THE THINK TANK
DEA
ADVANCING KNOWLEDGE

What do we know about risk-taking in science and science funding?

A policy-oriented survey of the literature

This background report was developed by the Think Tank DEA in collaboration with and with co-funding from The Independent Research Council Denmark in preparation for the development of their joint report “Risikovillig forskningsfinansiering” (“Risk-taking in science funding”).

Publisher:
The Think Tank DEA
Fiolstræde 44
1171 Copenhagen K
Web: thinktankdea.org

Copenhagen, Denmark
October 2019

Authored by:
Maria Theresa Norn, Head of Analysis, DEA

VI FREMMER VIDEN

Table of contents

VI FREMMER VIDEN

Introduction

This report presents a survey of the literature on risk-taking in science and science funding. The survey was undertaken by the Think Tank DEA and co-funded by the Independent Research Fund Denmark in preparation for the development of "Risikovillig forskningsfinansiering" ("Risk-taking in science funding"), a joint report from the Think Tank DEA and the Independent Research Fund Denmark.

The aim of this survey of the literature was not to undertake an exhaustive review but to identify key insights and empirical findings pertaining to

- Definitions of risk-taking (and related concepts including e.g. novelty, originality, creativity and innovation) in science and science funding,
- Conditions for risk-taking in science,
- The role of research funding in supporting or hindering risk-taking in science, and
- Research funding bodies' practices with a view to supporting risk-taking in science.

An assessment of the data and methods used in the studies covered was beyond the scope of this survey. Instead, the aim was to identify key themes and factors related to risk-taking in science to inform discussions on the state of risk-taking in science and the role of science funding in supporting it.

The survey of the literature is presented as a narrative review to give policymakers and science funders an overview of key themes in the literature on risk-taking in science and science funding. This approach was considered well-suited because of the heterogeneous nature of the body of work covered (see e.g. Snyder 2019; Wong et al. 2013), which addresses a number of related but distinct themes in the literature including but not limited to the nature of scientific advances; risk-taking in science; novelty, originality, creativity and innovation in science; breakthrough science or transformative science; risk taking in science funding bodies and instruments; and sources of bias in peer review processes.

In the search for and selection of literature, emphasis was placed on academic research published in peer reviewed journals and books. Selected opinion pieces and commentaries were also included. Finally, selected "grey" literature in the form of reports from e.g. science funding bodies was also included.

Literature was identified through searches in Google Scholar and selected academic databases, hereunder primarily Science Direct, Oxford Journals, SAGE Journals Online, and JSTOR. Grey literature was identified through references in academic publications and supplementary searches via Google. Key issues and factors in the literature were identified using thematic analysis, and the identified themes formed the basis for the structure of this report.

The main takeaways from the survey of the literature are summarized in box 1.

Boks 1. Main takeaways from the survey of the literature

Research contributes to the advancement of scientific knowledge both by small steps that expand and deepen our knowledge of the world and by transformative research, which by large steps sows the seed for new breakthroughs and shifts the frontiers of science. Both types of advances play a key role in the development of the scientific knowledge that lays the foundation for innovation and higher education, two processes which play a crucial role in reaping the fruits from society's investments in research.

There is no widely accepted definition of transformative research, which is moreover associated with a number of related terms e.g. revolutionary research, breakthrough science, creative or novel science, and "high-risk/high-gain" research. All these terms however point to two defining characteristics of what we here primarily refer to as transformative research: it involves a significant degree of *novelty* vis-à-vis etablished research paths and approaches within a given research field, and it involves a significant degree of *risk*.

Novel science comes with a higher degree of risk, because the exploration of new directions in research by its very nature is marked by uncertainty and challenges. Novel research efforts are associated with a greater likelihood of both producing new breakthroughs and of failure. In addition, novel science develops at the periphery of or across established research fields and therefore often does not adhere to existing practices or quality criteria. This leads to difficulties in assessing the quality and contribution of novel research efforts, even after the research has been completed and published, as novel research typically requires longer time to achieve recognition by the scientific community and to be applied in further research.

One of the central arguments for the public funding for science and for placing the responsibility for research in universities was to ensure adequate conditions for research, which is of a fundamental nature and associated with a high degree of uncertainty, and to shield it from expectations of tangible results within the short tem. There are however growing concerns that researchers today have greater incentives to engage in "safer", more predictable projects than to pursue riskier but potentially pathbreaking research. Ultimately, this may delay or impede the overall progress of science and the development of new breakthroughs. The survey of the literature identifies several factors that influence conditions for risk-taking in science:

- *Etablished practices and conservative tendencies in the scientific community* that can make it difficult for transformative research to achieve scientific recognition and impact, which may negatively affect the careers, professional recognition and funding options of unconventional researchers.
- The *assessment and reward systems* that are used to evaluate research, in hiring and promotion decisions, and in decisions regarding allocation of research funding, which mean that incremental contributions to established research paradigns represent a "safer" path than novel, risky research.
- *The increased importance and concentration of external research funding.* It can be more difficult to attract funding for transformative research, particularly when success rates are low. Increased concentration of funding may moreover lead to lower diversity and strengthened conservatism.
- *Conservative tendencies in the assessment of funding applications and in funding allocation decisions,* which are based to a large extent on peer review, which has a tendency to favor incremental contributions within established scientific paradigms. Peer review is moreover prone to several types of bias, including bias against early career researchers, against female researchers, and against cognitively diverse research teams.
- *The length and type of funding.* Stable, internal and flexible funding all appear to contribute to better conditions for risk-taking and breakthroughs, but much research is funded via external grants that can have a short grant period and limited flexiblity to allow for adjustments of aims and approaches.
- *Size and composition of research teams*. Diverse and smaller groups may have better preconditions to engage in transformative research. For instance, smaller teams make it easier for the leading

researcher to participate actively in the research, to set a direction for the team's work, to promote effective collaboration, and to enable less hierarchical decisionmaking processes within the team.

Research funders have a particular role to play in ensuring good conditions for transformative research because of the increasing importance of external funding for research, and because the ways in which we allocate, distibute and use research funding shape the conditions under which reseach is undertaken.

All investments in research involve some element of risk-taking because of the uncertain nature of scientific research. The question is therefore not whether there *is* risk-taking in science and science funding, but whether there is a *sufficient* level of risk-taking. There are no clear recipes for how research funders can promote greater risk-taking in science, should they wish to do so. Nonetheless, the survey of the literature does point to a number of possible approaches and issues to be aware of:

- Researchers call for *experimentation* with different types of approaches to support greater risk-taking to generate insight into the suitability, benefits and downfalls of these approaches. Research also underlines the importance of ensuring that researchers have *a high degree of autonomy within broadly defined reseach goals*; enabling *flexibility in the use of funding* rather than demanding that projects are confined by original, narrow aims; offering *longer grant periods* to promote experimentation and risk-taking; *offering progressive or continuous funding* to support long-term risk-taking; and *limiting demands for ongoing status and output reports.*
- Research also stresses the importance of efforts to *raise success rates on funding applications* and *decrease the degree of concentration of research funding* with a view to securing a broader disperal of resources, increased variation in research, and strengthened funding for the growth layer oF talent and ideas that emerge from the periphery or outside of well-funded, established research teams.
- The literature identifies two overall types of funding instruments to promote greater risk-taking: programs that *fund outstanding individual researchers* in their long-term pursuit of unconventional ideas, and programs that *fund unconventional ideas,* that would likely be rejected under peer review, but which are suitable for a time-limited research project (e.g. to enable initial exploration and development of the ideas to a point where they would be better able to compete for funding against more conventional ideas). The first type of program is characterized by larger annual grant sizes, typically provides funding over a longer period of time, and accounts for a large proportion of the total budget of the research funder. The second type typically awards smaller and shorter grants and accounts for a smaller proportion of the funder's total budget and portfolio of funding instruments.
- Other recommendations for supporting risk-taking in science funding include: *avoiding arbitrary a priori funding thresholds* and instead adapting grant sizes to the given pool of talent and its funding needs; *to take into account applicants' current funding levels and portfolio of ongoing projects* that they have committed themselves to; *to consider placing responsibiltiy for "high-risk/high-gain"-instruments in dedicated organizations* rather than in existing funding organizations; *to limit the use of bibliometric methods in assessing applicants and funded research; to shift focus in evaluations from individual researchers to research teams and groups;* and to consider other approaches to support novel work e.g. *scientific prizes* that can promote risky approaches and experimentation.
- Several studies make recommendations for how to *reduce bias in peer review and the assessment of funding applications* and *ensure a suitable composition of peer review panels,* including e.g. not requiring or preferring agreement among reviewers, instructing reviewers to apply broad assessment criteria, ensuring a diverse group of reviewers, looking for reviewers who are open to approaches outside their own field of expertise/established practices, and providing training to reduce bias.
- Finally, research underlines the benefits of universities *ensuring stable and/or internal funding* for researchers; that *researchers are employed in permanent positions*; and that *hiring, promotion and tenure decisions are not based on either bibliometric indicators or researchers' grant portfolio*.

The role of novelty and risk-taking in science

How does novelty matter for the advancement of science?

Understanding how scientists choose which research problems to pursue is important, because the sum of these choices "give scientific knowledge its shape and guide its future evolution" (Foster, Rzhetsky, and Evans 2015, p. 875).

There are two main ways by which science advances our existing knowledge. First, most science proceeds in small steps, through incremental accumulation of knowledge, with each contribution confirming, refuting or extending existing work by small steps. Such advancements may be described as *evolutionary science*, which contribute to the development of a scientific paradigm (Kuhn 1962), understood as a common knowledge base and consensus on which questions are interesting and legitimate to explore, which approaches are useful and appropriate in seeking to answer them (Boudreau et al. 2016). Second, science may advance by large steps, as *revolutionary science*, connecting existing knowledge in entirely new days or otherwise making substantial changes in the types of problems and/or approaches pursued, by introducing discoveries, ideas or approaches that significantly alter the path of existing research fields or lead to the establishment of entirely new research paradigms or fields. This distinction is described in several studies, some of which are exemplified in table 1, all of which employ different terms but convey similar points.

Table 1. Examples of conceptualizations of "evolutionary" and "revolutionary" science

References	Evolutionary science	Revolutionary science
Kuhn 1962 Kuhn 1977	"Normal science" "Tradition"	"Revolutionary science" "Innovation"
Polanyi 1969	"Conformity", "discipline"	"Rebellion"
Bourdieu 1975	"Succession"	"Subversion"
Whitley 2000	"Relevance"	"Originality"
National Science Board 2007	"Evolutionary science"	"Transformative science"
Foster, Rzhetsky, and Evans 2015	"Productive tradition" (consolidating existing knowledge clusters)	"Risky innovation" (bridging knowledge clusters)
Wang, Veugelers, and Stephan 2017; drawing on March 1991	"Exploitative research" (combining existing knowledge pieces in well-understood ways)	Explorative research" (combining existing knowledge pieces in an unprecedented fashion)
Aviña et al. 2018	"Convergent thinking" (testing and selection of ideas)	"Divergent thinking" (generation of ideas)

In practice, distinguishing between these forms of scientific progress is difficult, as they often overlap and proceed hand-in-hand (National Science Board 2007). However, the distinction is useful, because it draws attention to the importance of nurturing conditions for transformative breakthroughs (ibid.).

Key to all the alternative conceptualizations of evolutionary vs. revolutionary science is the notion of originality, creativity or *novelty*, referring to the unprecedented ways in which existing knowledge pieces are brought together to form new insights (Wang, Veugelers, and Stephan 2017), or to the introduction of new empirical observations or discoveries, new theoretical contributions, new methods or research instrumentation, or some combination of the above (Heinze 2013). For the purposes of this review, the term novelty will be preferred, but used more or less synonymously with other, related terms such as originality and creativity, in recognition of the wide set of scientific contributions that use different terms to describe closely related phenomena.

Heinze (2013) argued that novelty or, as he focused on, creativity in science is generally seen as a context specific phenomenon, meaning that the degree of creativity underlying scientific advances is observed and assessed from within scientific disciplines, with reference to the established practices and methods within that discipline.

In line with Boudreau et al. (2016), novelty is here understood as a spectrum: Within evolutionary science, novel advances have an incremental nature, advancing within existing scientific questions and approaches (Boudreau et al. 2016), whereas revolutionary advancements represent departures from the existing scientific paradigm, characterized by a higher degree of novelty (ibid.). Moreover, novelty is understood not as a precondition for breakthroughs, which may also result from a sequence of incremental, cumulative research advancements, but breakthroughs are often associated with novel research (Wang, Veugelers, and Stephan 2017).

Research efforts will typically involve both elements of novel and conventional approaches and insights. Uzzi et al. (2013) found 17.9 million papers spanning all scientific fields and concluded that the highest-impact science is usually based on highly conventional combinations of prior work, yet at the same time introduces unusual elements and combinations. Based on their findings, the authors argued (p. 468) that “the building blocks of new ideas are often embodied in existing knowledge” and that “balancing atypical knowledge with conventional knowledge may be critical to the link between innovativeness and impact.”

Why is risk-taking important for scientific breakthroughs?

As mentioned in the previous section, revolutionary science is associated with a high degree of novelty vis-à-vis established scientific research agendas and approaches.

Novelty in science is crucial in driving major advances in scientific inquiry and understanding and thus in the development of new breakthroughs. While revolutionary science is more likely to lead to major breakthroughs than evolutionary science, it also comes with a substantial *risk* of achieving no or low impact (Wang, Veugelers, and Stephan 2017). Foster, Rzhetsky, and Evans (2015, p. 877) argued that:

“When following a conservative strategy and adhering to a research tradition in their domain, scientists achieve publication with high probability: they remain visibly productive, but forgo opportunities for originality. When following a risk-taking strategy, scientists fail more frequently: they may appear unproductive for long periods … If a risky project succeeds, however, it may have a profound impact.”

On a related note, several funding bodies including e.g. the US National Science Foundation (NSF), the US National Institutes of Health (NIH) and the European Research Council (ERC) acknowledge that research at the frontiers of science is inherently risky.

Risk is intimately associated with uncertainty. As argued by Luukkonen (2012, p. 51), “Researchers … often refer to groundbreaking or unconventional research by terms that emphasize risk, with this risk implying that the outcome of a potential research project is highly uncertain.” Luukkonen (ibid.) further highlighted that risky research is associated with controversy, i.e. some degree of resistance from the established scientific community. On a related note, Sternberg (1998) emphasized that novel research is not always perceived as holding value by the scientific community.

Indeed, the history of science describes numerous examples of revolutionary science encountering difficulties in being accepted by the scientific community or even identified as contributions to a given field because they clashed with established paradigms, practices or approaches in the field (Kuhn 1962; Polanyi 1969).

In a seminal paper on the rationale for public funding of science, Nelson (1959) argued that the more "basic" (as opposed to "applied") research is, the less closely tied it becomes to specific applications, the less clearly defined its goals become, and the greater the degree of uncertainty or risk attached to the results of the research becomes. The loose definition of goals in basic research, he argued, is a rational way of dealing with the high level of uncertainty and of increasing the ultimate payoff on the investment in that research. In addition, sufficient resources are required to spread risk by funding multiple, concurrent basic research projects, recognizing that not all of these projects can be expected to come to fruition (ibid.). Similarly, Rosenberg (1990) emphasized that basic research involves an unusually high degree of uncertainty regarding the possible uses and thus eventual financial payoff from the research, and that this eventual payoff is moreover highly unlikely to show itself in the short term.

Nelson (1959) argued that the high level of risk associated with basic science is a key argument for the public funding of science, as it is likely to lead private actors to be cautious about investing in basic science, or to abstain entirely from it. A similar point was made by Arrow (1962), who also highlighted the inherently risky nature of research and argued that this nature is bound to generate some discrimination against investment in research and ultimately underinvestment by private actors, even though from a societal perspective such investments have the potential to generate great value for a broad set of actors. He therefore argued for a strong case for public funding in risky, basic research.

Describing science as "basic" or "fundamental" implies that it is not concerned with hands-on problem-solving and immediate practical ends. Its fundamental nature does not however preclude its practical relevance: over the history of science, many fundamental discoveries and advances have shown themselves to hold great use for practical applications (Rosenberg 1990; Rosenberg and Nelson 1994; Stokes 1997), although usually as an intermediate good (Rosenberg 1990), that is, an input in other R&D projects, as one of many building blocks used in the development of technologies and solutions (Nelson 1959).

Interdisciplinary research is another form of research, which is often perceived as risky (see e.g. Laudel 2006a) and associated with low funding success (Bromham, Dinnage, and Hua 2016) and difficulties in getting published (Martin 2013), among other reasons because its cross-disciplinary nature reduces the usefulness of established scientific practices and knowledge in gauging the quality of science. In addition, as shown by Leahey, Beckman, and Stanko (2017), scientists who engage in interdisciplinary research may receive many citations but tend to be less productive than other comparable researchers; the authors attribute this effect to the time needed to work through cognitive and collaborative challenges associated with interdisciplinary research and also hurdles in the review process.

It is worth noting that there is no established or widespread taxonomy of risk in science. Indeed, risk may refer to many different types of risks, including but certainly not limited to *operational risk*, which is associated with potentially problematic factors in the execution of research projects and can usually be mitigated, pending adequate and relevant resources, and *conceptual risk*, which stems from scientific challenges in exploring novel research paths, which by their very nature are susceptible to failure.

Moreover, risk may be relevant to consider at different stages of the scientific process. As mentioned above, risk is an element in the *execution* of research projects or in the *dissemination* thereof, for example considering risks related to the reception of findings by the scientific community or by stakeholders outside of academia. But risk may also be considered in the initial *development and selection* of research projects. For instance, researchers may choose not to venture down uncertain research pathways for fear of the possible implications of doing so for their scientific performance, reputation and career. Ideas for novel or risky research that do not yield publishable findings are impossible to observe (Foster, Rzhetsky, and Evans 2015). Also, it is impossible to determine how many ideas are deselected upfront or abandoned after some initial failures in scientists' pursuit of them. As previously mentioned, risky research ventures are more likely to fail and therefore not lead to publishable findings; this is known as the "file drawer problem", referring to the unknown number of studies that have been conducted but not published (Rosenthal 1979). Indeed, Dietz and Rogers (2012) pointed to the importance of ensuring that "enough" creative ideas are put forth, as this alone might increase the number of creative ideas that ultimately obtain support. Addressing this issue, they argued, requires dealing with the factors that give researchers strong incentives to abandon unconventional ideas in favor of "safer" projects that are deemed more likely to attract funding.

Growing concerns regarding conditions for novelty and risk-taking

In recent years, growing concerns have been raised regarding the state of, and conditions for, risk-taking in science, for instance within biomedicine (Alberts et al. 2014; Foster, Rzhetsky, and Evans 2015; Rzhetsky et al. 2015). Essentially, concerns revolve around the question of the research system has become too conservative, opting for 'safe' over 'bold' research ventures, and primarily encouraging incremental advances in our scientific knowledge. Ultimately, the concern is that a lack of risk-taking will impede the progress of science and decrease the likelihood that new scientific and technological breakthroughs are made (Wang, Veugelers, and Stephan 2017).

Stephan (2013) argued in an essay that the US university research system faces a number of challenges that may have a negative influence on discovery and innovation based on academic research. One of these factors is a tendency on the part of faculty and funding agencies to be risk averse. The other factors identified in the paper are the tendency that more PhDs are educated than the marker for research positions requires, a concentration of research in the biomedical sciences, flat or decreasing federal research funding, and the continued expansion of universities, placing universities at increased financial risk. The latter factor was also described as a challenge to the health of the US biomedical research system by Alberts et al. (2014).

Expanding on her point regarding risk aversion among faculty and research funders, Stephan (2013) argued that incremental research advancements are important but not sufficient to realize substantial gains from research. She also argued that a key motivation for placing research in the university sector was to ensure appropriate conditions for "basic research of an unpredictable nature" (p. 32), and a belief that universities were well suited for this task by providing an environment which is conducive to the advancement of scientific knowledge, among other things because they are not under pressure to deliver tangible results in the short-term. Stephan (ibid., p. 32), however, laments the development whereby university researchers have come under pressure for "quick, predictable, results".

Other researchers have pointed out that excess conservatism in choices of research topics and methods may lead to some areas of research becoming "over studied", to the neglect of alternative, less explored research agendas. For instance, Yao et al. (2015) concluded that the allocation of resources to biomedical research in the US is influenced far more by previous allocations and research than by current health needs, leading to some medical conditions becoming over studied.

More generally speaking, in an opinion piece in PNAS, Geman and Geman (2016, p. 9386) lamented the growing tendency to assess and reward scientists based on their number of publications, the growing dependency on external funding in science, and the lack of time and incentives to take risks: "The response of the scientific community to the changing performance metrics has been entirely rational: … Being busy needs to be visible, and deep thinking is not. Academia has largely become a small-idea factory. We are awash in small discoveries, most of which are essentially detections of "statistically significant" patterns in big data." They continued: "Not surprisingly, many papers turn out to be early "progress reports," quickly superseded. … the incentives for exploring truly novel ideas have practically disappeared. All this favors incremental advances, and young scientists contend that being original is just too risky" (ibid). On a related note, (Martin 2016, p. 17) argued that

"… while some of the early efforts to improve the efficiency of university research may have resulted in significant gains, attempts to achieve yet further gains have come at a disproportionate cost. Assessment schemes and performance indicators have over time tended to skew research towards safe, incremental, mono-disciplinary mainstream work guaranteed to produce results publishable in top academic journals, and away from interdisciplinary and more heterodox, risky and long-term research. They have also generated perverse incentives, encouraged cynical game-playing to beat the system, and resulted in various unintended consequences."

What is the role of risk-taking in science funding?

The role of risk-taking in science funding is less clear from the literature. Because of the inherently uncertain nature of scientific research (Nelson 1959; Arrow 1962), all science funding can be seen as involving some degree of risk-taking.

As pointed out by Heinze (2008), the way in which research funding is allocated, distributed and spent has a strong influence on the conditions under which research is carried out. He also emphasized that a key challenge for policymakers, research funders, research managers is finding out how to support novel and unconventional research that can expand the frontiers of science, particularly in view of the heightened uncertainties associated with such research and the barriers it is likely to encounter. Along a similar vein, Lyall et al. (2013) emphasized the role of research funders' support in enabling novel interdisciplinary research, particularly in the development of large-scale interdisciplinary initiatives.

Returning to Heinze (2008), he examined several funding schemes aimed at encouraging scientists to conduct unconventional and high-risk research and showed that such programs generally strive to remedy specific deficiencies in national research systems with respect to improving conditions for novel, high-risk research. He identified two main categories of programs: those that fund outstanding individual *scientists* in long-term pursuit of unconventional ideas, and those that fund unconventional *ideas* that would be likely to be rejected under peer review but which can be pursued within the format of a research project (for instance with a view to developing the ideas to a point where they are better able to obtain other, more conventional sources of funding). Some of the benefits and key issues to consider in connection with these types of programs are described in the last section of this report, *How can risk-taking be stimulated?*

One of the main conclusions from Heinze's work was that many of the programs, despite their explicit aim to promote risk-taking in creativity, still showed strong tendencies toward exploitation and the support of "safe" projects. As stated by Heinze (2008, p. 316):

"Given the fact that the nine programs under review aim at funding 'high-risk' research questions, it is compelling that the decision process itself tends to be rather risk averse. Interviewees, including those from private foundations, typically argued that they want to make an investment that bears fruit; that their budget is relatively small; and that their decisions must be fully accountable. Thus, even in programs for high-risk research, the 'forces of exploitation' … remain strong."

On a related note, Prendergast, Brown, and Britton (2008) surveyed the availability of European funding programs that aim to stimulate high-risk research and found, among other things, a lack of favor afforded to "speculative" projects and an emphasis on applicants' "track record"; they concluded that funding agencies did not always live up to stated aims of supporting risk-taking. Also, Wagner and Alexander (2013) described an evaluation of the US National Science Foundation's (NSF) Small Grants for Exploratory Research (SGER) program, which ran during the period 1990 to 2006 to encourage investments in high-risk, high-reward research across the NSF. They concluded that SGER was highly successful in supporting transformative research projects, but that the program was underutilized by NSF program directors for most of the years that it was in operation, which they argued indicated that program managers "remained risk averse and continued to support projects that were likely to produce positive outcomes" (p. 187). This would support the argument put forth by Heinze (2008), that exploitative forces remain strong when high-risk programs constitute a small part of a larger portfolio of research funding instruments.

Dietz and Rogers (2012) emphasized the role of funding organizations and their program officers in promoting greater risk-taking in research, as these program officers, like reviewers of grant applications, are forced "well beyond [their] normal comfort zone", where usual evaluation criteria have limited use in the assessment of the merit of an application. They further argued (p. 41), that "Funding agencies must be willing to play a moderating role and to stay the course when pressures may mount to abandon the cause."

These years, many funding organizations are considering whether and, if so, how to adjust their activities in response to the growing international focus on conditions for transformative research; Feller (2016) even described it as the "widespread international bandwagon interest in transformative/breakthrough/high-risk research" and underlined that the concept of transformative research "remains mired in mystique" and characterized by vague definitions. He suggested transformative research to be the last in a long line of priorities in science policy, which according to him includes at least the following: "pure, strategic, strategic basic, fundamental, mission (non-mission) oriented, Bohr/Pasteur Quadrants, Translational, Basic Technological Research, Need Driven/Curiosity Driven, Mode 1/Mode 2" (p. 258).

"Filtered through this historical perspective," Feller (2016, p. 258) argued, "transformative research appears yet but another policy epicycle." He furthermore argued (ibid., p. 263) that funding organizations "… have yet to sort through how to integrate, or splice, transformative research initiatives into, atop, or alongside existing programmatic structures."

Indeed, Foster, Rzhetsky, and Evans (2015) considered that policy initiatives to promote more risk-taking would lead to a higher prevalence of risky research strategies, which might eventually push researchers into "outlandish projects". They reflected, however, that it is implausible that the current research system is near this point, implying that there is good cause to promote better conditions for novel research.

Factors that influence risk-taking in science

This section reviews key insights from the literature regarding the factors that may influence the degree and nature of risk-taking in science. These factors have been grouped into three categories: *established practices in the scientific community*, *reward systems in science*, a wide range of *factors related to research funding*, and *research groups' size and composition.*

Established practices in the scientific community

Established paradigms and practices among scientists working within the same or related fields may encourage evolutionary science or directly discourage highly novel, potentially revolutionary science. Kuhn (1962) argued that convergent thinking involves researchers focusing on the same problems and approaches, leading to a well-defined community of practice, which eventually exhausts itself, leading to revolution. Science is, in this perspective, marked by an endless cycle of convergent research (or "normal science") and divergent research ("revolutionary science"). In this perspective, convergent thinking is a natural element in the cycle of science, and tradition and innovation exist in a productive tension for all scientists (Kuhn 1977).

On a related note, Bourdieu (1975) described how researchers face a choice between "succession" and "subversion" in science, and argued that deviance from established scientific practices is censored and even punished because researchers are disposed to cite and recreate established practices and through competition for recognition among academic peers. However, he also held that scientists who pursue new research directions that deviate from the established practices may achieve scientific capital and recognition that is proportionate to the success with which they pursued new directions.

More recently, Foster, Rzhetsky, and Evans (2015) reiterated Kuhn's (1962) argument that scientists must continually manage a tension between tradition and innovation, and argued that scientists manage this tension through their choice of research strategies: "Tradition is not pursued purely because of training; it is a reliable strategy to accumulate recognition. Innovation is not a happy accident; it is a risky gamble." (Foster, Rzhetsky, and Evans 2015, p. 879) The authors moreover argue that tradition and innovation can coexist (in tension) within scientific fields, within individual scientists, and even within papers.

In examining what influences the choice of research problem[1], Foster, Rzhetsky, and Evans (2015) argued that scientists' choices regarding which research to pursue are informed by their expectations of the outcome of alternative possible research paths, including an assessment of the level of risk of

[1] The choice of strategy is not necessarily a deliberate process. Foster, Rzhetsky, and Evans (2015, p. 902) explain: "While we use the language of research "choice"…, we do not imply that the selection of one problem over another is necessarily the outcome of a deliberate or deliberative process. We mean that out of many possibilities, one is pursued and possibly published."

failure, their perception of the willingness of relevant academic journals to publish findings of such research, and their expectations of the reception and citation of the findings by the scientific community. As cited in Foster, Rzhetsky, and Evans (2015), a study by Bateman and Hess (2015) explored publication portfolios for diabetes researchers to better understand why some scientists pursue deep contributions within their knowledge domain, while other pursue broad contributions spanning multiple knowledge domains. Survey findings revealed that diabetes researchers were less likely to pursue a hypothetical broad contribution spanning multiple domains than a focused, deep contribution, viewing the latter as “potentially very important” and less risky.

Drawing on the academic literature, Foster, Rzhetsky, and Evans (2015) also pointed to several other factors that may influence a scientist’s choice of research problem, including their past interests and training, serendipitous encounters with new collaborators or information, practices and norms within their scientific discipline or local research environment, whether their research field is emerging or mature, the level of consensus within their research fields, and the stage of the academic career and level of accumulated scientific capital of individual researchers, which influences both their opportunities to pursue novel research paths and their ability to reap the rewards of fruitful attempts to do so (ibid.). For instance, the authors pointed out that risk-taking strategies are likely to be easier to pursue and sustain when scientists have accumulated significant scientific capital to allow them to spend time pursuing risky ventures and bolster themselves against risks associated with failed experiments or delayed or lacking recognition of highly novel findings.

On a related note, Sternberg (1998, p. 210) stated that “Scientists enforce conformity in a number of ways, both formal and informal”, including how researchers are trained, difficulties associated with getting publications that describe findings that diverge from established knowledge and methods published, conservative grant assessment procedures (a point which we will return to later in the report), recognitions such as prizes and awards that typically reward scientists working within established and accepted scientific paradigms, and informal networks in science, whereby scientists working on the fringes or outside of established paradigms are less likely to be invited to serve as reviewers, on committees, give invited talks etc. All these factors may contribute to conservatism to the detriment of highly novel, risky research efforts. Indeed, a study of Nobel class discoveries described the resistance that future Nobel Laureates met from the scientific community and from scientific journal editors and referees on articles describing the discoveries that would later earn them a Nobel prize (Campanario 2009).

Chai and Menon (2019) recognized prior studies on the bias against novelty and introduced an additional mechanism to explain on why novel research may encounter particular difficulties in gaining recognition within the established scientific community, namely that newly published work must compete with other publications for attention among the researchers who might build upon them. In other words, drawing on previous research, they argue that the impact of novel research depends not only on the intrinsic qualities of the research itself but also on the extent to which it is recognized by and build legitimacy among other researchers, and that this recognition is influence by other factors than merely the quality of the research (which in itself, as previously pointed out, can be difficult to assess when research is highly novel and therefore may not fit neatly into established research standards and quality criteria) and the extent to which it draws on familiar scientific knowledge domains. The authors argued that other factors which may influence the recognition of novel research is the sheer overload of information and scientific publications combined with limited time and cognitive resources among scientists, but also the extent to which scientists are aware of developments in fields outside their own area of research.

To explore these issues, Chai and Menon (2019) looked into more than 5.3 million research publications in the life sciences from 1970 to 1999, indexed in the Web of Science and MedLine, and found that publications on rarely addressed topics as compared to publications on more popular topics both tended to receive more citations and to have a higher chance of being among the top 1% of forward citations for papers published in a given field, in a given year. Put differently, papers on novel topics have a significantly higher chance of being recognized and built upon by their scientific peers. The study also found similar effect sizes for “home” and “foreign” citations, i.e. for citations from within or from outside the publishing authors’ own field of research. The study moreover showed that competition for recognition is stronger in the first year after the publication of an articles and its effect weakens over time. Finally, the authors concluded that both mechanisms of bias against novelty and competition for attention can work simultaneously, and that the latter has been largely overlooked in the previous literature.

Reward systems in science

Another factor identified in the literature as having a detrimental impact on novelty and risk-taking in research is the set of formal systems by which scientists are assessed and rewarded, including systems linked to researchers’ scientific productivity and their ability to attract competitive research funding.

Scientific productivity

Scientific productivity, e.g. as indicated by the number of scientific articles that a researcher publishes, is a key factor in scientists’ hiring, promotion, and funding. As pointed out by Foster, Rzhetsky, and Evans (2015), productivity is easier to attain and sustain through incremental contributions along established research paths than through high-risk research. The authors further argued (p. 899) that “a disposition toward tradition is plausibly adapted to maximizing productivity and reliably accumulating scientific capital, while a disposition toward occasional innovation is motivated by a gamble for posterity and a desire to achieve higher position in the field.”

In a related paper, Rzhetsky et al. (2015) show through a study of three decades’ worth of biomedical publications that incremental advances, while probably supporting researchers’ scientific career advancement, slow scientific advance. Their findings were particularly pronounced in mature research fields, where the authors argue that greater risk-taking, greater interdisciplinarity and less redundant experimentation would accelerate discovery.[2] The authors concluded by calling for changes in scientists’ reward systems that encourage greater diversity and risk-taking in science as well as the publication of failed experiments to increase the speed of scientific discovery.

Essentially, the concern is that scientists cannot afford to (or at least dare not) waste resources in a high-risk project that may not bear fruit, or which may at least involve delayed outputs and recognition compared to more incremental work within established paradigms.

[2] Rzhetsky et al. (2015) acknowledged that their work was based on the premise that the scientific community as a whole has an implicit objective to explore the space of possible research problems, looking for novel and useful knowledge, although individual researchers may pursue other objectives, including e.g. maximizing their number of publications or expected citations to their work. The authors also acknowledged that their study only considered one aspect of efficiency, namely maximizing the discovery of novel links, recognizing that other functions exist, e.g. minimizing error or increasing the robustness of discovered knowledge.

Wang, Veugelers, and Stephan (2017) pointed out that funding bodies but also hiring institutions are placing increasing importance in their assessment of candidates on readily available bibliometric indicators. They argued that this practice may explain in part the perception of growing risk aversion and emphasis on “safe” projects over novel, high-risk projects, as bibliometric indicators tend to be biased against novelty. They confirmed this claim in a paper (ibid.) introducing a bibliometric measure of novelty based on new combinations of journal references in published scientific papers published in 2001 and indexed in the Web of Science, taking into account the difficulty of making such new combinations through the distance between the journals. Their findings confirmed the “high risk, high gain” nature of novel research, which received both more citations on average and were more likely to become among the top cited (i.e. top 1%) papers, but also experience higher variance in the number of citations received. Furthermore, the authors found that even though highly novel papers were more likely to become top cited, this was observable only when using a longer time window of at least four years, which is longer than the windows used in many other bibliometric studies. In other words, highly novel research is more likely to be highly cited but takes longer to accumulate recognition and therefore citations. As explanations for this finding, the author mentioned that novel research may display “scientific prematurity” affecting its recognition (citing Stent 1972), experience “delayed recognition”, possibly due to the period of time required for findings to be recognized and incorporated into follow-on research (citing e.g. Garfield 1980), or have the qualities of so-called “sleeping beauties” i.e. a publication that goes unnoticed for a long period of time before suddenly attracting a lot of attention (citing van Raan 2004), possible explained at least in part by resistance from established scientific paradigms.

The study by Wang, Veugelers, and Stephan (2017) also showed that highly novel research was more likely to stimulate follow-on breakthroughs, and that the recognition of novel research typically comes not from within its own “home” field of research but from researchers outside its own field, or “foreign” fields. Finally, the authors’ findings confirmed a bias against novelty in standard bibliometric indicators, as novel research was less likely to be highly cited in typical short-term citation windows, and overall also more likely to be published in journals with a lower Journal Impact Factor (JIF), a commonly used but heavily criticized and flawed bibliometric measure. These findings lend support to the argument that overreliance on standard bibliometric measures is likely to discourage novel and potentially groundbreaking research and to overlook the follow-on breakthroughs that build on novel research. Nonetheless, the authors acknowledge that their method focuses on combinatorial novelty, which captures only one of several dimensions along which novelty may be characterized.

On a similar note, Yegros-Yegros, Rafols, and D’Este (2015) examined the effect of degree of interdisciplinarity on the citation impact of individual publications for four different scientific fields. They concluded that while drawing on multiple disciplinary fields has a positive effect on the generation of new knowledge, successful research (as indicated by its citation impact) is better achieved through research that draws on a relatively close range of fields, because interdisciplinary research drawing on more distant fields is more risky and likely to fail. The authors point out, however, that their findings may also indicate that scientists are reluctant to cite papers that draw on highly dissimilar scientific fields and thereby give less credit to research that is highly novel and potentially groundbreaking. Either way, their findings emphasize that high levels of novelty as indicated by a high degree of interdisciplinarity may be penalized in later citations to the work.

This is unsurprising in view of findings from several studies that describe peer review as having conservative and risk-minimizing tendencies, leading peer reviewed processes to favor disciplinary and conventional research (e.g. Langfeldt 2006; Luukkonen 2012; Martin 2013; Zoller, Zimmerling, and Boutellier 2014).

Pressures to secure external funding

Reward systems focus not only on scientific productivity but also on other indicators of scientific performance, including the ability to attract external research funding. Stephan (2013) drew attention to the pressure on researchers to obtain funding, not only in their early career but also on a continued basis. She argued (p. 31) that this pressure means that faculty members – particularly those employed in "soft money" (i.e. externally funded) positions – must produce "doable" research projects and "can ill afford to follow a research agenda of an overly risky nature. They need tangible results and they need them quickly."

Laudel (2006b) set out to study whether the general assumption that competitive funds are awarded to the best researchers or proposals could be confirmed. Based on a comparative study of the conditions of fund acquisition among German and Australian experimental physicists, she concluded that though proposal quality and researcher reputation were important prerequisites for success in grant applications, the success of a funding proposal was contingent upon several factors that were neither linked to quality nor all under the control of scientists, including for instance the available funds of the university (which affect what is offered as basic supplies and resources to scientists) but also the availability of suitable collaborators. She also drew attention to the likely role of the "Matthew Effect" (see Merton 1968; and, more recently, Bol, de Vaan, and van de Rijt 2018) in explaining disproportionately large amounts of funding flowing to already well-funded researchers and creating obstacles for initial or continued funding for other applications. Laudel (ibid.) also noted that the scientists interviewed in her study generally avoided risky research because of concerns that this would affect their chances of getting funded negatively. Finally, the author drew attention to strategies pursued by scientists in acknowledgement of the strong dependence on external funds, including e.g. using "a money-laundering strategy to supplement funds of external grants by rearranging money from other externally funded projects" (p. 392), for instance to fund prior work in preparation for grants to de-risk them and increase the assessment of their feasibility.

On a related note, Wang, Lee, and Walsh (2018) compared the novelty of papers published by Japanese authors and found that papers funded by competitive project funding were, on average, more novel. However, this finding did not hold for junior or female researchers: here, novelty was higher when the researchers were funded by non-competitive block funding. These findings suggest that competitive funding may be less receptive to innovative ideas from these types of researchers, or that funding conditions associated with a grant may eventually induce them to pursue more conventional research. Alternatively, these types of researchers may expect greater scrutiny, and therefore choose not to submit more novel ideas, ultimately leading to funding for more conventional than unconventional research.

Hypercompetition in science

Some researchers have drawn attention to the combined effect of the indicators on which scientists' performance and due rewards are assessed, arguing that they have led to a situation of "hypercompetition". Alberts et al. (2014) published an essay arguing that a combination of ongoing expansion in biomedical research in the US and decreasing funding for the National Institutes of Health (NIH) have led to a situation of "hypercompetition for the resources and positions that are required to conduct science" (p. 5774). The authors moreover argued that this hypercompetition "suppresses the creativity, cooperation, risk-taking, and original thinking required to make fundamental discoveries." (p. 5774) They also lamented a decrease in coherent time for research, (p. 5774):

"The development of original ideas that lead to important scientific discoveries takes time for thinking, reading, and talking with peers. Today, time for reflection is a disappearing luxury for the scientific community. In addition to writing and revising grant applications and papers, scientists now contend with expanding regulatory requirements and government reporting on issues such as animal welfare, radiation safety, and human subjects protection. Although these are important aspects of running a safe and ethically grounded laboratory, these administrative tasks are taking up an ever-increasing fraction of the day and present serious obstacles to concentration on the scientific mission itself."

Factors related to research funding

In many national research systems across the world, there has been a shift in funding from stable funding to competitive funding of projects (see e.g. Franssen et al. 2018), based at least in part on the assumption that the best researchers or proposals win grants (Laudel 2006b). These issues are particularly important to consider in light of findings by Tatsioni, Vavva, and Ioannidis (2010), who examined the funding sources reported in landmark scientific papers of Nobel Prize winners and concluded that a substantial portion of this work appeared to be unfunded, although particularly governmental sources of funding contributed to a large portion of the work. Subsequent interviews with Nobel laureates whose landmark papers reported no funding confirmed that much of the Nobel-level work arose from entirely unfunded research, particularly when institutions offered a protected environment for their scientists.

Concentration in research funding

Another aspect of the research funding system that has attracted attention in the academic literature as a possible contributing factor to conservatism and risk aversion in science in the growing concentration of research funding (e.g. Viner, Powell, and Green 2004; Shibayama 2011; Bol, de Vaan, and van de Rijt 2018; Rigby and Julian 2014).

Lawrence (2009) argued that the current competitive funding system favors "an upper class of skilled scientists who know how to raise money for a big group" (p. 2). These successful principal investigators are often adept at attracting and sustaining high levels of funding for their research groups, which according to Lawrence (2009, p. 3) "can appear effective even when they are neither efficient nor innovative." In addition, Lawrence (2009) pointed out that large, well-funded research groups can lead to a large number of early career researchers working to extend the work and boost the careers of their principal investigators rather than pursuing independent projects, and that these groups may often outcompete smaller, less established groups.

Concerns regarding the concentration of research funding are described in a recent review article by Aagaard, Kladakis, and Nielsen (2019), which addresses the question of whether giving large amounts of funding to a limited number of elite scientists yields the greatest return on investments in science, or whether scientific progress is better supported by giving smaller portions of funding to a larger number of individual researchers and research teams. Based on a review of the literature, they conclude that the literature "demonstrates a strong inclination towards arguments in favor of increased dispersal" (p. 1). The authors also concluded that increasing grant sizes appear to show stagnant or diminishing returns to scale in terms of the research performance of the funded groups.

A related study of the concentration of competitive research funding over a 12-year period in Denmark (DEA 2019) confirmed that research funding has indeed become more heavily concentrated in the hands of a relatively small but highly successful group of elite researchers. More precisely, the study finds that 20 percent of Danish researchers are grant holders or principal investigators responsible for approximately 90 percent of the total grant sum allocated by the public and private research funding organizations included in the study during the 12-year period covered. This finding raises questions regarding how suitable the current Danish research funding system is for supporting not only already successful research groups but also the growth layer of new talents and ideas that emerge at the periphery of established research fields. Similar findings are also described by Bol, de Vaan, and van de Rijt (2018) who found evidence of a “Matthew Effect” in science funding.

Bias in funding applications assessment and grant decisions

Another subset of the literature is concerned with the processes by which research funding applications are assessed and granted, focusing in particular on preferences and biases among grant evaluators, which may skew assessments and decisions in favor of evolutionary over revolutionary science. As pointed out by Nicholson and Ioannidis (2012, p.34), “… concern is growing in the scientific community that funding systems based on peer review … encourage conformity if not mediocrity, and that such systems may ignore truly innovative thinkers.”

For instance, Sternberg (1998) pointed to the existence of challenges in obtaining grants for research that diverges from established knowledge and paradigms, arguing that just one negative reviewer can often block a proposal, that many programmatic funding bodies work within their established program of research but can have difficulties or entirely reject in working outside of it, and that reviewers are often scientists who work within the established scientific paradigms.

On a similar note, Stephan (2013) lamented the tendency among funding bodies and the evaluators they draw upon to assess grant applications based on their “doability”, arguing that grants are often selected because the feasibility of the proposed research is deemed to be high (citing Alberts 2009) and perhaps even partially undertaken (citing Azoulay, Zivin, and Manso 2012).

Guthrie, Ghiga, and Wooding (2018) examined the efficiency of grant peer review processes in the health science and found “strong evidence” of bias against innovative research and “fairly clear evidence” that peer review assessments are a poor indicator of future research performance. They also concluded that ratings varied considerable among peer reviewers.

With reference to a number of prior studies, Boudreau et al. (2016) pointed out that reliability in the assessment of funding applications across evaluators has been found to be very low. What explains this variation? Some of the factors at play may include researcher and evaluator characteristics, ties between researchers and their evaluators, formats of funding proposals, and application evaluation procedures. Boudreau et al. (ibid.) set out to investigate the role of “intellectual distance” between the knowledge embodied in a research proposal and evaluators’ own expertise. In order to do this, the authors designed and executed a grant proposal process at a leading research university, where they randomized the assignment of evaluators and proposals to more than 2,000 evaluator-applicant pairs. They found that evaluators systematically gave lower scores to research proposals that were closer to their own areas of expertise and to proposals that were highly novel.

In view of the types of concerns described in this section, Nicholson and Ioannidis (2012) investigated whether biomedical researchers who do the most influential scientific work get funded by the NIH, a key source of funding for biomedical research in the US. They found that three out of five authors of influential papers (that had received 1,000 citations or more since 2001) did not currently have NIH funding as principal investigators, which a large majority of the current members of NIH study sections (the people who recommend which grants to fund) did have NIH funding irrespective of their citation impact, which was typically modest. The authors stressed that there could be many reasons why the highly cited scientists in their study did not have current NIH funding, incl. e.g. having moved to industry, that they were being funded as co-investigators (not principal investigators), or that they were at the beginning of their career. Yet they maintained that their findings suggested that the NIH's mandate to fund "the best science, by the best scientists" was not being met. Nicholson and Ioannidis (2012) also cautioned against allowing grant holders to serve as grant reviewers, arguing that this practice introduces an inevitable conflict of interest, which may result in potentially promising novel ideas being deselected. They further argued that creative scientists may opt out of reviewing tasks due to a perceived lack of support for novel ideas in the grant application assessment project. A study by Stavropoulou, Somai, and Ioannidis (2019) found similar results for UK scientists, suggesting the trends identified above may apply to other countries as well.

Based on a study of peer review at the National Institutes of Health (NIH) in the US, Li (2017) showed that evaluators are both better informed and more biased about the quality of projects that fall within their own area of expertise, indicating that the benefits of expertise must be weighed against the costs of bias. The author draws attention to the negative downsides of bias but also emphasizes the importance of qualified assessments of the quality of proposals. Similar thoughts were presented by Li and Agha (2015), who based on an analysis of more than 130,000 grants funded by the NIH during the period 1980-2008 stressed the benefits of peer evaluations, especially for identifying applications with high-impact potential.

On a related note, in a paper on the European Research Council's (ERC) peer review system and its ability to pursue its mission to promote excellent, groundbreaking research, Luukkonen (2012) argued that the selection of highly novel research is constrained by the established scientific knowledge and practices against which the potential of new proposed projects are assessed, affecting the extent to which peer reviewers feel comfortable taking risks in their assessments. She also underlined that controversy and uncertainty were inherent elements in the assessment of potentially groundbreaking proposals, and that predicting the outcomes of peer review processes in such a context is difficult due to the existence of a large number of factors that ultimately affect these outcomes.

Langfeldt (2001; 2006) has shed light on key aspects of the peer review process. Her 2001 paper emphasized the random nature of peer review: the outcomes of which are highly dependent on who the reviewers are and on the way in which peer review processes are organized. More specifically, she found that novel and controversial projects fare better when funding budgets are ample and rating scales rough (as this is likely to produce identical scores for a higher number of proposals, enabling reviewers to consider other criteria and merits, including for instance the originality of the research) than when budgets are restricted and scales fine-grained. This suggests that the process by which peer review is organized may lead to assessments which are more or less supportive of novel, risky research.

Langfeldt (2006) addressed the conservative and risk-minimising aspects of peer review, arguing that these aspects serve to the disadvantage of interdisciplinary and unconventional research. She also pointed to the risks of engaging "scholarly enemies" and "persons 'intolerant' to the kind of research under review" (p. 38) as reviewers, and argued that a focus on meeting established standards or ensuring a high degree of thoroughness in peer review is likely to favor "safe" over unconventional research. This, she argued, happens because such peer review processes are likely to employ a larger number of peers as reviewers, thereby increasing the likelihood that one or more of them is close-minded or even skeptical towards unconventional research. This situation is particularly problematic when consensus among reviewers is necessary or preferred.

More generally, Mueller, Melwani, and Goncalo (2012) argued that people are often likely to reject creative ideas, even when encouraging creativity is an explicit aim. They explained this by the argument that bias against creative ideas may not always be apparent and observable but activated when people seek to reduce uncertainty in decision making.

Bias affecting particular groups of researchers

Other studies have explored whether bias in peer review processes disfavors certain groups of scientists, notable early career researchers or female researchers. For instance, Guthrie, Ghiga, and Wooding (2018) examined the efficiency of grant peer review processes in the health science and found some evidence of bias against younger researchers; similar concerns were addressed by Melin and Danell (2006).[3]

Another study suggested funding bodies may be biased against diverse teams. Banal-Estañol, Macho-Stadler, and Pérez-Castrillo (2019) investigated this issue, arguing that diversity in teams has been positively linked to the development of transformative research. Yet studies have suggested that funding bodies may be biased against diversity in research (citing e.g. Langfeldt 2006; Laudel 2006a), for instance because the research they propose is perceived as more risky or less "doable" (citing Luukkonen 2012). Focusing on grant decisions by the UK Engineering and Physical Sciences Research Council (EPSRC), they found that teams were more likely to undertake transformative research when characterized by greater diversity in knowledge and skills, education, and/or scientific ability. These teams were however also found to be less likely to obtain funding. It is interesting to note that this bias was weakened or disappeared entirely when diverse teams were led by prestigious researchers; interestingly, however, the presence of these prestigious principal investigators was not found to either mitigate or amplify the positive impact of the team's diversity on its ex-post performance.

In a recent NBER working paper, Kolev, Fuentes-Medel, and Murray (2019) pointed out that blinded review – a process whereby all identifying information on applicants is removed from applications for research funding – is increasingly used to reduce bias and increase diversity in the selection of people and projects in connection with the allocation of research funding. In an effort to explore how effective blinded review really is, the authors explored the impact of blinded review on gender inclusion in innovative research grant proposals submitted to the Gates Foundation during the period 2008-2017. They found that even despite blinded review, female applicants received significantly lower scores than male

[3] Research indicates that young researchers can play an important role in novel research: a study of Nobel Prize winners from physics, chemistry, medicine and economics showed that most prize winnners were under the age of 40, when they did their groundbreaking work (van Dalen 1999). Another study however suggests that more recent Nobel prize winners are older, when they do their prize winning work (Jones and Weinberg 2011).

applications, and that these differences could not be explained by other reviewer characteristics, the topics covered by the proposals, or ex-ante measures of applicant quality. The authors controlled for text-based measures of proposals' titles and descriptions, which lead the gender score gap to no longer be significant. The authors uncovered substantial gender differences in the usage of "narrow" (i.e. topic-specific) words and "broad" words (i.e. words used across a wide range of topic areas). Their findings indicated that differences in male and female researchers' use of words and communication style affected the blinded reviews and were a key driver of the gender score gap. More precisely, female scientists were 16 percent less likely than their male counterparts to get a high score on their grant proposal. Moreover, the study found that text-based measures that could predict higher reviewer scores did not predict higher ex-post innovative performance. In fact, female researchers included in the study demonstrated a greater response in follow-on scientific output subsequent to their proposal being accepted, as compared to male applicants. The findings of the study draw attention to limitations to the effectiveness of blinded review processes and likely help to explain gender disparities in the evaluation of research proposals. Finally, the study showed that repeat applicants typically received higher reviewer scores on subsequent proposals, but also that female researchers were less likely to resubmit a proposal after an initial rejection. The authors of the study recommended continuing the use of blinded reviews, which they argued has shown promise in equalizing opportunities for candidates across other categories like age and race, but to implement training to limit reviewers' sensitivity to gendered communication styles and to increase the number of female reviewers, who were found less likely to favor proposals authored by men than male reviewers.

Low success rates on applications for research funding

The issues addressed in the previous section is heightened by low success rates on grant applications to funding bodies across the world. Stephan (2013) argued that funders' preferences for funding research which is "doable" are strengthened when funding is scare vis-à-vis the demand for funding, and success rates on funding applications correspondingly low. As explanations for this effect on funder preferences, she mentioned that funding bodies often feel pressed to report successful research (Petsko 2012 as cited in Stephan 2013), and because "it is easier to justify funding safe bets when funding is in short supply" (Stephan 2013, p. 31). On a similar note, Alberts et al. (2014, p. 5774) argued in an essay on the state of biomedical research in the US that:

"Now that the percentage of NIH grant applications that can be funded has fallen from around 30% into the low teens, biomedical scientists are spending far too much of their time writing and revising grant applications and far too little thinking about science and conducting experiments. The low success rates have induced conservative, short-term thinking in applicants, reviewers, and funders. The system now favors those who can guarantee results rather than those with potentially path-breaking ideas that, by definition, cannot promise success. Young investigators are discouraged from departing too far from their postdoctoral work, when they should instead be posing new questions and inventing new approaches. Seasoned investigators are inclined to stick to their tried-and-true formulas for success rather than explore new fields."

As Laudel (2006b, p. 391) argued, for scientists, "As a result of shrinking success rates, it becomes even more important to know how to 'play the game'. This includes knowledge about which funding programs are available, what the formal rules of each funding scheme are and general knowledge about how to write a grant proposal."

Moreover, Nicholson & Ioannidis (2012) argued that low success rates have a tendency to make reviewers and funders more conservative, i.e. increase the likelihood that they bet on ”safe” academic profiles and projects. This increasing risk aversion as success rates decrease is likely associated with concerns that scarce, valuable resources are “wasted” on highly risky research ventures.

Length or type of funding

Bourke and Butler (1999) examined the impact of both short-term and long-term funding for biological research in Australia and concluded that researchers with stable institutional funding had higher scientific impact than peers with time-limited external grants. Moreover, among researchers with time-limited external grants, those with longer grants (i.e. up to five years) were more productive and more often cited than those with shorter (three-year) grants. According to the authors, these findings indicated that longer and especially stable funding may provide better conditions for the identification and pursuit of research problems that have a wider and deeper content, whereas short-term grants may induce researchers to focus on more predictable and less uncertainty problems. On a related note, Laudel and Gläser (2014, p. 2014) argued with reference to the grants of the European Research Council (ERC), that "‘funding schemes that offer large amounts of resources that can be flexibly used for a relatively longtime (five years and more) constitute an institutional innovation that increases the diversity of conditions for research.”

Laudel (2006a) examined how Australian and German physicists adapt to funding conditions and argued that competitive funding for research projects promotes low-risk, applied and more inflexible research. She further argued that such funding – which is typically provided for a set, limited number of years – serves as a disincentive to longer-term research questions that may for instance involve more uncertain or “playful” elements or seek to explore new connections between fields.

Heinze et al. (2009) explored institutional and organizational influences on creativity in scientific research, based on a case-based study of creative scientific research accomplishments in the fields of nanotechnology and human genetics in Europe and the US. Among other things, their study drew attention to the importance of stable research sponsorship (through some form of basic institutional funding or dedicated funding schemes for early career researchers). The authors concluded that the increase in competitive research funding at the expense of flexible institutional sponsorship posed a potential threat to creative science.

“… our findings suggest that the continued expansion of peer-reviewed funding, in particular at early stages of the research process, may eliminate ideas that are judged by peers as speculative, unorthodox, or transdisciplinary. Peer-review criteria, such as plausibility and validity tend to encourage conformity, while originality draws upon and encourages dissent. For this reason, funding arrangements based on peer review tend to discriminate against the early stages of exploratory research, as they have an inherent tendency to support conventional mainstream research and scientific work that follows established research lines while ignoring visionary and high-risk approaches.” (Heinze et al. 2009, p 620)

Azoulay, Graff Zivin, and Manso (2011) studied the careers of investigators of the Howard Hughes Medical Institute (HHMI), which they describe as a “funding people, not projects”-scheme characterized by long award cycles (five years and typically renewed at least once), tolerance of early failure, reward for long-term success, and for giving investigators considerable freedom to experiment. They compared HHMI Investigators with recipients of R01 grants from the National Institutes of Health (NIH), whose programs are characterized by short review cycles, predefined deliverables (rather than in-depth feedback on performance), and renewal policies that are not tolerant of failure. They found that the HHMI Investigators produced high-impact articles at a much higher rate than similarly accomplished scientists funded by the NIH. They were however also more likely to “flop”, i.e. publish more articles that failed to clear the citation bar of their least well cited pre-appointment work. Further examination by Azoulay and colleagues thus indicated that the HHMI Investigators were not simply “rising stars” that were picked up and funded by HHMI, but that they appeared to place more risky scientific bets subsequent to their appointment as HHMI investigators. The authors also found that HHMI Investigators’ work was characterized by more novel keywords than controls, and cited by a more diverse set of journals (as compared to before their appointment and to the control group). Their findings, the researchers argued, suggest that the HHMI program leads to changes in the research of HHMI Investigators, inducing them to explore novel research paths. However, Azoulay and colleagues did warn that it is unclear how easily, and at what cost, the program could be scaled up; they emphasized that their results might not generalize beyond the outstanding existing recipients of the HHMI Investigator grant to the broad population of scientists eligible for grant funding, and that the quality of the feedback provided by elite, recognized scientists would be likely to decline if a greater number of investigators were appointed, and the costs of providing feedback grew accordingly. Finally, the authors pointed out that the nature of the HHMI as a private foundation provides degrees of freedom not readily available to public foundations, which for instance must often provide funding for a wider set of scientists and research projects.

On a related note, Crossley (2015) pointed out that funding instruments that support people rather than projects tend to focus on applicants’ prior work and performance, which may disfavor early career researchers with great ideas but limited track records. Wilkinson (2010) pointed out that academic researchers often also engage in teaching as part of their job, but that the scope and extent of teaching responsibilities vary greatly among faculty members; as such, instruments that fund people rather than projects may favor researchers who have a higher proportion of their worktime available for research over those who have greater teaching duties.

Other research points to the role of research autonomy and flexibility in supporting novelty in science. Heinze et al. (2009) explored institutional and organizational influences on creativity in scientific research, based on a case-based study of creative scientific research accomplishments in the fields of nanotechnology and human genetics in Europe and the US. Among other factors, they drew attention to the importance of having a high degree of research autonomy within the larger set of research problems pursued by the group. They argued (p. 616) that the “Freedom to define and pursue individual scientific interests within or beyond a broadly defined thematic area is central to understanding why scientists and their groups are highly creative.”

Moreover, the authors drew attention to the importance of flexible research funding, that is, funding that was not earmarked for specific research purposes but available for group leaders to invest in the pursuit of high-risk research ideas that emerges from the group’s work. The authors recommended more flexibility from research funders in the use of grant income and fewer requests for ongoing progress and output reports in order to stimulate creativity in research.

Along a similar vein, Luukkonen and Thomas (2016) drew attention to the role of ensuring a “negotiated space” allowing university researchers autonomy in the selection of research topics and pursuit of scientific research, independent of implicit or explicit steering by e.g. external research funders and university strategies and policies. They also highlight the impact that the design of funding mechanisms can have on this 'negotiated space'. Similarly, (Whitley 2014) drew attention to how changes in the funding and governance of academic research influence scientists' "protected space" and their willingness to pursue unconventional, risky projects over extended periods of time.

Research groups' size and composition

The previously mentioned study by Uzzi et al. (2013), who examined 17.9 million research articles published over a period of five decades, research teams were significantly more likely to publish research combining familiar knowledge and novel perspectives than solo authors.

But how large should a team be to be innovative? Heinze et al. (2009) identified a number of factors as bring conducive to creativity in science included having a small group size of typically six to eight researchers (but sometimes as small as two to three people), and being affiliated with an organization sufficient access with access to a complementary variety of disciplinary and technical skills. The role of access to complementary skills and knowledge bases was explained by the fact that it provides access to specialized knowledge and/or instrumentation, allows for a rapid testing (and, when relevant, abandonment) of new ideas, and creates opportunities for stimulating exchanges and collaboration. The authors also found that leaders of creative research groups carefully managed their research groups, for instance by selecting new group members based on their complementary skills, and by ensuring flexibility to divert resources to the pursuit of new ideas and problems that arise during the course of research. The study also indicated that leaders of creative research groups played an important role in bridging disconnected knowledge domains, shaping the direction of groups' research, and in establishing a protected space within which group members could work.

Heinze et al. (ibid.) argued that the main advantages of small group size include allowing the research group leader to be actively involved in research and to support and drive productive exchanges within the group. Small groups also showed less hierarchical decision-making processes and enabled close mentor-student relationships, both of which the authors argued fueled creativity in the group's research. Based on their case studies the authors noted that several groups grew significantly in size during the period *after* the main creative event, seemingly to allow the group to follow up and capitalize on their opportunities created through their research, but potentially also having negative unintended effects including e.g. more hierarchical decision-making and lower degrees of involvement in group processes by the group leader.

On a related note, Lee, Walsh, and Wang (2015) drew attention to the increasing prevalence of team science (on this topic, see also e.g. Wuchty, Jones, and Uzzi 2007; Falk-Krzesinski et al. 2011) and analyzed the effect of team size, and field and task variety on creativity, based on an expectation derived from findings in prior studies that large teams are likely to be associated with greater variety in research fields and tasks, to have access to a broader knowledge base, and therefore able to generate more creative (here understood as more novel and/or useful) research outputs. However, other prior research suggests that groups may experience declining marginal benefits from larger and more diverse groups, potentially having a negative impact on the creativity and novelty of their outputs. The study by Lee, Walsh, and Wang (2015) drew on bibliometric and survey data to conclude that increasing team size has an inverted-U shaped relation with novelty, indicating that diminishing benefits to novelty of research outputs do set in as the size of the team grows. They also found that team size has a positive impact on the likelihood of producing a high-impact paper, but found no direct impact on impact of increasing knowledge variety, net of novelty and size.

Wu, Wang, and Evans (2019) also stressed the growth of large teams in all scientific areas, and a corresponding decrease in the prevalence of small teams and solitary researchers, as a result of increasing specialization of scientific activities, new opportunities for collaboration offered by advances in communication technology, and the complexity of problem solving, which calls for complex and often interdisciplinary solutions. The authors examined more than 65 million papers, patents and software products produced during the period 1954 to 2014 and conclude that smaller teams are more likely to disrupt science and technology (using a new citation-based index of the "disruptiveness" of science), offering novel perspectives and opportunities, while larger teams are more likely to further develop existing ideas and methods. Moreover, they found that small team contribution were more likely to draw on past work and to be viewed as disruptive. Based on their study, they argued that both small and large teams have an important role to play in advancing science, and that science policies should promote diversity in team sizes, based on the observed propensity of small teams to disrupt and of large teams to develop.[4]

A related stream of research looks at the relationship between team composition and the nature and impact of their work. For instance, Wagner, Whetsell, and Mukherjee (2019) cite prior research indicating that international research collaboration tends to be associated with higher scientific impact, and that diverse teams appear more likely to produce more novel research. They expected that international collaboration would lead to more creative research. However, using data from Web of Science and Scopus in 2005, they found that that international collaboration results in conventional research rather than novel or atypical research. The authors proposed transaction costs and communication barriers as possible explanations for lower levels of novelty, and that the higher impact of international collaborations may be explained by an "audience effect", that is, that authors from multiple countries provide access to a larger potential citing community. In conclusion, they called for greater emphasis on the incentivization of creativity and novelty in international research collaborations.

Other research suggests certain individuals may play a key role in driving novelty in science and in bridging various research teams. Wagner et al. (2015) investigated Nobel Laureates in Physiology or Medicine who received the Nobel Prize between 1969 and 2011 and compared them to a matched group of scientists. They found that the Laureates produced fewer papers but received higher average citations, and that are equally collaborative (compared to the matched group) but had a lower number of authors across their careers and also produced more sole-authored papers, both before and after winning the Nobel Prize. They also found that the Laurates were more likely than the matched group to build bridges across a research network, broking connections across so-called "structural holes" (Burt 2000). This, the authors argued, may provide the Laureates with non-redundant information that allows them to better differentiate their work within the network and "connect-to-the-connected", which in turn may facilitate originality in their research efforts.

[4] It should be noted that a recent SSRN paper by (Carayol, Agenor, and Oscar 2019) found that novel contributions to science were more often performed in larger teams that span more institutional boundaries and geographic areas.

Finally, other aspects of research teams and the individual researchers that make up these teams may matter for novelty in science. For instance, Heinze et al. (2009) drew attention to the role of researchers' mobility for creativity in science. Based on case studies of creative scientific research accomplishments in the fields of nanotechnology and human genetics in Europe and the US, they found that researchers tend to move to research environments that offer opportunities to move into new research fields or address risky research problems.

How can risk-taking be stimulated?

The survey of the literature has uncovered various suggestions for how risk-taking in science can be stimulated and supported. This final section presents key suggestions of this type, reiterating relevant points from the survey of the literature and presenting additional recommendations not described earlier in this report.

Funding can play a key role in strengthening conditions for risk-taking in science. Foster, Rzhetsky, and Evans (2015, p. 900) held that "agencies can lower the barriers to risky projects by funding them more aggressively", and argued that such interventions may be able to reduce conservatism in the science system. The question is *how much to invest?* As discussed earlier in this report, Heinze (2008), Wagner and Alexander (2013) and Foster, Rzhetsky, and Evans (2015) all argued that there is good reason to believe that more funding could be channeled into initiatives aimed at supporting risk-taking and novelty in science.

For instance, Heinze (2008) examined a number of programs aimed at increasing risk-taking and creativity in science, and noted that the programs examined generally had relatively small budgets (e.g. less than one percent of a funding agency's total budget), raising the question of whether this level of investment is sufficient to counter the disincentives for groundbreaking research. As he pointed out, a proper answer to this question would require insights into the ratio of groundbreaking to conventional research. As such, it is impossible to pinpoint the 'right' proportion of funding to spend on novel, risky research.

However, as previously described, Langfeldt (2001) found that peer review of novel funding applications was more likely to be supportive of risky, unconventional research when funding budgets are ample rather than restricted.

The next question is, how should such initiatives be designed? According to Wang, Veugelers, and Stephan (2017), an important unanswered question in the literature, is whether certain funding models encourage funding recipients to take a more exploratory approach than others. This explains why several researchers call for increased experimentation with alternative funding mechanisms (e.g. Ioannidis 2011; Nicholson and Ioannidis 2012; Feller 2016).

Although it offers no clear recipes for how to promote greater risk-taking in science, the literature does offer several suggestions for how funding models can be adapted to boost novelty and creativity.

For example, Heinze et al. (2009) emphasized the importance of *ensuring flexibility from research funders in the use of grant income,* rather than adhering strictly to original, narrow aims. This may give better conditions for researchers to abandon less interesting research paths and pursue more promising ideas and problems that emerge during the course of research.

On a related note, Heinze et al. (2009) also drew attention to the value of allowing researchers *a high degree of autonomy within broadly defined research aims*. Similarly, Ioannidis (2011) suggested keeqping funding applications short in length and broad in scope. The latter option, he recognized, may on the one hand lead scientists to present exaggerated promises and claims and risk favoring elite scientists, but may on the other hand reduce the workload associated with the development of proposals and leaves room for greater flexibility during the course of funded projects. These reflections are in line with the aforementioned work by Luukkonen and Thomas (2016) on funders' role in ensuring 'negotiated spaces' that safeguard researchers' autonomy in the selection of research topics and pursuit of scientific research, and work by (Whitley 2014) on how changes in the funding and governance of academic research influence scientists' "protected space" and their willingness to pursue unconventional, risky projects over extended periods of time.

Some researchers emphasized the value of *longer funding periods* for supporting experimentation and risk-taking (e.g. Bourke and Butler 1999; Heinze et al. 2009; Azoulay, Graff Zivin, and Manso 2011). This allows researchers greater scope for exploration and adjustment of research aims, and reduces incentives to ensure rapid or continuous tangible outputs from the research (as compared to e.g. two to three-year projects which are often expected to document outputs in the form of e.g. publications, training of young researchers, and maybe patentable discoveries). On a related note, Heinze et al. (2009) advised funders to make *fewer requests for ongoing progress and output reports* to stimulate creativity in research.

Closely associated to suggestions re. longer funding periods are suggestions to *provide progressive or continuous funding for high-risk research*. For instance, Ioannidis (2011) proposed to allow researchers with promising ideas apply for a series of small funding grants as long at the research continues to show promise. The aim of providing the funding in small portions is to reduce overall risk for the funding body.

Other suggestions in the literature focus on seeking to *increase success rates on applications for research funding* (e.g. Stephan 2013; Alberts et al. 2014; Laudel 2006b; Nicholson and Ioannidis 2012) or to *reduce the degree of concentration of research funding*, with a view to ensuring broader dispersal of research funding, increasing variety in research and securing funding for the growth layer of talent and ideas, particular those that emerge from the periphery of or entirely outside of established research fields and strong, well-funded research groups (see e.g. Aagaard, Kladakis, and Nielsen 2019).

For example, Ioannidis (2011) proposed allocating the entire research budget to eligible scientists in equal shares. The aims of this approach would be to reduce the effects of peer review bias, provide small amounts of funding for all researchers, and keep administrative burdens low; however, egalitarian funding is poorly suited for supporting larger and more costly research efforts and does not recognize the large contributions made by exceptional scientists. He also drew attention to the possible use of *lottery draws* among eligible scientists and/or applications. Again, this approach would keep administrative costs low and reduce the effects of peer review bias but would also overlook some deserving scientists. Lotteries do however recognize the degree of randomness in who gets funded and who doesn't and have also been put forth by e.g. Fang and Casadevall (2016), Gross and Bergstrom (2019) and by Guthrie, Ghiga, and Wooding (2018) as a possible response to the randomness that is associated with peer assessments when the number of qualified applicants cannot be recognized with the amount of funding available.[5]

In terms of more overarching approaches to the design of funding programs aimed at promoting risk-taking and novelty in research, Heinze (2008) identified two main approaches in use in a variety of such programs: programs that *fund outstanding individual scientists* in long-term pursuit of unconventional ideas, and programs that *fund unconventional ideas* that would be likely to be rejected under peer review but which can be pursued within the format of a research project (for instance with a view to developing the ideas to a point where they are better able to obtain other, more conventional sources of funding).

The first type of program, as described by Heinze (2008), tend to provide larger annual funding budgets and to provide funding over a longer term, on average five years. He also found that they typically account for a substantial share of the funding body's research spending. The HHMI Investigators program is an example of such a program, which was as previously mentioned examined by Azoulay, Graff Zivin, and Manso (2011), who drew attention to the importance of long award cycles, tolerance of early failure, reward for long-term success, and considerable freedom to experiment in explaining the success of the HHMI Investigators program.

Programs aimed at funding outstanding individuals are in line with the interest in "funding people, not projects" proposed by e.g. Ioannidis (2011) and Nicholson and Ioannidis (2012), who argued in favor of supporting researchers on the basis of exceptional originality and potential contribution to important advances, based on proposals presenting broad goals only. The authors acknowledged, however, that while providing favorable conditions for research who have proven their worth, this practice also risks favoring elite, well-connected researchers, implying that it may further strengthen the concentration of research funding and may therefore disfavor researchers who are less established or working on the fringes of established research areas. Moreover, this is a highly labor-intensive approach. Nonetheless, as argued by Rzhetsky et al. (2015), funding people over projects helps funders spread risk across a portfolio of experiments that pursue multiple research strategies.

The second type of program, aimed at funding unconventional ideas, typically accounting for a relatively small share of the total set of funding and instruments within the funding body, as found by Heinze (2008).

Heinze (2008) also identified several shortcomings of programs aimed at encouraging groundbreaking research and based on this advised:

- *Not to impose arbitrary a priori funding thresholds* but rather respond flexibly to the existing talent pool and its funding needs.
- *Taking into account applicants' current funding levels and the number of ongoing projects that the (and their research groups) are committed to.* None of the schemes examined by Heinze took into account applicant's level of core funding or their number of ongoing research projects, even though prior research indicates that additional resources may not promote creative research if for instance research groups are too large or key researchers are already committed to many ongoing projects.

[5] Adding to the discussions on the arguments for a lottery-approach to funding allocation, others have suggested exploring peer-to-peer systems for distributing research funding, see e.g. Bollen et al. (2017) and Barnett et al. (2017).

The suggestion to take into account applicant's current funding portfolio is line with recommendations from Rigby and Julian (2014).

- Heinze also argued in favor of *establishing funding programs for unconventional research in dedicated agencies rather than within existing funding organizations*, to avoid them becoming merely a residual funding category or a signal to the world of the funding organization's commitment to potentially revolutionary science. He further argued that in either of these cases, novelty, risk or creativity-oriented programs might be perceived as a threat to established programs and wisdoms about "good" proposals and assessment procedures, particularly as they signal that all other schemes funded by the body support research which is not novel, risky or creative. In reaction to such circumstances, he found that some programs sought to increase their legitimacy within their organization by either being absorbed into the usual selection and assessment processes that apply to other schemes (in which case they may not achieve their original aims, or at least not do so to the full extent possible), or to have their applicants subjected to special scrutiny (which may not only be counterproductive to the program's mission but also increasing administrative costs and thus potentially leading to the termination of the program).

Several researchers advise funders to *limit the use of bibliometrics in performance assessment*, especially in the short-term. As pointed out by Boudreau et al. (2016), a fundamental challenge in efforts to support potentially transformative science is that the true quality and potential of a research proposal cannot be observed – and is often even difficult to assess after the research has been executed. This, they argued, requires funders and grant reviewers to find other ways of assessing and selecting between applications.

Bibliometrics are increasingly used in the ex-ante and ex-post evaluation of applicants for funding, but as shown by Wang, Veugelers, and Stephan (2017), they have a bias against novelty, particularly when e.g. citation-based indicators rely on a standard, short time window, that does not reflect the amount of time needed for high-impact, novel work to accumulate citations.

This implies that funders should develop multiple approaches to assess applicants' publication profiles and performance, and entirely avoid short-term citation counts and other deeply problematic indicators such as journal impact factors. According to Wang, Veugelers, and Stephan (2017), other approaches may include using experts from outside the main field and to periodically examine the performance of grant applicants using five- or even ten-year windows.

Wang, Veugelers, and Stephan (2017) argued, moreover, that the bias against novelty that is strengthened by the inappropriate use of bibliometric indicators applied not only to funding decisions but also to science policy more generally, calling for wider changes in the ways in which we evaluate research.

This is in line with suggestions by other researchers, including a proposal by Rzhetsky et al. (2015) to *shift evaluation from the individual to the group* to cultivate productive risk-taking. Several authors also advise funders and employers to *not base either funding or hiring, promotion and tenure decisions on bibliometric indicators or the size of researchers' grant portfolio*. For instance, Wang, Veugelers, and Stephan (2017) warned against basing hiring and promotion decisions on poor bibliometrics indicators, as this is likely to disincentivize novel research. Instead, in a related piece, Stephan, Veugelers, and Wang (2017) emphasize the importance of universities ensuring that assessment and hiring committees actually *read* candidates' research instead of relying on e.g. bibliometric indicators. On a related note, Ioannidis (2011) warned against basing promotions or tenure decisions on the size of scientists' grant portfolio; avoiding this practice, he argued, would at least not reinforce scientists' incentives to pursue lower-risk ideas to secure funding, to the disadvantage of the pursuit of higher-risk but potentially higher-gain ideas. Also, Foster, Rzhetsky, and Evans (2015) advised decoupling job security from scientific productivity in order to promote greater originality in research. Similarly, Laudel (2006a) warned against using grants as an indicator of the quality of research or researchers, particularly when grant proposals are not reviewed by qualified peers in a competitive system or there is a problematically high degree of competition for scare funding.

Several publications covered in this survey of the literature are concerned with *how to address bias in peer review and grant assessment* and *how to ensure an appropriate composition of review panels.* For instance, Heinze (2008) found that almost all the creativity-inducing programs he examined used peer review to assess applicants, either through scientific advisory bodies or external reviewers. While fully aware of the conservative bias associated with peer review, the programs examined still aimed for unanimous judgment on the part of the reviewers, and controversy among reviewers was treated as a signal of lacking or uncertain quality and not of novelty. As stated by Heinze (2008, p. 316):

"Given the fact that the nine programs under review aim at funding 'high-risk' research questions, it is compelling that the decision process itself tends to be rather risk averse. Interviewees, including those from private foundations, typically argued that they want to make an investment that bears fruit; that their budget is relatively small; and that their decisions must be fully accountable. Thus, even in programs for high-risk research, the 'forces of exploitation' … remain strong."

This is in line with the recommendation from e.g. Langfeldt (2006) to *avoid requiring or preferring consensus among reviewers*.[6] Langfeldt (2001) moreover recommended ensuring that *reviewers are instructed to apply broad assessment criteria* in their reviews, as she found that novel and controversial projects fare better when funding budgets are ample and rating scales rough, as this is likely to produce identical scores for a higher number of proposals, enabling reviewers to consider other criteria and merits, including for instance the originality of the research. Langfeldt (2006) moreover argued that a focus on meeting established standards or ensuring a high degree of thoroughness in peer review is likely to favor "safe" over unconventional research. This, she argued, happens because such peer review processes are likely to employ a larger number of peers as reviewers, thereby increasing the likelihood that one or more of them is close-minded or even skeptical towards unconventional research. "When the purpose is to promote competition and 'conventional research quality'", she argued (p. 39) "high emphasis on screening out all projects that might be problematic is adequate. If on the other hand (part of) the purpose is to promote for instance interdisciplinarity, such a procedure may imply both a waste of resources and unnecessary barriers to interdisciplinary research. The process should rather focus on detecting all projects that might turn out to promote promising new interfaces between research

[6] However, it is worth noting that Barnett, Glisson, and Gallo (2018) did not find that funding proposals where reviewers disagreed in their assessment had a a higher than average return as indicated by subsequent citations to the proposed research. On the contrary, they found a clear increase in relative citations for proposals with a higher mean based on the aggregated scores of the reviewers. However, the authors call for larger-sample studies to further investigate the relationship between reviewer assessment of proposals and the subsequent scientific impact of the research proposed.

areas." In extension of this point, it is worth noting that van den Besselaar, Sandström, and Schiffbaenker (2018) through a linguistic analysis of review reports found that review panels rejected applications based on a search for weak points in the applications, and not based on an effort to identify the "high-risk", "high-gain" ideas that might be in the proposal, contributing to a sub-optimal selection.

Reviewer panels should be diverse and ensure appreciation for a wide range of fields. Langfeldt (2006, p. 38) advised funders to be wary of engaging "scholarly enemies" and "persons 'intolerant' to the kind of research under review" as reviewers and to consider potential conflicts of interests among reviewers or among reviewers and applicants in the assignment of reviewers to a panel. She even suggested considering alternatives to peer review, or supplementing it, by other methods, when the aim is to identify and support potentially groundbreaking research, arguing that the research is already submitted to peer review in other contexts, e.g. in connection with hiring decisions and publication of findings. For instance, she suggested setting up a commission or working group representing different a wide set of positions and interested parties and requesting their opinions on the proposed research, which might help guard against scholarly bias and random outcomes of typical peer review processes. On a similar note, Nicholson and Ioannidis (2012) gave an example of a funding organization that *uses impartial laymen* in their grant reviews to limit bias and counter the effects of strong personal opinions of individual researchers. Wang, Veugelers, and Stephan (2017) also suggested including perspectives from experts with outside-field expertise and having interdisciplinary panels evaluate proposals for novel research efforts. On a similar note, Alberts et al. (2014) argued in favor of including a diversity of research fields on review panels, arguing that experienced scientists with an appreciation for different fields can help counteract bias and insular tendencies in peer review. Meanwhile, as previously mentioned, Nicholson and Ioannidis (2012) cautioned against allowing grant holders to serve as grant reviewers, arguing that this practice introduces an inevitable conflict of interest, which may result in potentially promising novel ideas being deselected. Finally, the survey of the literature indicated that *blinded review* processes may help counter reviewer bias associated with e.g. age and ethnicity, but not gender differences in communication styles in the wording of funding proposals, as pointed out by Kolev, Fuentes-Medel, and Murray (2019). In consequence, Kolev and colleagues recommended implementing training to limit reviewers' sensitivity to gendered communication styles and to increase the number of female reviewers, who were found less likely to favor proposals authored by men than male reviewers.

Turning our attention from external funding to internal funding for research, some researchers have pointed to *the importance of institutional funding for novel research* (e.g. Bourke & Butler 1999; Laudel 2006a; Heinze et al. 2009; Tatsioni, Vavva, and Ioannidis 2010), particularly in its early stages when it is particularly vulnerable to bias in peer review processes and for research areas that are not adequately supported by external funding. For instance, Laudel (2006a) argued in favor of counter-mechanisms to external funding that help maintain scientists in the research system and work against tendencies in external funding allocation to favor mainstream, low-risk research and to help ensure continuity in research efforts in light of fluctuating funding.

Employment conditions for scientists, like the allocation of institutional funding, are primarily affected by the institutions who employ scientists. Here, Zoller, Zimmerling, and Boutellier (2014) found evidence that *securing permanent positions for researchers* increases the likelihood that they will engage in risky research.

Rzhetsky et al. (2015) pointed out that other mechanisms can also be considered to promote novelty and risk-taking in research, including *scientific prizes that may promote risky experiments with great potential value for industry and society.* This is in line with research by Franssen et al. (2018), who argued that prizes differ from project-specific research grants in that they allow for a more flexible use of funds during the research process and for greater deviation from epistemic and organizational standards.

Another point that emerges from the survey of the literature is a call for *strengthened possibilities to publish failures and null results* in scientific outlets (e.g. Rzhetsky et al. 2015). This is crucial, as promoting greater risk-taking requires a greater tolerance for failures or lacking results (ibid.), which today often remain unpublished, which may cause researchers to select research projects or to present their research in such a way as to increase their chances of getting published in prestigious outlets (Kühberger, Fritz, and Scherndl 2014; Franco, Malhotra, and Simonovits 2014; Fanelli 2010; 2012). On this topic, it is worth noting that the role of journal editors in making final decisions on the acceptance or rejection of manuscripts is widely overlooked, and that characteristics of the individual editors may play a substantial role in shaping their willingness to accept innovative research (Petersen 2017).

Finally, Alberts et al. (2014) called for *wider changes in science policy* with the aim of making the research environment more sustainable, based on their observations of changes in the biomedical research environment in the US. Their suggested changes included ensuring predictable, long-term budgets for public research funding agencies and striving for changes in the composition of the academic workforce, including increasing the number of staff scientists (e.g. vis-à-vis postdocs in “soft money” positions), and to rebalance the research portfolio to achieve a better balance between smaller and large projects and better conditions for proposals for imaginative, long-term research.

References

Aagaard, Kaare, Alexander Kladakis, and Mathias W. Nielsen. 2019. “Concentration or Dispersal of Research Funding?” *Quantitative Science Studies*, August, 1–29. https://doi.org/10.1162/qss_a_00002.

Alberts, Bruce. 2009. “On Incentives for Innovation.” *Science* 326 (5957): 1163–1163. https://doi.org/10.1126/science.1184848.

Alberts, Bruce, Marc W. Kirschner, Shirley Tilghman, and Harold Varmus. 2014. “Rescuing US Biomedical Research from Its Systemic Flaws.” *Proceedings of the National Academy of Sciences* 111 (16): 5773–77. https://doi.org/10.1073/pnas.1404402111.

Arrow, Kenneth. 1962. “Economic Welfare and the Allocation of Resources for Invention.” *1962* 5 (January). https://doi.org/10.1007/978-1-349-15486-9_13.

Aviña, Glory E., Christian D. Schunn, Austin R. Silva, Travis L. Bauer, George W. Crabtree, Curtis M. Johnson, Toluwalogo Odumosu, et al. 2018. “The Art of Research: A Divergent/Convergent Thinking Framework and Opportunities for Science-Based Approaches.” In *Engineering a Better Future: Interplay between Engineering, Social Sciences, and Innovation*, edited by Eswaran Subrahmanian, Toluwalogo Odumosu, and Jeffrey Y. Tsao, 167–86. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-91134-2_14.

Azoulay, Pierre, Joshua S. Graff Zivin, and Gustavo Manso. 2011. “Incentives and Creativity: Evidence from the Academic Life Sciences.” *The RAND Journal of Economics* 42 (3): 527–54. https://doi.org/10.1111/j.1756-2171.2011.00140.x.

Azoulay, Pierre, Joshua S. Graff Zivin, and Gustavo Manso. 2012. “NIH Peer Review: Challenges and Avenues for Reform.” Working Paper 18116. National Bureau of Economic Research. https://doi.org/10.3386/w18116.

Banal-Estañol, Albert, Inés Macho-Stadler, and David Pérez-Castrillo. 2019. “Evaluation in Research Funding Agencies: Are Structurally Diverse Teams Biased Against?” *Research Policy* 48 (7): 1823–40. https://doi.org/10.1016/j.respol.2019.04.008.

Barnett, Adrian G., Philip Clarke, Cedryck Vaquette, and Nicholas Graves. 2017. “Using Democracy to Award Research Funding: An Observational Study.” *Research Integrity and Peer Review* 2: 16. https://doi.org/10.1186/s41073-017-0040-0.

Barnett, Adrian G., Scott R. Glisson, and Stephen Gallo. 2018. “Do Funding Applications Where Peer Reviewers Disagree Have Higher Citations? A Cross-Sectional Study.” *F1000Research* 7: 1030. https://doi.org/10.12688/f1000research.15479.2.

Bateman, Thomas S., and Andrew M. Hess. 2015. “Different Personal Propensities among Scientists Relate to Deeper vs. Broader Knowledge Contributions.” *Proceedings of the National Academy of Sciences* 112 (12): 3653–58. https://doi.org/10.1073/pnas.1421286112.

Besselaar, Peter van den, Ulf Sandström, and Hélène Schiffbaenker. 2018. “Studying Grant Decision-Making: A Linguistic Analysis of Review Reports.” *Scientometrics* 117 (1): 313–29. https://doi.org/10.1007/s11192-018-2848-x.

Bol, Thijs, Mathijs de Vaan, and Arnout van de Rijt. 2018. “The Matthew Effect in Science Funding.” *Proceedings of the National Academy of Sciences of the United States of America* 115 (19): 4887–90. https://doi.org/10.1073/pnas.1719557115.

Bollen, Johan, David Crandall, Damion Junk, Ying Ding, and Katy Börner. 2017. “An Efficient System to Fund Science: From Proposal Review to Peer-to-Peer Distributions.” *Scientometrics* 110 (1): 521–28. https://doi.org/10.1007/s11192-016-2110-3.

Boudreau, Kevin J., Eva C. Guinan, Karim R. Lakhani, and Christoph Riedl. 2016. “Looking Across and Looking Beyond the Knowledge Frontier: Intellectual Distance, Novelty, and Resource Allocation in Science.” *Management Science* 62 (10): 2765–83. https://doi.org/10.1287/mnsc.2015.2285.

Bourdieu, Pierre. 1975. “The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason.” *Information (International Social Science Council)* 14 (6): 19–47. https://doi.org/10.1177/053901847501400602.

Bourke, Paul, and Linda Butler. 1999. “The Efficacy of Different Modes of Funding Research: Perspectives from Australian Data on the Biological Sciences.” *Research Policy* 28 (5): 489–99. https://doi.org/10.1016/S0048-7333(99)00009-8.

Bromham, Lindell, Russell Dinnage, and Xia Hua. 2016. “Interdisciplinary Research Has Consistently Lower Funding Success.” *Nature* 534 (7609): 684–87. https://doi.org/10.1038/nature18315.

Burt, Ronald S. 2000. “The Network Structure Of Social Capital.” *Research in Organizational Behavior* 22 (January): 345–423. https://doi.org/10.1016/S0191-3085(00)22009-1.

Campanario, Juan Miguel. 2009. “Rejecting and Resisting Nobel Class Discoveries: Accounts by Nobel Laureates.” *Scientometrics* 81 (2): 549–65. https://doi.org/10.1007/s11192-008-2141-5.

Carayol, Nicolas, Lahatte Agenor, and Llopis Oscar. 2019. “The Right Job and the Job Right: Novelty, Impact and Journal Stratification in Science.” SSRN Scholarly Paper ID 3347326. Rochester, NY: Social Science Research Network. https://papers.ssrn.com/abstract=3347326.

Chai, Sen, and Anoop Menon. 2019. “Breakthrough Recognition: Bias against Novelty and Competition for Attention.” *Research Policy* 48 (3): 733–47. https://doi.org/10.1016/j.respol.2018.11.006.

Crossley, Merlin. 2015. “Science Funding Should Go to People, Not Projects.” The Conversation, June 29, 2015. 2015. http://theconversation.com/science-funding-should-go-to-people-not-projects-44001.

Dalen, Hendrik P. van. 1999. “The Golden Age of Nobel Economists.” *The American Economist* 43 (2): 19–35. https://doi.org/10.1177/056943459904300203.

DEA. 2019. “Koncentration Af Konkurrenceudsatte Forskningsmidler: Er Der Kommet Flere Penge På Færre Hænder Og, i Så Fald, Er Det et Problem for Dansk Forskning?” DEA. https://dea.nu/sites/dea.nu/files/konkurrence_og_koncentration_0.pdf.

Dietz, James S., and Juan D. Rogers. 2012. “Meanings and Policy Implications of ‘Transformative Research’: Frontiers, Hot Science, Evolution, and Investment Risk.” *Minerva* 50 (1): 21–44. https://doi.org/10.1007/s11024-012-9190-x.

Falk-Krzesinski, Holly J., Noshir Contractor, Stephen M. Fiore, Kara L. Hall, Cathleen Kane, Joann Keyton, Julie Thompson Klein, Bonnie Spring, Daniel Stokols, and William Trochim. 2011. “Mapping a Research Agenda for the Science of Team Science.” *Research Evaluation* 20 (2): 145–58. https://doi.org/10.3152/095820211X12941371876580.

Fanelli, Daniele. 2010. “‘Positive’ Results Increase Down the Hierarchy of the Sciences.” *PLOS ONE* 5 (4): e10068. https://doi.org/10.1371/journal.pone.0010068.

———. 2012. “Negative Results Are Disappearing from Most Disciplines and Countries.” *Scientometrics* 90 (3): 891–904. https://doi.org/10.1007/s11192-011-0494-7.

Fang, Ferric C., and Arturo Casadevall. 2016. “Research Funding: The Case for a Modified Lottery.” *MBio* 7 (2): e00422-16. https://doi.org/10.1128/mBio.00422-16.

Feller, Irwin. 2016. “Interdisciplinary Research and Transformative Research as Facets of National Science Policy.” In , 243–73. https://doi.org/10.1057/978-1-137-59420-4_9.

Foster, Jacob G., Andrey Rzhetsky, and James A. Evans. 2015. “Tradition and Innovation in Scientists’ Research Strategies.” *American Sociological Review* 80 (5): 875–908. https://doi.org/10.1177/0003122415601618.

Franco, Annie, Neil Malhotra, and Gabor Simonovits. 2014. “Social Science. Publication Bias in the Social Sciences: Unlocking the File Drawer.” *Science (New York, N.Y.)* 345 (6203): 1502–5. https://doi.org/10.1126/science.1255484.

Franssen, Thomas, Wout Scholten, Laurens K. Hessels, and Sarah de Rijcke. 2018. “The Drawbacks of Project Funding for Epistemic Innovation: Comparing Institutional Affordances and Constraints of Different Types of Research Funding.” *Minerva* 56 (1): 11–33. https://doi.org/10.1007/s11024-017-9338-9.

Garfield, Eugene. 1980. “Premature Discovery or Delayed Recognition-Why.” *Current Contents* 21: 5–10.

Geman, Donald, and Stuart Geman. 2016. “Opinion: Science in the Age of Selfies.” *Proceedings of the National Academy of Sciences* 113 (34): 9384–87. https://doi.org/10.1073/pnas.1609793113.

Gross, Kevin, and Carl T. Bergstrom. 2019. “Contest Models Highlight Inherent Inefficiencies of Scientific Funding Competitions.” Edited by John PA Ioannidis. *PLOS Biology* 17 (1): e3000065. https://doi.org/10.1371/journal.pbio.3000065.

Guthrie, Susan, Ioana Ghiga, and Steven Wooding. 2018. “What Do We Know about Grant Peer Review in the Health Sciences?” *F1000Research* 6 (March): 1335. https://doi.org/10.12688/f1000research.11917.2.

Heinze, Thomas. 2008. “How to Sponsor Ground-Breaking Research: A Comparison of Funding Schemes.” *Science and Public Policy* 35 (5): 302–18. https://doi.org/10.3152/030234208X317151.

———. 2013. “Creative Accomplishments in Science: Definition, Theoretical Considerations, Examples from Science History, and Bibliometric Findings.” *Scientometrics* 95 (3): 927–40. https://doi.org/10.1007/s11192-012-0848-9.

Heinze, Thomas, Philip Shapira, Juan D. Rogers, and Jacqueline M. Senker. 2009. “Organizational and Institutional Influences on Creativity in Scientific Research.” *Research Policy*, Special Issue: Emerging Challenges for Science, Technology and Innovation Policy Research: A Reflexive Overview, 38 (4): 610–23. https://doi.org/10.1016/j.respol.2009.01.014.

Ioannidis, John P. A. 2011. “More Time for Research: Fund People Not Projects.” Comments and Opinion. Nature. September 28, 2011. https://doi.org/10.1038/477529a.

Jones, Benjamin F., and Bruce A. Weinberg. 2011. “Age Dynamics in Scientific Creativity.” *Proceedings of the National Academy of Sciences* 108 (47): 18910. https://doi.org/10.1073/pnas.1102895108.

Kolev, Julian, Yuly Fuentes-Medel, and Fiona Murray. 2019. “Is Blinded Review Enough? How Gendered Outcomes Arise Even Under Anonymous Evaluation.” Working Paper 25759. National Bureau of Economic Research. https://doi.org/10.3386/w25759.

Kühberger, Anton, Astrid Fritz, and Thomas Scherndl. 2014. “Publication Bias in Psychology: A Diagnosis Based on the Correlation between Effect Size and Sample Size.” *PLOS ONE* 9 (9): e105825. https://doi.org/10.1371/journal.pone.0105825.

Kuhn, Thomas. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Kuhn, Thomas S. 1977. *The Essential Tension : Selected Studies in Scientific Tradition and Change / Thomas S. Kuhn*. Accessed from https://nla.gov.au/nla.cat-vn2250139. Chicago: University of Chicago Press.

Langfeldt, Liv. 2001. “The Decision-Making Constraints and Processes of Grant Peer Review, and Their Effects on the Review Outcome.” *Social Studies of Science* 31 (6): 820–41. https://doi.org/10.1177/030631201031006002.

———. 2006. “The Policy Challenges of Peer Review: Managing Bias, Conflict of Interests and Interdisciplinary Assessments.” *Research Evaluation* 15 (1): 31–41. https://doi.org/10.3152/147154406781776039.

Laudel, Grit. 2006a. “The Art of Getting Funded: How Scientists Adapt to Their Funding Conditions.” *Science and Public Policy* 33 (7): 489–504. https://doi.org/10.3152/147154306781778777.

———. 2006b. “The ‘Quality Myth’: Promoting and Hindering Conditions for Acquiring Research Funds.” *Higher Education* 52 (3): 375–403. https://doi.org/10.1007/s10734-004-6414-5.

Laudel, Grit, and Jochen Gläser. 2014. “Beyond Breakthrough Research: Epistemic Properties of Research and Their Consequences for Research Funding.” *Research Policy* 43 (7): 1204–16. https://doi.org/10.1016/j.respol.2014.02.006.

Lawrence, Peter A. 2009. “Real Lives and White Lies in the Funding of Scientific Research.” *PLOS Biology* 7 (9): e1000197. https://doi.org/10.1371/journal.pbio.1000197.

Leahey, Erin, Christine M. Beckman, and Taryn L. Stanko. 2017. “Prominent but Less Productive: The Impact of Interdisciplinarity on Scientists’ Research.” *Administrative Science Quarterly* 62 (1): 105–39. https://doi.org/10.1177/0001839216665364.

Lee, You-Na, John P. Walsh, and Jian Wang. 2015. “Creativity in Scientific Teams: Unpacking Novelty and Impact.” *Research Policy* 44 (3): 684–97. https://doi.org/10.1016/j.respol.2014.10.007.

Li, Danielle. 2017. “Expertise versus Bias in Evaluation: Evidence from the NIH.” *American Economic Journal: Applied Economics* 9 (2): 60–92. https://doi.org/10.1257/app.20150421.

Li, Danielle, and Leila Agha. 2015. “Big Names or Big Ideas: Do Peer-Review Panels Select the Best Science Proposals?” *Science* 348 (6233): 434–38. https://doi.org/10.1126/science.aaa0185.

Luukkonen, Terttu. 2012. “Conservatism and Risk-Taking in Peer Review: Emerging ERC Practices.” *Research Evaluation* 21 (1): 48–60. https://doi.org/10.1093/reseval/rvs001.

Luukkonen, Terttu, and Duncan A. Thomas. 2016. "The 'Negotiated Space' of University Researchers' Pursuit of a Research Agenda." *Minerva* 54 (1): 99–127. https://doi.org/10.1007/s11024-016-9291-z.

Lyall, Catherine, Ann Bruce, Wendy Marsden, and Laura Meagher. 2013. "The Role of Funding Agencies in Creating Interdisciplinary Knowledge." *Science and Public Policy* 40 (1): 62–71. https://doi.org/10.1093/scipol/scs121.

March, James, G. 1991. "Exploration and Exploitation in Organizational Learning." *Organization Science* 2 (1): 71–87. https://doi.org/10.1287/orsc.2.1.71.

Martin, Ben R. 2013. "Whither Research Integrity? Plagiarism, Self-Plagiarism and Coercive Citation in an Age of Research Assessment." *Research Policy* 42 (5): 1005–14. https://doi.org/10.1016/j.respol.2013.03.011.

———. 2016. "What's Happening to Our Universities?" *Prometheus* 34 (1): 7–24. https://doi.org/10.1080/08109028.2016.1222123.

Melin, Göran, and Rickard Danell. 2006. "The Top Eight Percent: Development of Approved and Rejected Applicants for a Prestigious Grant in Sweden." *Science and Public Policy* 33 (10): 702–12. https://doi.org/10.3152/147154306781778579.

Merton, Robert K. 1968. "The Matthew Effect in Science: The Reward and Communication Systems of Science Are Considered." *Science* 159 (3810): 56–63. https://doi.org/10.1126/science.159.3810.56.

Mueller, Jennifer S., Shimul Melwani, and Jack A. Goncalo. 2012. "The Bias Against Creativity: Why People Desire but Reject Creative Ideas." *Psychological Science* 23 (1): 13–17. https://doi.org/10.1177/0956797611421018.

National Science Board (USA). 2007. "Enhancing Support of Transformative Research at the National Science Foundation." National Science Foundation. https://www.nsf.gov/nsb/documents/2007/tr_report.pdf.

Nelson, Richard R. 1959. "The Simple Economics of Basic Scientific Research." *Journal of Political Economy* 67 (3): 297–306.

Nicholson, Joshua M., and John P. A. Ioannidis. 2012. "Research Grants: Conform and Be Funded." Comments and Opinion. Nature. December 5, 2012. https://doi.org/10.1038/492034a.

Petersen, Jessica. 2017. "How Innovative Are Editors?: Evidence across Journals and Disciplines." *Research Evaluation* 26 (3): 256–68. https://doi.org/10.1093/reseval/rvx015.

Petsko, Gregory A. 2012. "Goodbye, Columbus." *Genome Biology* 13 (5): 155. https://doi.org/10.1186/gb-2012-13-5-155.

Polanyi, Michael. 1969. *Knowing and Being: Essays*. Routledge and Kegan Paul.

Prendergast, P.J., S.H. Brown, and J.R. Britton. 2008. "Research Programmes That Promote Novel, Ambitious, Unconventional and High-Risk Research: An Analysis." *Industry and Higher Education* 22 (4): 215–21. https://doi.org/10.5367/000000008785201793.

Raan, Anthony F. J. van. 2004. "Sleeping Beauties in Science." *Scientometrics* 59 (3): 467–72. https://doi.org/10.1023/B:SCIE.0000018543.82441.f1.

Rigby, J., and K. Julian. 2014. "On the Horns of a Dilemma: Does More Funding for Research Lead to More Research or a Waste of Resources That Calls for Optimization of Researcher Portfolios? An Analysis Using Funding Acknowledgement Data." *Scientometrics* 101 (2): 1067–75. https://doi.org/10.1007/s11192-014-1259-x.

Rosenberg, Nathan. 1990. "Why Do Firms Do Basic Research (with Their Own Money)?" *Research Policy* 19 (2): 165–74. https://doi.org/10.1016/0048-7333(90)90046-9.

Rosenberg, Nathan, and Richard R. Nelson. 1994. "American Universities and Technical Advance in Industry." *Research Policy* 23 (3): 323–48. https://doi.org/10.1016/0048-7333(94)90042-6.

Rosenthal, Robert. 1979. "The File Drawer Problem and Tolerance for Null Results." *Psychological Bulletin* 86 (3): 638–41. https://doi.org/10.1037/0033-2909.86.3.638.

Rzhetsky, Andrey, Jacob G. Foster, Ian T. Foster, and James A. Evans. 2015. "Choosing Experiments to Accelerate Collective Discovery." *Proceedings of the National Academy of Sciences*, November, 201509757. https://doi.org/10.1073/pnas.1509757112.

Shibayama, Sotaro. 2011. "Distribution of Academic Research Funds: A Case of Japanese National Research Grant." *Scientometrics* 88 (1): 43–60. https://doi.org/10.1007/s11192-011-0392-z.

VI FREMMER VIDEN

Snyder, Hannah. 2019. “Literature Review as a Research Methodology: An Overview and Guidelines.” *Journal of Business Research* 104 (November): 333–39. https://doi.org/10.1016/j.jbusres.2019.07.039.

Stavropoulou, Charitini, Melek Somai, and John P. A. Ioannidis. 2019. “Most UK Scientists Who Publish Extremely Highly-Cited Papers Do Not Secure Funding from Major Public and Charity Funders: A Descriptive Analysis.” *PloS One* 14 (2): e0211460. https://doi.org/10.1371/journal.pone.0211460.

Stent, Gunther S. 1972. “Prematurity and Uniqueness in Scientific Discovery.” *Scientific American* 227 (6): 84–93.

Stephan, Paula. 2013. “The Endless Frontier: Reaping What Bush Sowed?” Working Paper 19687. National Bureau of Economic Research. https://doi.org/10.3386/w19687.

Stephan, Paula, Reinhilde Veugelers, and Jian Wang. 2017. “Reviewers Are Blinkered by Bibliometrics.” *Nature News* 544 (7651): 411. https://doi.org/10.1038/544411a.

Sternberg, Robert J. 1998. “Costs and Benefits of Defying the Crowd in Science.” *Intelligence* 26 (3): 209–15. https://doi.org/10.1016/S0160-2896(99)80003-8.

Stokes, Donald E. 1997. *Pasteur’s Quadrant: Basic Science and Technological Innovation*. Washington, D.C: Brookings Institution Press.

Tatsioni, Athina, Effie Vavva, and John P. A. Ioannidis. 2010. “Sources of Funding for Nobel Prize-Winning Work: Public or Private?” *FASEB Journal: Official Publication of the Federation of American Societies for Experimental Biology* 24 (5): 1335–39. https://doi.org/10.1096/fj.09-148239.

Uzzi, Brian, Satyam Mukherjee, Michael Stringer, and Ben Jones. 2013. “Atypical Combinations and Scientific Impact.” *Science* 342 (6157): 468–72. https://doi.org/10.1126/science.1240474.

Viner, Neil, Philip Powell, and Rod Green. 2004. “Institutionalized Biases in the Award of Research Grants: A Preliminary Analysis Revisiting the Principle of Accumulative Advantage.” *Research Policy* 33 (3): 443–54. https://doi.org/10.1016/j.respol.2003.09.005.

Wagner, Caroline S., and Jeffrey Alexander. 2013. “Evaluating Transformative Research Programmes: A Case Study of the NSF Small Grants for Exploratory Research Programme.” *Research Evaluation* 22 (3): 187–97. https://doi.org/10.1093/reseval/rvt006.

Wagner, Caroline S., Edwin Horlings, Travis A. Whetsell, Pauline Mattsson, and Katarina Nordqvist. 2015. “Do Nobel Laureates Create Prize-Winning Networks? An Analysis of Collaborative Research in Physiology or Medicine.” *PLoS ONE* 10 (7). https://doi.org/10.1371/journal.pone.0134164.

Wagner, Caroline S., Travis A. Whetsell, and Satyam Mukherjee. 2019. “International Research Collaboration: Novelty, Conventionality, and Atypicality in Knowledge Recombination.” *Research Policy* 48 (5): 1260–70. https://doi.org/10.1016/j.respol.2019.01.002.

Wang, Jian, You-Na Lee, and John P. Walsh. 2018. “Funding Model and Creativity in Science: Competitive versus Block Funding and Status Contingency Effects.” *Research Policy*, March. https://doi.org/10.1016/j.respol.2018.03.014.

Wang, Jian, Reinhilde Veugelers, and Paula Stephan. 2017. “Bias against Novelty in Science: A Cautionary Tale for Users of Bibliometric Indicators.” *Research Policy* 46 (8): 1416–36. https://doi.org/10.1016/j.respol.2017.06.006.

Whitley, Richard. 2000. *The Intellectual and Social Organization of the Sciences*. Vol. 11. https://doi.org/10.2307/2070338.

———. 2014. “How Do Institutional Changes Affect Scientific Innovations? The Effects of Shifts in Authority Relationships, Protected Space, and Flexibility.” Book-part. July 26, 2014. https://doi.org/10.1108/S0733-558X20140000042012.

Wilkinson, Emma. 2010. “Wellcome Trust to Fund People Not Projects.” *Lancet (London, England)* 375 (9710): 185–86. https://doi.org/10.1016/S0140-6736(10)60075-X.

Wong, Geoff, Trish Greenhalgh, Gill Westhorp, Jeanette Buckingham, and Ray Pawson. 2013. “RAMESES Publication Standards: Meta-Narrative Reviews.” *BMC Medicine* 11 (1): 20. https://doi.org/10.1186/1741-7015-11-20.

Wu, Lingfei, Dashun Wang, and James A. Evans. 2019. “Large Teams Develop and Small Teams Disrupt Science and Technology.” *Nature* 566 (7744): 378. https://doi.org/10.1038/s41586-019-0941-9.

Wuchty, Stefan, Benjamin F. Jones, and Brian Uzzi. 2007. “The Increasing Dominance of Teams in Production of Knowledge.” *Science* 316 (5827): 1036–39. https://doi.org/10.1126/science.1136099.

Yao, Lixia, Ying Li, Soumitra Ghosh, James A Evans, and Andrey Rzhetsky. 2015. “Health ROI as a Measure of Misalignment of Biomedical Needs and Resources.” *Nature Biotechnology* 33 (August): 807.

Yegros-Yegros, Alfredo, Ismael Rafols, and Pablo D’Este. 2015. “Does Interdisciplinary Research Lead to Higher Citation Impact? The Different Effect of Proximal and Distal Interdisciplinarity.” *PLOS ONE* 10 (8): e0135095. https://doi.org/10.1371/journal.pone.0135095.

Zoller, F.A., E. Zimmerling, and R. Boutellier. 2014. “Assessing the Impact of the Funding Environment on Researchers’ Risk Aversion: The Use of Citation Statistics.” *Higher Education* 68 (3): 333–45. https://doi.org/10.1007/s10734-014-9714-4.