

Funding Breakthrough Research. An Update and Research Agenda

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1. Introduction

Scientific progress and technological change are central to solving human problems and fostering economic growth and social well-being. Progress typically comes in incremental improvements to existing scientific and technological paradigms. But once in a while, the technology frontier is shifted by important breakthroughs. The research that leads to such improvements, which we call here *breakthrough research*, is not purely a matter of luck, but often pioneering, relying on unconventional, innovative approaches that take a long and bumpy road and require patience and tolerance for failure. Because the probability of success is typically small but the potential payoff is very large, breakthrough research is often described as high-risk, high-gain research.

Concerns have been raised that the emergence of breakthroughs has failed to keep pace with the large increase of scientific productivity and total investments over time (Bloom et al., 2020; Park et al., 2023), raising fears that the current research environment is insufficiently supportive of the pioneering science needed to achieve breakthroughs (Alberts et al., 2014; Petsko, 2012).

In Franzoni, Stephan and Veugelers (2022) we illustrated the challenges in getting support for research faced by scientists who did the pioneering research behind mRNA-based drugs. We used that exceptional moment for science in society's battle against the COVID pandemic, to reflect on whether the current scientific environment is sufficiently supportive of research needed for key breakthroughs, and whether the system of funding encourages sufficient risk-taking to induce scientists to explore transformative paths. We suggested possible interventions to foster risk-taking and avoid missing out on future breakthroughs.

Since that paper was published, the landscape has evolved considerably in terms of new empirical indicators and analyses of biases against novelty in funding science, as well as new science funding policy experimentation. This chapter revisits the core arguments, integrates recent evidence, highlighting the newest areas of experimentations, and reflects on emerging trends. Most notable among these are a tightening of public science funding in the US, a growing emphasis on directing research towards national priorities and tangible impact, but also an effort to modify the use of bibliometrics. These shifts further underscore the tension between breakthrough science and the established structures that shape funding

decisions. We conclude by drawing a research agenda for scholars and policy makers moving forward.

2. Breakthrough research: features, measures and implications

Scientific breakthroughs are jumps in conventional knowledge leading to the emergence of new scientific and technological trajectories (Kuhn, 1991; Kuhn, 1962; Schumpeter, 1942). Breakthroughs can only be identified and defined in terms of the outcome of the research, and therefore *ex-post*. However, fostering breakthroughs would ideally require to know *ex-ante* which research has more potential to lead to exceptional outcomes. This is considerably more challenging and requires looking at the *input* rather than the *output* of the research process.

Foundational theories of scientific and technological change describe research leading to breakthroughs as pathbreaking, unconventional, highly uncertain and subject to frequent setbacks and failures (Kuhn, 1962; Merton & Barber, 2004; Zuckerman, 1977). It often explores topics that are understudied, or at the frontier of research, challenge common assumptions and theories, or focus on unexplained anomalies (Kuhn, 1962; Margolis, 1993; Chai, 2017). Such approaches have often ambitious planned impact, although the final outcomes may be unexpected and distant from the initial intentions (Merton & Barber, 2004; Yaqub, 2018; Laudel & Gläser, 2014; Strokes, 1997). Breakthrough research also makes extensive use of new ideas and methods (Arts and Fleming, 2018; Chai and Menon, 2019), actively recombine distant bodies of knowledge (Fleming, 2001; Hargadon & Sutton, 1997; Lee et al., 2015), or pivot from existing trajectories in unusual ways (Uzzi et al., 2013; Veugelers & Wang, 2019; Hill et al., 2025), sometimes, though not always, collaborating within and across the boundaries of the communities or disciplines (Wang et al., 2017; Chai & Menon, 2019; Singh & Fleming, 2009; Wu et al., 2019).

Although, in principle, these studies describe riskiness, novelty, and interdisciplinarity as the conditions and attributes of research associated with breakthrough outcomes, it is important to note that none of these elements is necessary or sufficient to generate breakthroughs. Although *ex ante* attributes are not able to predict perfectly which research will eventually produce a breakthrough, they can identify research that has *ex ante* a higher probability of doing so. Studies of breakthrough research follow the same rationale: they

cannot identify projects that would deliver a breakthrough, but they can identify projects with greater potential to do so.

One approach to study breakthrough research involves looking at projects that addresses unusual topics or that make some unusual combinations of prior knowledge. For example, Foster et al. (2015) examine studies of previously unexplored chemical relationships—jumping beyond current knowledge. They find that such jumps are more likely to achieve high impact, but also to fail. The expected rewards for *jumps* are small, given a high probability of failure, suggesting higher expected returns for researchers pursuing safer paths. Also Hill et al. (2025) document a ‘pivot penalty’, in which the impact of new research steeply declines the further researchers moves away from their previous work, a penalty that seems to have increased over time.

A second approach is based on identifying papers that combine knowledge from distant communities, proxied by unusual journal combinations cited together. Wang et al. (2017) look at papers making *first time ever* combinations of referenced journals, accounting for the difficulty of making such combinations. They show that *novel* papers have higher mean and variance in citation performance and also a higher probability of becoming highly cited or having no/low citations. They also find that novel research takes more time to become top-cited and is published in journals of lower Impact Factor. These findings suggest that bibliometric indicators with a short citation window—such as journal Impact Factor—may be biased against novel research. They also find that citations to novel research are more likely to come from a broader set of disciplines, suggestive that novel research has a tendency to be best appreciated and spark applications across disciplinary boundaries.

Uzzi, Mukherjee, Stringer, and Jones (2013) calculate the relative commonness of journal combinations in paper references and identify *atypicality* as the lowest 10th percentile commonness score and *conventionality* as the median commonness score. They find that top-cited papers are not maximally atypical: they conversely exhibit large conventionality with just a small touch of atypicality.

Other scholars have focused on identifying research that brings a disruption in the process of scientific accumulation, making existing knowledge obsolete and no longer used (Funk & Owen-Smith 2017; Wu et al. 2019; Park et al. 2023). They identify *disruptive* contributions (in patents or publications) are those that are cited by subsequent contributions,

while discarding citations to predecessors, whereas consolidating contributions are cited together with their predecessors. Disruption thus involves novelty that has a transformative impact.

The measures just reviewed are based on references in scientific documents—citations to papers or patents—. Other approaches have focused on the content of scientific documents, in terms of words and keywords. The rationale is that breakthroughs lead to the emergence of new concepts and theories that are not comprised within or conceived in prior knowledge, requiring the introduction of new lexicon and nomenclature, and can therefore be identified by new words or phrases beyond those previously in use (Kuhn, 1962; Schumpeter, 1942; Foster et al., 2015; Shi & Evans, 2023; Veugelers & Wang, 2019; Wang et al., 2017; Merton, 1957). When embraced by others, those new words become popular, forming new keywords and making the seminal papers highly-cited (Merton, 1957).

For example, Azoulay, Graff-Zivin and Manso (2011) and Packalen and Bhattacharya (2020) look at the content of biomedical papers, as represented by the Medical Subject Headings (MeSH), a collection of keywords curated by the National Library of Medicine. They identify the age at which the ideas were first coined, as a proxy for novelty. Azoulay, Graff-Zivin and Manso (2011) also use unusual combinations of MeSH descriptors to identify recombination of ideas.

A similar approach has been used to study research proposals, rather than the papers resulting from them, which is suitable for investigating breakthrough potential in funding competitions. Boudreau, Guinan, Lakhani, and Riedl (2016) and Feliciani, Christensen, Walsh and Franzoni (2026) extrapolate MeSH terms from proposal texts and identify as novel those that relate to MeSH terms introduced in the MeSH taxonomy only at a date subsequent to the proposal date, or to MeSH pair combinations that had not yet appeared in the previous PubMed literature.

Finally, a substantial body of research scattered across various disciplines from physics to sociology, is more recently exploring ways to distill information from high-dimensional textual data rather than citations or keywords (Kelly et al., 2021; Shi & Evans, 2023; Shibayama et al., 2021; Yin et al., 2023). Arts, Melluso and Veugelers (2025) use natural language processing techniques to analyze the titles and abstracts of all scientific publications covered in the latest version of Microsoft Academic Graph and identify new words and novel combinations of words or noun phrases that appear for the first time.

Alternatively, they measure a paper's novelty based on the similarity of its entire text to all prior papers, using SPECTER, a pre-trained document-level embedding of scientific papers (Cohan et al., 2020). To measure the impact or influence of new scientific ideas, they count the number of subsequent papers reusing new words, noun phrases, or combinations of either words or noun phrases. They validate the methods with Nobel Prize-winning papers, which likely pioneered impactful new ideas, and literature review papers, which typically consolidate existing knowledge. The results illustrate the improvement of text-based metrics over traditional citation-based measures to predict Nobel Prize winning papers. They also show that papers introducing new ideas attract more citations and that novel papers have more intellectual neighbors published after them, indicating they are ahead of their intellectual peers.¹

3. Empirical evidence on breakthrough research in science funding

Concerns that science funders may be overly cautious and avoid supporting breakthrough research with its uncertain returns have been cyclical in the US and Europe both inside and outside funding institutions (Laudel, 2017; Lipinski et al., 2009; Luukkonen, 2012; Mazzucato, 2015; NIH, 2009; NSF, 2007).

In recent years, a number of empirical studies have attempted to investigate whether breakthrough research is disadvantaged in funding, using the methods and indicators discussed supra for measuring correlates of breakthrough research. Table 1 shows a summary of these studies, with indications of the funding agency and years investigated, the measure used and the evidence found.

Several studies focused on US institutions. Packalen and Bhattacharya (2020) find that the NIH propensity to fund projects that build on the most recent ideas had progressively declined over the last several decades. Moreover, they find that NIH is more likely to fund ideas of intermediate maturity (7 to 10 years old) compared not only to consolidated ideas, but also to more recent ideas.

Azoulay et al. (2011) compare the publications of NIH investigators to those of the HHMI—an institute with a mission to support frontier research—and find the latter to be more novel and produce more variable outcomes—hits and flops, consistent with supporting

¹ They provide open access to all data and code. Data: <https://zenodo.org/records/13869486>. Code: <https://github.com/nicolamelluso/science-novelty>. Accessed September 18, 2025.

more risky trajectories, compared to NIH supported investigators. It is not clear, as they are quick to point out, whether the results depend on the criteria for selection or other factors, such as the longer duration of grants and the practice of HHMI to not require preliminary results or to expect early research results.

[Table 1 about here]

Azoulay and Greenblatt (2025) study the renewals of R01, the most common type of NIH grant. They find that grants whose first-stage outcomes reflected greater risk-taking, were significantly less likely to be renewed. The magnitude of the effect - between 4% and 9.5% lower renewal rate- appears large enough to suggest that it may not entirely depend on lack of trying to renew.

In an experiment conducted at the Harvard Medical School, Boudreau, Guinan, Lakhani, and Riedl (2016) found that more novel research proposals receive more negative evaluations during peer-review than do less novel ones. The result is driven by proposals with particularly high levels of novelty (Boudreau et al., 2016).

Similar findings hinting at a penalty for novelty were confirmed in EU-based funding institutions. Veugelers, Wang and Stephan (2025) examine whether the ERC, the most important funding agency of the European Commission, set up in 2007 with the explicit aim to fund “high gain/high risk” research, is biased against novelty. They find a significantly negative selection effect for applicants with a track record of producing highly novel publications prior to application. The bias against novelty holds also among top-cited researchers. Moreover, the penalty for novelty is larger and more significant for early career applicants than for advanced career applicants. However, early career applicants operating in top host environments escape the penalty, suggesting that panel members are more likely to tolerate novel approaches from top-scholars, seniors, and juniors working in “trusted” environments. In a difference-in-differences analysis they find no significant treatment effects of funding on inducing more novel research for advanced career recipients, only positive treatment effects for early career grantees. This positive treatment effect is, however, partly due to unsuccessful applicants cutting back on highly novel research, rather than successful applicants engaging in more highly novel research. The authors interpret this finding as a strategy on the part of the failed junior applicants to prepare for resubmission with safer topics.

Ayoubi, Pezzoni, and Visentin (2021) investigated the Swiss National Science Foundation (SSNF) SINERGIA program that seeks to promote interdisciplinary, collaborative and breakthrough research. They found that scientists inclined towards novel research are more likely to apply but less likely to be funded.

Lanoë (2019) studied the French ANR funding programs directed towards new areas and found that although individuals with a history of novel research are more likely to apply, they are not more likely to get funding.

Feliciani, Christensen, Walsh and Franzoni (2026) study the funding rate of novel proposals submitted to the Novo Nordisk Foundation (NNF), an independent private funding institution, over a ten-year period. At NNF, proposals are first scored independently by a set of reviewers, then discussed in a panel meeting to recommend winners. They find that novel and conventional proposals received similar average review scores from individual reviewers, suggesting that novelty was not promoted, but also not penalized. However, after panel meetings, novel proposals were about 1.5 and 2 percentage points less likely to be funded, compared to non-novel proposals within the same call, with the same budget and with the same average review score, corresponding to roughly a 7-9% reduction in the average funding rate of 21.4%. This result suggests that the penalty for novelty may arise especially at the final decision stage, especially in panels.

In conclusion, we have considerable evidence collected from diverse institutions across diverse time-windows and using different sets of indicators which consistently indicates that research projects that share features of breakthrough research or are proposed by investigators who were most likely to generate breakthroughs were disadvantaged in the competition for funding, compared to more conventional projects and investigators. This appears to be particularly the case when bold proposals compete directly against more conventional ones, but it is observed even in the case of the ERC or SINERGIA, whose programs should be designed to support especially high-risk high-reward science.

In the next sections we turn to the question of why funding agencies eschew supporting breakthrough research. Is this a deliberate or unconscious choice, embedded in their modes of operation? And what can be done to avoid biases of funders against breakthrough research?

4. Why is the Science System conservative?

Given the complexity of the research system and the many actors involved, multiple factors are likely to interact and play a role. In this section we provide an overview of potential explanations, following the structure adopted in Franzoni et al. (2022) and summarized in Figure 1. While these factors remain in many cases possible rather than proved explanations, we also summarize recent progress made in understanding some of them.

[Figure 1 about here]

We start with hypotheses that arise from outside the funding agencies and relate to the broader research system. We then discuss how these translate into a set of incentives and opportunities that could induce principle investigators to refrain from writing and submitting pathbreaking grant proposals, moving to panelists and policy officers to favor more conventional research and finally end with discussing funding agencies setting up programs and evaluation procedures that disadvantage pathbreaking proposals.

4.1 Science system

In recent decades the science system has arguably become less tolerant of failure, and more short-term oriented, a combination that could be particularly detrimental for breakthrough research. First, there has been a generalized push for greater accountability in the use of public funds. Many national governments have set up regular evaluations of universities that put increasing pressure on publicly funded institutions to demonstrate results, especially those aligned with political cycles, favoring shorter-term and targeted outcomes. More targeted research programs may however backfire and push the science system towards an emphasis on results within increasingly narrow time windows. This is evident in the heated discussions surrounding the share of overall R&D budgets allocated to more targeted, top-down agendas and applied research programs, compared to bottom-up untargeted research programs of ERC, NSF, and NIH. But even within untargeted research programs, the pressure to show results quickly may discourage publicly funded agencies from funding breakthrough research. A major factor is impatience. Breakthroughs take a long time to materialize (Wang et al., 2017). Funding agencies may feel that they cannot afford to wait long to show impact and opt instead to fund safer projects that give tangible results in the near term, even if these are less likely to generate breakthroughs in the long term, a type of science myopia.

Second, pressure for accountability is particularly impactful when it is combined with the widespread use of bibliometric indicators. Commonly-used metrics, like the journal Impact Factor or short windows for calculating citations to measure research “quality” can discourage breakthrough research, as these measures appear to be biased against novel research (Stephan et al., 2017).

Third, tenure positions, which in academia mark the passage from a period of short-term performance scrutiny to a period of job security, have become less common over time, are reached at a later age, particularly in the biomedical fields, and are increasingly subjected to mandatory periodic re-examination that can lead to the dismissal of underperforming professors (Clement, 2022). Although the tenure system was primarily intended as a way to protect academics from ideological or political influence—rather than from the pressure of scientific competition, empirical research has shown that, after tenure, scholars engage more in projects that depart from pre-existing research trajectories and produce more novel works, likely because tenure allows researchers to afford the worst consequences of a project failure (Franzoni & Rossi-Lamastra, 2017; Tripodi et al., 2025). Consequently, fewer, later and weaker tenures can also lead to less engagement in breakthrough research. A similar problem concerns the large use in the US of “soft money” positions, i.e. positions where salary is funded from grants that the researcher is responsible for obtaining, making the university free to cut its losses and hire another individual into the position when the grant expires or is not renewed (Stephan, 2007). In periods of large budget cuts and/or uncertainty about future budgets, like the one currently unfolding, breakthrough research may be terribly risky for scholars on soft money.

4.2 Principal investigators

A system of incentives based on short-time outcomes and reliant on biased metrics inevitably may constrain the supply of breakthrough research proposals from Principal Investigators. Consistent with this, Veugelers, Wang and Stephan (2025) find that non-funded junior ERC applicants who fail in the second review stage but still have the option to reapply, have significantly lower likelihood of producing novel papers after being rejected, compared to the successful ones, suggesting that rejected applicants learn that novel research is not rewarded. Faced with the pressures to (re-) apply for funding, they adjust their research portfolio away from novel research, something which the successful applicants, who secured long-term funding, are “freed” from doing.

The findings from Foster et al. (2015) and Hill et al. (2025) reported supra, also suggest that returns may be higher for following a safer research path. Stephan (2019) has called this the “*Quad effect*”, referring to the fact that competitive female figure skaters attempt fewer quadruple jumps, arguably because the incremental score they can earn for completing a quad, compared to successfully completing a triple jump, is insufficient to compensate for the risk of failing to complete the quad jump.

The preferences of scientists for the level of novelty involved in the projects they wish to pursue may not only reflect biases against novelty in the reward structure of science, but also aversion to failure on the part of scientists. This may be in line with the general human tendency to over-estimate the magnitude of prospective losses and under-estimate the magnitude of prospective gains (Kahneman & Tversky, 1979; Tversky & Kahneman, 1991), but it may also depend on long-term attitudes developed in research training. A recent article from Shibayama, Mattsson and Broström (2025) shows that scientists have persistent attitudes for taking or not taking risks in research and that PIs pass-down their risk-attitude to the PhD students that they supervise or mentor. These preferences eventually persist for 10 years after the end of training and endure change of affiliations and topics. They also find that PhD students that work in projects funded by grants are less likely to take risks. Overall, these results underscore the importance of a risk-taking culture in the science system in general, and points at a role for mentors and PhD schools more broadly.

Short supply of innovative approaches may also reflect the limited diversity of the scientific workforce, which—despite progress over time—remains predominantly underrepresented in minority groups in most of the STEMs, especially in senior and tenured positions (Ceci & Williams, 2011; Hoppe et al., 2019). Hofstra and colleagues (2020), analyzing more than thirty years of U.S. doctoral dissertations, found that research conducted by scholars from minority groups—defined as underrepresented genders and ethnicities within their discipline—tended to be more innovative than that of majority groups. Yet, these novel approaches were taken-up at lower rates by other scholars, compared to those from majority groups and led to less successful careers. Thus, limited diversity in the research community may be listed as one additional factor hindering the supply of breakthrough research.

4.3 Research agencies

The design of evaluation and selection procedures in research agencies is a critical step, with direct repercussions on what is funded and indirect repercussions on what is seen as fundable and therefore worth pursuing. Several elements of the selection process can in turn favor or disfavor breakthrough research.

To begin with, regular and breakthrough research are normally competing in the same programs, with the result that breakthrough research is disadvantaged due to low probability of success, early-stage ideas, lack of preliminary data and penalty for interdisciplinarity. Only a minority of research agencies operate special programs targeting high-risk/ high-gain proposals, where applicants and evaluators are explicitly instructed to prioritize bold projects. Yet, such initiatives remain rare and account for only a small fraction of total research budgets. For example, the NIH has created four programs with these aims, designed for rapid approval and without requirements for preliminary data, though one of them has recently been put on hold. Even so, the programs are modest in scope: representing only about 0.15% of the NIH extramural budget, with success rates of roughly 5% (Stephan & Franzoni, 2023). Even the ERC, although initially explicitly focusing on high-gain/high-risk research, has now changed its mission statement to supporting excellent research more generally.²

Research agencies also exert direct control on the procedures and practices used to evaluate proposals. These involve decisions such as the disciplinary composition of panels, the protocols defining evaluation criteria, the choice and number of evaluators, and the methods used to aggregate their opinions. While these may appear to be neutral procedural technicalities, they can have subtle unintended repercussions on the evaluation of breakthrough research and its chances to get funded, as explained in more detail below

Review panels are often designed by funding agencies to be discipline-based.³ As noted supra, papers of high novelty are often interdisciplinary, are more likely to garner citations from outside, not from within their own field, suggesting that the research is appreciated more by others than by disciplinary colleagues (Wang et al. 2017). Monodisciplinary panels may thus be biased against novel research whose benefits reach outside their own discipline. Consistent with this view, Bromham et al. (2016) find that

³ This, for example, is generally the case at NSF and ERC. The latter operates with 25 panels which are mostly discipline-focused.

proposals with higher interdisciplinarity submitted to the Australian Research Council Discovery Program, had lower probability of being funded. Wang, Lee and Walsh (2018) find that creative, combinatorial interdisciplinary research was more commonly funded with internal block-funding, as opposed to external, competitive funding in Japan. Banal-Estanol and colleagues (2019) show that proposals from interdisciplinary teams submitted to the UK's Engineering and Physical Sciences Research Council—where evaluation also relies on disciplinary panels—were less likely to be funded, although when funded, they tended to be more successful.

Franzoni and Stephan (2023) stress that the evaluation protocols used to elicit experts' opinions, although apparently neutral, may in fact unintentionally penalize riskier projects, because they elicit project impact with a single score that conflates value and probability. Suppose, for example, a reviewer believes that a project could deliver a breakthrough but with a very low chance of success. If the protocol asks to assess the project impact giving only a single score on a scale -say from 1 to 6-, the reviewer will likely give a low score, given the low probability of the breakthrough. This score would make the project indistinguishable from another project believed to have a low impact with a high probability. Even though the reviewer has recognized the possibility of an exceptional outcome in the first project and not in the second, the protocol would not allow the possibility to express this insight. This underpins the relevance of scales and scores that allow to represent the possibility of breakthroughs and take this into account in funding decisions (Franzoni & Stephan, 2023).

Evaluations of proposals are done by collecting several expert opinions, which are then combined to produce a decision. In practice, however, decision mechanisms may not be neutral with respect to breakthrough research. Proposals involving radically novel approaches are generally more controversial and allow ample room to challenge their validity, as they are often not grounded in proved assumptions or preliminary findings. They may simply be more likely to spark disagreement, given the larger uncertainty involved. Major funding agencies, like NIH and ERC, convene *panel meetings* to forge communal decisions. In practice, however, such decision rule may disadvantage novel projects (Feliciani et al. 2026). One problem is that the need to take communal decisions may favor proposals that are uncontroversial and are thus more palatable to the majority of panelists (Lamont, 2009). Consistent with this, the work by Feliciani et al. (2026) suggests that giving

each panelist, once per funding round, a golden ticket to grant funding irrespective of others' opinions, breaking the communal decision rule, may promote more novel research.

Finally, panels are generally organized to review proposals on a “one by one” basis, expressing the merit of each proposal separately, without considering an overall desirable level of funding for bolder research.⁴ This “one by one” approach leaves no room for hedging risk, which would be possible by taking a portfolio approach.

4.4 Panelists and research officers

The workload of agencies and evaluators is typically substantial. For example, the NIH evaluates approximately 80,000 applications annually, engaging over 2,000 reviewers per year and has more than 150 standing Study Sections (Franzoni et al., 2022). The ERC averages about 15 members on each of its 25 separate panels. The average panel member for the Starting Grants looks at more than 100 proposals per call. Given the heavy workload, it is not surprising that reviewers and panel members may seek ways to rapidly screen proposals, especially on a first pass. One of the easiest ways to do so is to focus on the publishing record of the scientist proposing the research, by examining readily available bibliometric indicators on platforms such as Google Scholar and Scopus. Such use of bibliometrics may harm breakthrough research. The work by Wang, Veugelers and Stephan (2017) suggests that novel research is systematically less likely to be published in journals with high Impact Factor and takes longer to receive citations, becoming a top hit than does non-novel research. This bibliometric penalty can lead panels to select against individuals with a history of novel (risky) research, especially when the applicant is young and has a short history of citations. More generally, a focus on bibliometrics shifts the basis of decisions away from the substance of the proposal to an easily accessible metric.

Peer review evaluations may also disadvantage breakthrough research in more subtle ways. When panelists fear that modest results could undermine future support for a program, they may focus excessively on “what can go wrong” and favor proposals that appear feasible or present strong “preliminary findings”, offering reassurance that the research will not come up “empty-handed.” In effect, this creates an implicit requirement that research be de-risked before it is funded. Consistent with this view, empirical analyses that disentangle reviewers' scores across sub-criteria suggest that reviewers place a heavy emphasis on feasibility (Lane

⁴ At some agencies, such as NSF, program officers have some leeway in making decisions, but this is not common.

et al., 2022). Specifically, sub-criteria assessing the strength of a proposal's approach and methodology are weighted roughly twice as heavily as those assessing its potential impact on science and society (Franzoni et al., forthcoming). Because breakthrough research is typically early-stage, but stronger in potential impact, this weighting can systematically disadvantage novel and exploratory proposals, consistent with findings from Boudreau et al. (2016).

Concerns exist that policy officers too may skimp risks. For example, Wagner and Alexander (2013) evaluate the SGER NSF program designed to support high risk, high reward research that ran from 1990 to 2006. Funding decisions of SGER were made entirely by program directors with no external review. Skipping external reviews, the program was found to be highly successful in producing transformative science. Nevertheless, even in this program, the directors routinely used but a small percent of available funds, suggesting that they were reluctant to take risks, despite the agency mandate and the freedom to do so.

5. Promoting Risk Tolerance: Emerging Approaches

The discussion in the prior section has outlined a number of possible root causes that may contribute to hinder breakthrough research. This section follows up with a set of possible remedies and related evidence regarding their efficacy, when available.

Failure-tolerant research system

Breakthrough research requires environments that tolerate failure. One way to foster tolerance to failure is by reducing the pressure for showing short-term outcomes and instead emphasizing progress toward broader long-term goals. An interesting benchmark in this respect is provided by entrepreneurial communities, where failure is not stigmatized, and narratives highlighting lessons learnt from failure are actively encouraged. PhD programs and initiatives for early-career scholars could play an important role in cultivating a similar culture (Shibayama et al., 2025). Likewise, the availability of “Truly Legendary Freedom” — type funding that would provide researchers with the protective space to explore their breakthrough ideas (Whitley, 2014), but which seems to be disappearing, could foster a culture of risk-taking.

Journal policies that permit the publication of registered reports, including those that do not produce the expected results, are another step in this direction. In recent years, such policies have been promoted by several communities, including the Center for Open Science⁵ and over 300 journals have adhered to the policy, including the flagship journal *Nature*.

Deemphasize bibliometrics

Bibliometric indicators, as we discussed, are particularly problematic for novel and pathbreaking research. Although still widely used, there is increasing awareness of their limitations. Universities and funding agencies can reinforce this trend, by embracing the principles of responsible research assessments, as articulated in the DORA Declaration⁶ and Leiden Manifesto.⁷

Some agencies are making progress in this direction. One example is the curricula template introduced in 2022 by the Swiss National Science Foundation, which requires “short narratives in combination with a limited number of research results, rather than an extensive publication lists”.⁸ Although we do not yet have evidence about their effectiveness, there is considerable hope that such practices would alleviate the bias against pathbreaking research.

Dedicated funding programs for breakthrough research

Programs dedicated to breakthrough research can be an effective way to increase the appetite for such research. But even when these programs are operative, there is a need to carefully select, instruct and direct panelists to ensure they factually prioritize breakthrough research, rather than perpetrating the usual excellence selection. The experience of the ERC suggests that this cannot be taken for granted (Veugelers et al., 2025). Moreover, it is possible that these programs are particularly effective if they are part of a well-balanced portfolio that has separate funding lines for both regular and risky research, so that proposals with different breakthrough potential compete in separate leagues with dedicated decision mechanisms. When funding is consolidated in a common program, like at the ERC, the competition among high-quality but less innovative projects may simply be too strong (Veugelers et al., 2025).

⁵ <https://www.cos.io/initiatives/registered-reports>. Accessed September 17, 2025.

⁶ <https://sfdora.org/>. Accessed September 17, 2025.

⁷ <https://www.leidenmanifesto.org/>. Accessed September 17, 2025.

⁸ <https://www.snf.ch/en/wBR6E3emu8PP1ZSY/news/a-new-cv>. Accessed September 17, 2025.

Allow for disagreement

We stressed that breakthrough research may raise more criticism and polarized views in evaluations. This warns against decision rules that require or emphasize consensus. Several funding agencies are currently experimenting with mechanisms that relax the requirement of consensus in various ways (Feliciani et al., 2026). One of these is the use of “golden tickets” that allow each panelist to promote one application per call, even without consensus. Another approach, implemented by the SNSF, the NNF, the British Academy and others is to use partial lotteries, where the panel can use a random draw to select among applications that pass the quality bar or in case of ties. Although the impact of this policy is still under testing, simulations suggest that golden tickets and, in some cases, partial lotteries, may help avoid penalties against breakthrough research (Feliciani et al., 2026).

Portfolio approach and staging

The ‘one by one’ approach typically used in panels prevents portfolio considerations. At a minimum, panels need to think about diversifying the risk profile of the proposals they are funding. More generally, a portfolio approach to address risk aversion could require panels to put in different baskets highly risky and moderately risky proposals and provide a way to choose proposals from each. In practice such a portfolio approach could be quite challenging to implement, posing issues of fairness, as some proposals may have to be eliminated to balance or de-risk the portfolio, even if they have merit. Moreover, portfolio theory requires that the research paths be sufficiently uncorrelated and correlation between research paths, in and of itself, can be hard to determine, particularly for research covering vastly different goals across different fields and with different research approaches. Such conditions may be more suited for mission-oriented research, where funding in parallel of competing strategies is used to overcome major technological bottlenecks or achieve breakthrough milestones. This strategy has long been adopted by DARPA. In recent years similar approaches in mission-oriented programs have been enlarged in the US, with the creation of E-ARPA, and started in the EU, with the creation of the ARIA⁹ in the UK and the SPRIN-D¹⁰ in Germany.

Similar considerations apply for the approach that funds in stages, common at DARPA and in the Venture Capital industry, where increasingly larger amounts of funding

⁹ <https://www.aria.org.uk/>. Accessed September 17, 2025.

¹⁰ <https://www.sprind.org/en>. Accessed September 17, 2025.

are allocated, depending on whether interim milestones are being met. Can such a staging approach also be used by science funding agencies, allowing them to take more risk? Interim evaluation is especially useful for projects that can start small, make quick tests and/or have clear interim milestones to be met (Ewens et al., 2018; Vilkkumaa et al., 2015). These conditions are more the exception than the norm in the natural sciences, where the share of research that requires expensive equipment and substantial effort is large (Stephan, 2012). This is probably one reason why we have not seen much progress in this direction.

6. Conclusions: What have we learnt and what remains unknown

Society needs scientific breakthroughs to tackle the many challenges we face. Because many of the paths to such breakthroughs require embracing unconventional approaches with high chances of failure, its science system, and particularly its public science funding system, needs to ensure that breakthrough research is encouraged or, at a minimum, that the system is not biased against research that has a good probability of generating breakthroughs. The evidence discussed *supra* suggests that the kind of research that explores new and uncertain approaches, combines previously distant knowledge, and generates new ideas, often crossing disciplinary boundaries, which we called *breakthrough research*, tends to be disadvantaged in funding competitions. Additional reflections on the broader scientific environment have made clear that we have serious concerns on the suitability of our current science system to deliver breakthroughs and have outlined possible ways forward.

It is important to note that we are not looking for risky or novel research per se, but for the antecedents of breakthroughs. There are a lot of proposals that are risky or novel without having any significant potential for breakthrough outcomes. It is therefore an empirical question whether or not having a process more open to risky or novel proposals would increase the extent to which proposals with breakthrough potential get funded.

The promotion of breakthrough research needs to be addressed within the entire science system. It cannot be solved by an individual program or funding agency and rather requires a holistic perspective on the science enterprise, activating not only funders, officers and reviewers, but also universities and research centers, PhD schools and mentors, journal editors and their reviewers and, last but not least, researchers themselves.

Nevertheless, within the science system, funding agencies play a pivotal role, given the relevance of funding in shaping the incentives of the research system. We discussed a number of interventions that can be adopted by funding agencies that wish to remove barriers against novel and risky research and pave the way for breakthrough research. It is important to stress that alleviating the biases against novel and risky proposals would increase the likelihood of breakthroughs, but cannot guarantee breakthroughs. Breakthrough ideas are hard to come up with and their realization would remain to a large extent a random process. Thus, programs designed to foster breakthroughs should also expect a physiologically-high failure rate, raising the question of whether the system is prepared to accept these risks.

We conclude by sketching a research agenda for advancing our understanding of breakthroughs, their research trajectories, their funding and their treatment in the science system more broadly.

The empirical analyses presented supra are based on measures of antecedents of breakthrough research, such as novel and risky science. Of course, proposals may be risky or novel without generating breakthroughs. There is a need of improving our understanding of breakthroughs and their relationship with novelty, risk and other antecedents. Future studies that investigate both conceptually and empirically such relationship are critical for improving our measurement and evidence-based analysis of the science system and informing sound policy intervention.

More empirical studies are needed to assess the potential biases of funding programs against breakthrough research. Studies with new and alternative metrics based on large language models and big databases are particularly welcome. Likewise, we need more large-scale empirical analyses exploring whether having or not having a process more open to risky or novel proposals would effectively increase the likelihood of breakthroughs.

Rigorous impact analyses of interventions and those analyses conducted in experimentally-controlled environments are also extremely important to understand causal mechanisms and support evidence-based policies. Several interventions have recently been adopted or announced to favor breakthrough research and we are eager to see their impact. These intervention address various mechanisms, working at the level of individual researchers, such as narrative CVs, and long-term investigator grants or at the level of panels, such as golden tickets and lotteries. All these warrant systematic evaluation to determine their effects on path breaking behavior and research outcomes. PhD programs are another potential

area of investigation, with a specific focus on provisions that may affect—for good or bad—the exploratory culture and risk-taking attitude in younger generations of researchers.

Future academic research on this topic should also do more in-depth case studies of past breakthrough research processes. Of particular interest would be case studies of failed or missed breakthroughs, scientific challenges that remained unsolved and eventually fell off the radar or analyses of the problems and setbacks encountered along the path.

The scientific environment is undergoing a period of rapid transformation, including a growing emphasis on national priorities and mission-oriented research, combined with a tightening of public budgets. An open question for the future remains whether trends towards reducing public funding for research, and directing more public funding towards specific research areas or missions will particularly affect breakthrough research. We need to know more about how mission-oriented research programs, often inspired by and modeled on the DARPA funding approach—translate to basic science funding and their broader outcomes. Of specific interest are the way in which program officers in mission-oriented agencies or programs select and manage projects, how do they weigh methodological soundness and potential impact, if they make technology portfolio considerations and how, or in which way do they evaluate progress made and take termination decisions.

Similarly, we need to know more about the impact of the retrenchment in global talent mobility and how this plays out specifically for breakthrough science. Finally, we need to understand whether the use of generative AI in scientific research will be neutral, positive or negative for breakthrough science.

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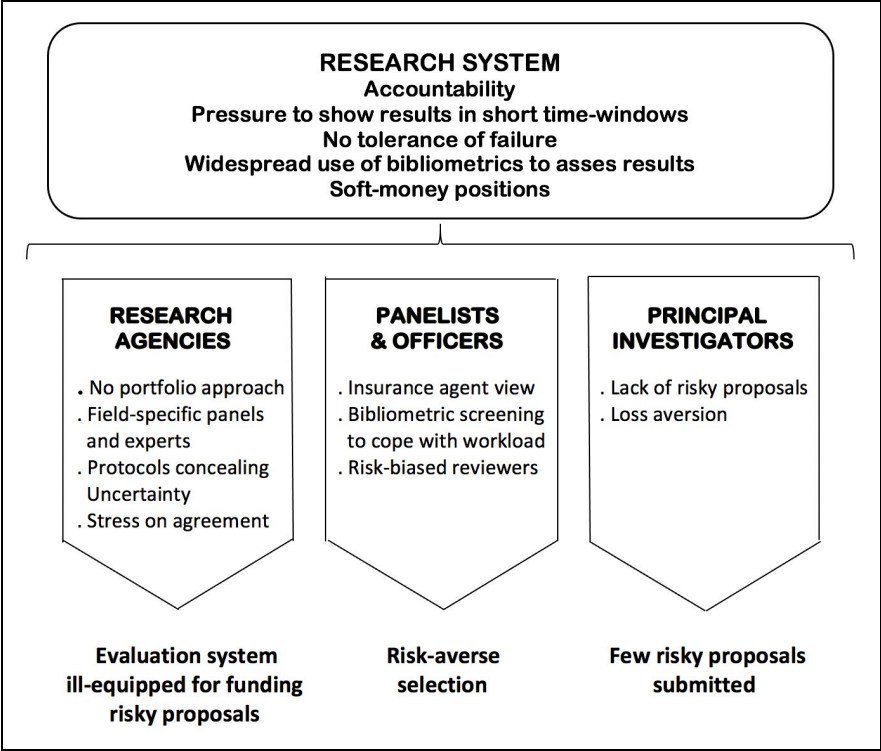
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Table 1. Summary of empirical studies.

Agency (year)	Measure	Evidence	Reference
NIH, HHMI (1970 – 2005)	Investigators' MeSH and MeSH pairs vintage, MeSH overlap before/after; hits & flops.	HHMI investigators work in more recent areas and broaden more their research agenda after funding than NIH investigators.	Azoulay et al., 2011
NIH (1970-2006)	New MeSH in publications acknowledging funding	NIH funding research of mid-maturity (7-10 y.o.), especially in recent decades	Packalen & Bhattacharya (2020)
NIH, R01 (1980-2015)	Extreme tail, highly-disruptive, pivoting or standing-out publications from prior grant	Greater risk-taking R01 less likely to be renewed	Azoulay and Greenblatt, 2025
ANR (2005-2009)	Investigator's publication originality in last 3 years, based keywords combination	Investigators with more recent original publications were less likely to get funded	Lanoë, 2019
ERC (2007-2013)	PI with history of novelty, based on papers citing a novel journal combination	PIs with history of novelty less funded. Bias affecting all, but stronger for younger applicants not based at top institutions.	Veugelers, Wang, Stephan, 2025
SNSF - SINERGIA (2008-2012)	PIs with at least one publication above a minimum threshold of success citing unusual combinations of journals in last 3 years	PIs more likely to apply, but less likely to be funded	Ayoubi, Pezzoni, Visentin, 2021
Harvard Medical School (2011)	New MeSH pairs in applications	More novel applications given lower scores	Boudreau et al., 2016
Novo Nordisk Foundation (2012-2022)	New MeSH, new phrases, recombination, new MeSH pairs	Novel proposals 1.5-2% less likely to be funded compared to non-novel proposal in the same call, with the same budget and with the same pre-panel average score.	Feliciani, Christensen, Walsh and Franzoni, 2025

Figure 1. Incentives and opportunities regarding risky research: A summary.



Source: Franzoni et al. 2022

