are sudden and unexpected disruptions in the development of science, society or the markets.

Quantitative research indicators can provide only limited forecasts of future leaps in development and they tend to favour established fields of research and researchers. However, it is crucial that research can always adapt and adjust to major transitions. Scientifically innovative, high-risk projects are about searching for alternative paths of development in which chance plays its own role. In this light the funding of breakthrough research should be seen as a necessary counterbalance to science policy steering that is based on foresighting and research indicators.

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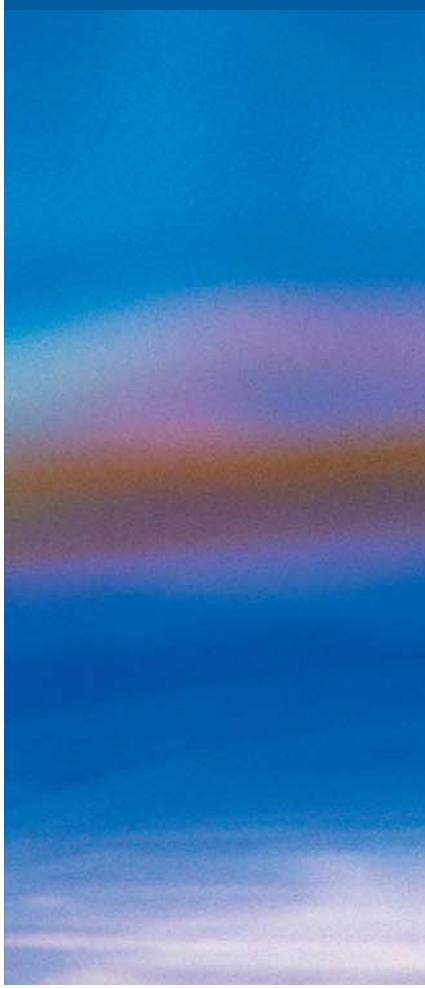

Breakthrough research is characterised by exceptional scientific innovativeness, conscious risk-taking in the choice of its research subjects and methods, and by its ambitious goals. Questions surrounding breakthrough research have received increasing attention in recent years with the continuing escalation of international competition between research systems and with the increasing difficulty of securing competitive research funding. As the funding criteria for research have become stricter and approval rates for proposals declined, the problem is whether all this will hamper the achievement of scientific breakthroughs and, therefore, the advance of science.

This report provides an international overview of the funding situation for breakthrough research and describes attitudes towards breakthrough research at the Academy of Finland. It looks at how potential breakthrough projects can be identified in the proposal review process and at how research funding should encourage researchers to present bold new ideas and take more risks.

ACADEMY OF FINLAND

Vilhonvuorenkatu 6 • PO Box 99, FI-00501 Helsinki
Tel. +358 9 774 881 • Fax +3589 7748 8299
www.aka.fi/eng • viestinta@aka.fi