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Process and Impact Evaluation of FWF's 1000 Ideas program

Final report

Anete Vingre, Diogo Machado, Florentine Frantz, Rima Martin, Laura Sutinen, Erik Arnold

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Table of Contents

Tables

Figures

Executive summary

The program and evaluation

The Austrian Science Fund's (FWF) 1000 Ideas program was created to support radically novel, high-risk, high-reward (HRHR) basic research that is unlikely to be funded through conventional peer-reviewed schemes. It offers small, time-limited grants to explore bold ideas at an early stage, using double-blind assessment and partial randomization (lottery) to reduce bias and increase tolerance for unconventional proposals.

This evaluation was commissioned by FWF and conducted by Technopolis between September 2025 and January 2026. It uses a mixed-methods approach combining surveys of applicants and awardees, qualitative interviews with researchers and decision-makers, program data, and econometric analysis of bibliometric outcomes. The program is benchmarked against FWF's Principal Investigator (PI) program, which serves as the mainstream model for competitive project funding.

The evaluation addresses six core questions: originality and risk, seed-funding effectiveness, differences in outputs, failure, the suitability of the selection process, and whether a stand-alone program is needed.

Overall findings

The evidence shows that 1000 Ideas is working as intended. Funded projects are riskier, more exploratory and more novel than those typically supported by mainstream funding. Survey and interview data show that awardees overwhelmingly see their projects as unconventional, speculative and uncertain, often challenging established assumptions or combining disciplines in unusual ways. At the same time, quantitative evidence shows that funded projects have more dispersed outcomes than PI program projects.

The program produces qualitatively different outputs. Compared to PI program projects, 1000 Ideas projects are less focused on immediate publications and more on new methods, new concepts, new combinations of fields, and exploratory learning. This is confirmed by both interview evidence and bibliometric indicators of novelty and interdisciplinarity. Econometric analysis shows that 1000 Ideas funding causally increases the novelty of research, with funded projects showing a statistically significant post-award rise in novel interdisciplinarity compared to unfunded applicants.

The program functions as genuine seed funding. Most of the ideas funded would not have been pursued without 1000 Ideas, and a substantial proportion of projects have already generated, or are expected to generate, follow-on funding from larger national and European schemes. In monetary terms, follow-on funding exceeds the original grant volumes and program budget, indicating strong leverage effects. Across all calls to date, the total FWF investment in the 1000 Ideas program amounts to €16.32 million. Against this, ResearchFish data reports €47.5 million in secured follow-on funding attributable to 1000 Ideas projects. This implies a leverage ratio of roughly 1:3 – for every euro invested by FWF, at least three euros of additional competitive research funding have already been mobilized.

Failure as a feature

A defining feature of HRHR funding is that some projects will not succeed in the conventional sense. In 1000 Ideas, around one-fifth of awardees report "failed" in the sense that their original approach did not work as planned. However, these failures are widely reframed as learning and discovery: new methods, unexpected insights, or redirection to more promising paths. Nearly two thirds of awardees report that their projects produced valuable scientific insights even when initial hypotheses were not supported.

Program processes: anonymization and partial randomization

The 1000 Ideas program uses two unusual assessment tools:

- Double-blind review (anonymization)
- Partial randomization (lottery) among proposals above a quality threshold

The evaluation finds that these mechanisms do not fundamentally change how applicants behave, but reshape who gets funded and what kinds of ideas are selected.

Anonymization in the 1000 Ideas program is associated with greater perceived fairness, especially among early-career researchers, and with improved access for unconventional ideas that might otherwise be disadvantaged by reputation or institutional status.

Partial randomization is more contested among the 1000 Ideas applicants, but within the program context, it is broadly accepted as a legitimate way to allocate funding when proposals are difficult to distinguish on conventional quality criteria. Econometric evidence shows that under the lottery, novelty-oriented characteristics (such as unconventional interdisciplinarity and disruption) matter more, while past citation performance matters less.

Together, these funding process elements function as structural enablers of high-risk research, increasing tolerance for uncertainty and reducing conservative bias in selection.

Is a stand-alone program necessary?

The evaluation finds that a stand-alone 1000 Ideas program is both necessary and justified.

Its distinctive design, small grants, short duration, absence of PhD funding, anonymized assessment, tolerance of failure, and partial randomization, create a protected space for scientific risk-taking. These safeguards allow FWF to support unproven ideas without exposing large budgets or researchers' careers to excessive risk.

If 1000 Ideas were integrated into mainstream programs, these protective features would be difficult to maintain. Larger budgets, longer timeframes, and stronger feasibility expectations would reintroduce conservative selection dynamics, undermining the program's HRHR logic.

Key recommendations

The evaluation supports continuing and strengthening 1000 Ideas as a stand-alone program, with targeted refinements rather than major redesign. Key recommendations are:

- Maintain the stand-alone HRHR seed-funding program to preserve its protected risk space

- Improve communication about how anonymization and randomization work, particularly for unfunded applicants, to strengthen transparency and legitimacy
- To strengthen anonymization while keeping the review process workable, FWF could introduce a more clearly separated, professionally supported pre-screening stage that is explicitly identity-blind. Although anonymization is a core and well-accepted feature of 1000 Ideas, its effectiveness is partly weakened by the current involvement of Austrian Board members in the first filtering step. International practice shows that eligibility and plausibility checks are typically handled by funder staff or international panels rather than by nationally embedded reviewers.

1 Introduction

This report presents the findings of the process and impact evaluation of the Austrian Science Fund's (FWF's) 1000 Ideas program. This study was commissioned by the FWF and carried out by Technopolis between September 2025 and January 2026.

1.1 1000 Ideas program and evaluation objectives

The 1000 Ideas program supports radically novel and unconventional basic research across all scientific disciplines, with the explicit aim of supporting bold, early-stage ideas that could shift scientific paradigms or open new research domains. As a deliberately high-risk/high-reward (HRHR) funding instrument, the program is designed to support exploratory research trajectories characterized by high uncertainty, limited preliminary evidence, and a high probability of failure. These features pose specific challenges for evaluation, as many intended outcomes are long-term, non-linear, and may not be adequately captured through conventional performance indicators.

Standard bibliometric indicators such as publication counts, citation impact, or short-term technological outputs tend to privilege established research trajectories and incremental advances, and are therefore poorly suited to assessing early-stage, interdisciplinary, or paradigm-challenging research. The evaluation, therefore, adopts a mixed-methods approach that combines survey evidence, qualitative interviews, and advanced bibliometric and econometric analysis. Rather than focusing solely on average output performance, the evaluation explicitly considers alternative indicators of novelty, risk, and exploratory capacity, as well as outcome variability and learning effects. This methodological design reflects the program's underlying logic, which treats failure and uncertainty not as shortcomings but as integral components of high-risk research.

The evaluation examines the effectiveness of the 1000 Ideas program in terms of its design, implementation, and emerging outcomes, and assesses the extent to which funded projects contribute to achieving the program's objectives. It also pays particular attention to the program's distinctive assessment features - anonymization and partial randomization - which were introduced to mitigate peer review biases and enable fairer consideration of unconventional proposals.

This evaluation assesses the 1000 Ideas program against its objectives, design logic, and intended role within the FWF funding portfolio. In line with the program's experimental and high-risk/high-reward (HRHR) character, the evaluation focuses not only on outputs and outcomes, but also on additionality, selection mechanisms, and the suitability of innovative assessment procedures. The following evaluation questions guided the analysis:

- **EQ1.** To what extent have particularly original or high-risk research ideas been implemented in the 1000 Ideas program to date, and have funded projects led directly to groundbreaking findings or laid the groundwork for such findings?
- **EQ2.** Does the program meet the requirements to be classified as seed funding for high-risk research, i.e.
  (i) are funded projects unlikely to be supported under conventional funding programs, and
  (ii) are the results achieved likely to lead to follow-on funding or further research?
- **EQ3.** Are there qualitative differences between the outputs and research trajectories generated under the 1000 Ideas program and those in FWF's Principal Investigator (PI) program?

- **EQ4.** To what extent have 1000 Ideas projects encountered failure, how have such failures been dealt with, and can failure be interpreted as an indicator of alignment with the program's high-risk/high-reward logic?
- **EQ5.** Is the FWF's selection procedure for the program - including anonymized applications and partial randomization - suitable for identifying radically new, particularly original, or high-risk research ideas? To what extent could the procedure be improved?
- **EQ6.** Is a separate program necessary to achieve the stated objectives or could they be integrated into another existing program? What adaptations might be necessary?

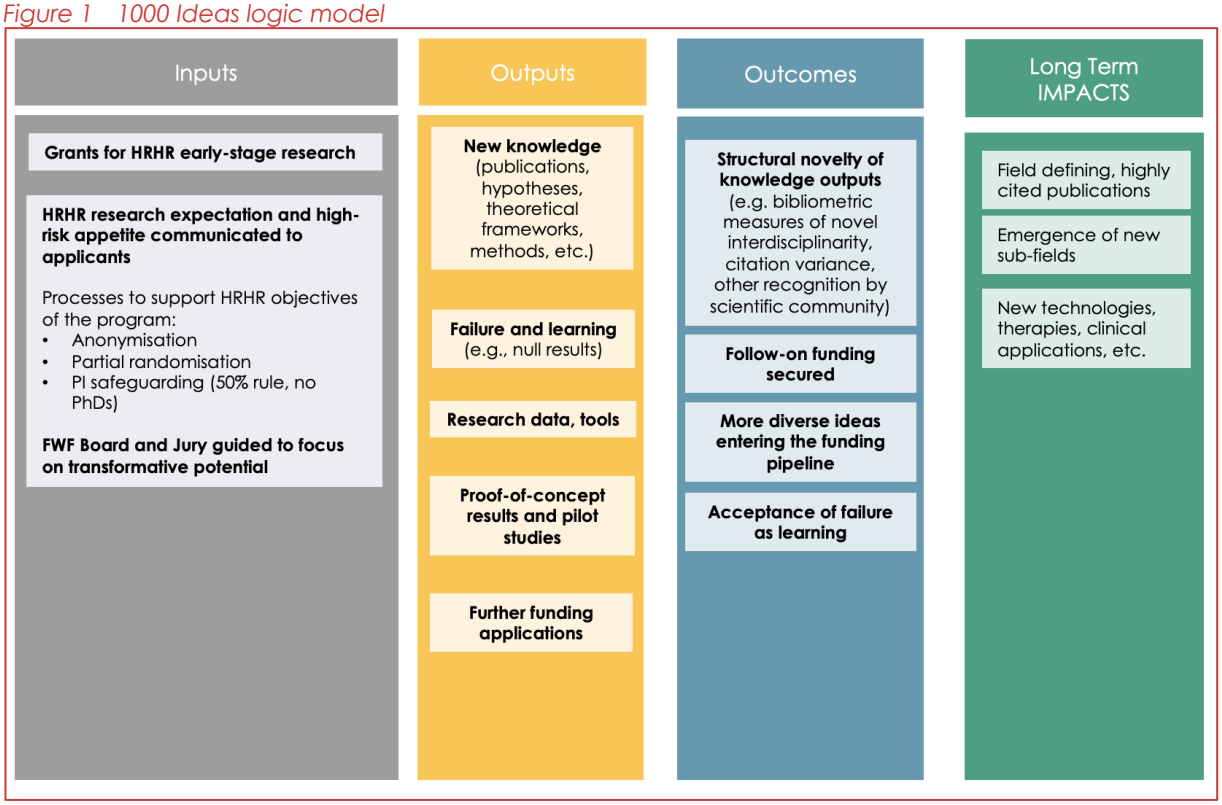
The following method note introduces methods, and Section 2 provides a detailed overview of the program's objectives, funding logic, and selection processes.

1.2 Method note

1.2.1 Logic model

Based on the program objectives, design, program documents and insights from FWF, Figure 1 shows a simple high-level program logic model. The model summarizes how program objectives, design elements and inputs are expected to generate outputs, outcomes, and longer-term impacts, and provides a common reference frame for the analysis.

Figure 1 1000 Ideas logic model



Inputs. The inputs reflect both financial resources and the program design choices that define the 1000 Ideas program as a high-risk/high-reward instrument. The program design sets expectations and conditions under which risky ideas are proposed and selected. The inputs include small, time-limited grants for early-stage HRHR research, explicit communication of a high tolerance for failure, and a set of safeguarding and selection mechanisms (anonymization, partial randomization, and PI employment requirements). Together, these

inputs are designed to lower the personal and institutional risks of pursuing unconventional ideas while simultaneously increasing the program's willingness to accept scientific uncertainty.

Outputs. Outputs are the immediate products of funded projects, including both successes and failures. In a high-risk program, outputs are not limited to publications or data. They also include null results, abandoned approaches, and proof-of-concept attempts, all of which represent learning about what does and does not work. The model, therefore, treats failure and learning as outputs, alongside new knowledge, hypotheses, methods, datasets, and pilot results. These outputs form the knowledge base from which more mature research trajectories can later emerge.

Outcomes. Outcomes describe how the characteristics of research change as a result of the program. Rather than expecting immediate breakthroughs, the 1000 Ideas program aims to alter how knowledge is produced. This includes increased structural novelty (e.g. novel interdisciplinary combinations, unusual citation patterns, and greater variance in outcomes), greater willingness to explore uncertain paths, and strong positioning for follow-on funding. Outcomes also occur at the system level - more diverse ideas enter the funding pipeline when the PIs seek follow-on funding, and failure becomes more acceptable as part of legitimate scientific work. The logic model makes clear that short-term bibliometric impact is not the primary success criterion; instead, the program is intended to seed transformations that unfold over time.

Long-term impacts. Long-term impacts are the scientific and societal returns of high-risk research. These might include the emergence of new fields, paradigm-shifting publications, breakthrough technologies, new clinical or policy applications, etc. Because the program funds very early-stage ideas, most of these impacts will materialize only over longer time horizons and likely through the inputs of follow-on funding.

1.2.2 Evaluation approach and methods

The evaluation adopts a **mixed-methods design** tailored to the specific challenges of assessing high-risk, exploratory research. Given that, according to the program's intervention logic, HRHR funding is expected to produce exploratory outputs and trajectory shifts before conventional impacts materialize, the evaluation deliberately combines multiple sources of evidence to capture both early signals of impact and structural changes in research trajectories. Quantitative and qualitative methods are used to address different aspects of the evaluation questions, and findings are interpreted through triangulation rather than reliance on any single data source. High-risk/high-reward research does not aim for incremental productivity but for change in knowledge production, shifting boundaries and creating new research trajectories. This evaluation, therefore, emphasizes structural novelty, interdisciplinarity, and citation variance rather than short-term citation impact.

The evaluation is guided by a logic model (see section 1.2.1), which distinguishes between program inputs, early-stage outputs (such as exploratory results and learning from failure), intermediate outcomes (such as structural novelty, follow-on funding, and shifts in research trajectories), and longer-term scientific and societal impacts. This logic provides the reference frame for interpreting evidence across all method streams.

Survey data from both awardees and unfunded applicants provide insight into applicant motivations, perceived novelty and risk, experiences with failure among awardees, behavioral additionality, and perceptions of process quality (addressing EQ1, EQ2, EQ4, EQ5, and EQ6). These self-reported perspectives are complemented by qualitative interviews with PIs, FWF staff, FWF Board members and international Jury, which provide a deeper contextual

understanding of program objectives, selection dynamics, risk management considerations, and the role of assessment innovations (particularly relevant for EQ4, EQ5, and EQ7).

At the outcome level, the evaluation draws on bibliometric and econometric analyses to assess whether 1000 Ideas funding is associated with systematically different research trajectories compared to relevant counterfactuals. Difference-in-Differences (DiD) and Difference-in-Difference-in-Differences (DDD) models compare funded and unfunded applicants, as well as successful 1000 Ideas projects and successful PI projects, controlling for pre-award trends. These analyses address EQ1, EQ2, and EQ3 by examining citation impact, interdisciplinarity, novelty, disruption, and outcome variance, thereby providing causal evidence on output profiles. Administrative program data and ResearchFish reporting support the analysis of follow-on funding, recognition, and early reputational effects. Taken together, this multi-method approach allows the evaluation to assess not only whether the program produces impacts, but also whether it operates consistently with its intended high-risk, exploratory mission.

Limitations. While the evaluation combines multiple complementary methods, several limitations should be borne in mind when interpreting the findings. First, part of the 1000 Ideas projects are still ongoing or only recently completed, meaning that longer-term scientific and societal impacts cannot yet be fully observed. Bibliometric and follow-on funding indicators, therefore, capture only early signals rather than mature outcomes. Second, survey evidence is subject to response bias and differential response rates between awardees and unfunded applicants, which may influence self-reported perceptions of novelty, risk, and additionality. Third, the econometric analysis of partial randomization is constrained by the relatively small number of applications that reach the "worthy of funding" pool and are then subject to the lottery. While this design creates a strong quasi-experimental setting in principle, in practice, the number of lottery winners and non-winners is limited, reducing statistical power. The results should therefore be interpreted as indicative evidence of how the lottery reshapes selection, rather than as precise estimates of its full causal impact. This reinforces the importance of triangulating the econometric findings with survey evidence and interview insights on how randomization affects fairness, diversity, and the risk profile of funded projects. For these reasons, the evaluation places greatest weight on triangulation across quantitative indicators, qualitative evidence, and comparative benchmarks rather than on any single metric or method.

To summarize, the evaluation used the following main components:

- **Document review.** This exercise comprised a thorough review of FWF-provided background and communications documentation pertaining to 1000 Ideas program, and the comparator, FWF's Principal Investigator program. The review aimed to develop a detailed understanding of the program's objectives, design features, and selection procedures, with particular emphasis on the assessment and decision-making processes.
- **Composition analysis.** An analysis of program data on applicants, their institutional affiliation, discipline, selection process outcomes and ResearchFish returns resulted in an accumulative and year-on-year trend review of the main types of applicants, success rates for each stage of the selection process, disciplinary trends among applicants and awardees and reported outcomes. The results of the analysis are presented in Section 2.3.
- **Bibliometric analysis and econometric analysis of bibliometric data.** Full details of this method component are provided in Appendix C. The econometric analysis combines longitudinal bibliometric data with quasi-experimental methods to assess both selection

effects and post-award research outcomes of the 1000 Ideas program. Using publication and text-based indicators covering scientific impact, interdisciplinarity, novelty, disruption, and broader societal relevance, the analysis compares funded and unfunded applicants over time and benchmarks the program against the standard FWF Principal Investigator program. Difference-in-Differences and Difference-in-Difference-in-Differences designs are employed to isolate post-award effects while accounting for pre-existing differences and common time trends, complemented by variance-based tests to examine whether the program is associated with greater dispersion in outcomes consistent with a high-risk, high-reward logic. In addition, selection-stage logit models exploiting partial randomization are used to assess how the lottery mechanism reshapes funding decisions by attenuating reliance on conventional performance indicators and increasing the relative weight of novelty-oriented characteristics. Together, these methods allow the evaluation to move beyond average effects and assess how the program changes what is funded and how research trajectories evolve.

- **Surveys of awardees and unfunded applicants.** Awardees were asked about their experience with program processes, opinions on process innovations, the realized or expected outputs of their work and the extent to which it differed from other grant funding work. Unfunded applicants were asked about their experience with program processes, opinions on process innovations, their satisfaction where feedback was received, and their possible resubmissions. The awardee survey was sent to 102 individuals and yielded a response rate of 61.8%. In turn, the population of 848 unfunded applicants responded with a rate of 29.2%. Further details of the survey and the raw data of the results can be seen in Appendix A.
- **Interviews with FWF staff, the 1000 Ideas Jury chair, members of the FWF Board, and PIs of funded projects.** In total 15 semi-structured online interviews were conducted to gain deeper insights into the implementation and impact of the 1000 Ideas program. Tailored questions allowed exploration of the perspectives of different stakeholders involved in and funded by 1000 Ideas. While interviews with FWF staff and actors involved in the selection process allowed for strategic reflections, management decisions and perspectives on the novel selection process, the interviews with award holders focused on gaining a context-sensitive understanding of the practical implementation of high-risk projects and comparing their funding experiences to other funding schemes. Appendix B provides a list of interviewees and the interview guidelines used.

Benchmark

Within the FWF funding portfolio, the comparison between the 1000 Ideas program and the Principal Investigator (PI) Projects is methodologically highly relevant and serves as a consistent benchmark across the evaluation. Both programs fund principal investigators at Austrian research institutions to conduct innovative basic research across all scientific disciplines. This allows differences in outcomes and impacts to be interpreted as reflecting program design features rather than differences in applicant type or research domain.

Benchmarking with the Principal Investigator program served as a reference point for both the qualitative and quantitative evidence in the evaluation. Across the methodological components, the PI Program served as the main comparator to assess the distinctive characteristics and effects of the 1000 Ideas program. The bibliometric analysis compared output indicators such as citation impact, interdisciplinarity, and novelty-related measures across the two programs using quasi-experimental econometric models. The applicant surveys

included a set of questions designed to capture perceived differences between 1000 Ideas and conventional PI funding, particularly regarding risk, novelty, and funding opportunities.

The qualitative interviews also placed strong emphasis on benchmarking with the PI program. FWF staff familiar with both funding instruments reflected on their strategic complementarity within the FWF portfolio as well as differences in implementation and assessment procedures. Members of the FWF Scientific Board elaborated on the effects of different selection modalities and the respective roles the two programs play within the Austrian research landscape. Interviewed principal investigators funded under 1000 Ideas frequently referred to the PI program during interviews to contextualize their experiences. In addition to interview questions that explicitly targeted cross-program comparison, the PI program emerged organically as a reference point throughout the interviews, serving as a baseline against which 1000 Ideas awardees interpreted the novelty, risk orientation, and distinctive features of the 1000 Ideas program. Table 1 provides a summary of key characteristics of both programs.

Table 1 Summary of 1000 Ideas and PI program key characteristics

	1000 Ideas Program	**Principal Investigator Program**
Program objective	Early-phase, high risk basic research funding	Thematically open basic research funding
Award size	Max. €175k	No fixed limit – depending on the amount of funding requested the number of reviewers increases (up to €450k two reviewers, for each additional increase of €200k one additional reviewer)
Award duration	24 months	48 months
Frequency of calls	Once per year	Rolling applications – 5 approval meetings per year
Success rate[1]	7,48%	23,23%
Projects per year	~20	~300
Requirements	Academic qualifications to lead the project, carried out at Austrian research institution with necessary infrastructure, employment at research institution of at least 50% (not financed by 1000 Ideas Program)	Academic qualifications to lead the project, caried out at Austrian research institution with necessary infrastructures
Selection process	Double-blind, partially randomized selection, based on assessments of FWF Board and international 1000 Ideas Jury	FWF Board based on international peer review
Other specific details	PhD students are not eligible for 1000 Ideas funding	

1.3 This report

In the second section of this report, we provide an introduction to the 1000 Ideas program, its processes and program composition analysis. The third section focuses on the program's impact. Chapter 4 focuses on program processes - whether and how program processes, especially double-blind review and lottery procedure, facilitate the selection of particularly innovative and high-risk research ideas. Chapter 5 provides main conclusions along the six evaluation questions and in the final main section, we provide recommendations and discuss the program's wider strategic significance, placement, and potential integration into other programs.

The table below summarizes how each evaluation question is addressed across the different strands of evidence and where the corresponding findings are presented in the report.

Table 2 Mapping of evaluation questions to evidence and report sections

Evaluation Question	Key Evidence and Methods	Main Report Sections
EQ1. To what extent have particularly original or high-risk research ideas been implemented, and have projects led to or laid the groundwork for groundbreaking findings?	• Survey evidence on perceived novelty and risk (awardees and unfunded applicants) • Bibliometric indicators of novelty, interdisciplinarity, disruption, and outcome dispersion • Cross-analysis of self-reported novelty and bibliometric outcomes • Qualitative examples from project end reports	Section 3.1 – Extent to Which the 1000 Ideas Program Has Enabled Original and High-Risk Research Leading to Groundbreaking Findings
EQ2. Does the program function as seed funding for high-risk projects unlikely to be funded elsewhere and likely to lead to follow-on funding?	• Survey counterfactual questions on whether projects would have occurred without 1000 Ideas • Survey and ResearchFish data on follow-on funding amounts and sources • Econometric DiD estimates on post-award outcomes • Qualitative descriptions of follow-on trajectories	Sections 3.3 and 3.4 – Additionality, Follow-on Funding, and Behavioral Effects
EQ3. Are there qualitative differences in outputs compared to FWF Principal Investigator (PI) projects?	• Econometric DiD and DDD comparisons between successful 1000 Ideas and PI projects • Survey and interviews data on how outputs differ	Section 3.5 – Output Profiles of 1000 Ideas Projects in Comparison to FWF Principal Investigator Projects
EQ4. Have projects failed, and how were failures dealt with in the context of a high-risk/high-reward program?	• Survey questions on failure incidence and learning and open-ended survey responses • Qualitative analysis of failure narratives, disciplinary differences in publication challenges based on survey and interview evidence	Section 3.2 – Failure as a Sign of Program Success
EQ5. Is the selection procedure (anonymization and partial randomization) suitable for identifying high-risk and original ideas, and how could it be improved?	• Survey evidence on awareness, perceptions, and behavioral influence of anonymization and randomization • Qualitative interviews with PIs, FWF staff, and Board members • Econometric evidence on selection effects	Section 4.2 – Role of Anonymization and Partial Randomization in Achieving Program Objectives
EQ6. Is a separate program necessary to achieve the stated objectives or could they also be integrated into another existing program? What adaptations might be necessary?	• Synthesis of findings across all method streams • Open-ended survey responses on future design options • Interview reflections • International examples of HRHR funding schemes	Section 6 – Recommendations on Program Design Options

2 1000 Ideas program

2.1 About the 1000 Ideas program

The 1000 Ideas program is a funding instrument of the Austrian Science Fund (FWF) designed to support radically novel and unconventional basic research across all scientific disciplines. It targets early-stage research ideas that extend beyond established scientific paradigms and have the potential to transform research domains or open entirely new lines of inquiry. By focusing explicitly on exploratory and conceptually ambitious projects, the program seeks to create space for research that would be unlikely to succeed in conventional competitive funding schemes.

The program originated from strategic reflections within the FWF leadership, initiated under a former FWF President. Drawing on experience across multiple funding schemes, FWF observed that standard peer review processes tend to be risk-averse, particularly when assessing high-risk, high-reward (HRHR) proposals. Therefore, the 1000 Ideas program was deliberately conceived as a small, targeted funding program designed to accommodate this type of research, recognizing that radically novel ideas often struggle to secure support through traditional evaluation mechanisms.

This strategic orientation is also aligned with broader science and innovation policy debates. In particular, the program's conceptualization reflects recommendations from the OECD's review of Austrian innovation policy, which call for stronger support for high-risk research and more experimental funding instruments (OECD, 2018). In practice, securing funding for unconventional research is often challenging due to limited evidence of feasibility and a high probability of failure. The 1000 Ideas program addresses this gap by providing seed funding to PIs at Austrian research institutions during an early and uncertain exploratory phase, with the acceptance of failure.

To understand the objectives, selection logic, and observed outcomes of the 1000 Ideas program, it is important to clarify how the FWF defines "high-risk/high-reward" research and the role of "failure". These concepts are articulated in the guidance provided to applicants and jury members, and they can shape what kinds of projects are proposed, how they are assessed, and how outcomes are interpreted. The following box summarizes the language used by the FWF to describe the nature of high-risk, transformative research.

How the FWF describes "high-risk/high-reward" and "failure" in the 1000 Ideas program

Guidance to applicants

"Funding can be requested for projects that investigate radically new and daring or original research ideas at an early stage. The project must be clearly defined and include a convincing description of the aims and methods. Additionally, the project must have high relevance for science or (arts-based) research and fall in the domain of basic research. These kinds of *unconventional* ***research ideas often involve interdisciplinary research approaches or are located at the boundaries of existing disciplines."***

"It is often difficult for researchers to take the first step when it comes to new, particularly original, or daring research ideas that go beyond the common understanding of science and scholarship. This is where the 1,000 Ideas program comes in. The project's chances of successful implementation are not as important as having the 'courage to *fail,'* ***which is an integral component of the program. The key***

aim of the program is to tackle** novel, **forward-looking themes with high relevance for science, research, and, ideally, society. They should exhibit the potential to transform existing research domains and/or fundamentally challenge established paradigms in science and research.“

Guidance to the International Jury

Definition of HRHR research: “Radically new and risky as well as particularly original research ideas that are beyond the current scientific understanding; high potential to transform a field of research and/or to fundamentally question established paradigms in science and research”.

The Jury is asked to assess the extent to which: “the underlying research idea** has the potential to fundamentally challenge a research domain, an established notion of research, or the accepted status quo or cause an unexpectedly substantial leap in the current research area**.“

While FWF is willing to take on a high level of risk in funding projects that may not lead to successful outcomes, the program is deliberately designed to minimize risks to applicants' careers. In particular, the scheme does not fund PhD positions and requires PIs to hold at least a 50% position at an Austrian research institution. This safeguarding logic reflects the view that postdoctoral researchers with a secure institutional position are better able to absorb the potential failure of a high-risk project, as they retain parallel research activities and employment security. By contrast, exposing PhD candidates, whose career prospects are more strongly tied to the success of a single project, to such levels of uncertainty was considered less appropriate.

Within the FWF portfolio, the 1000 Ideas program is explicitly designed as a seed-funding mechanism. Its primary objective is to support the generation of early evidence, preliminary data, and proof-of-concept results that can subsequently serve as a basis for applications to larger FWF funding instruments or external programs. In this sense, the program plays a strategic role by bridging the gap between speculative ideas and more established research trajectories, enabling researchers to test bold concepts before seeking more substantial funding.

The funding decisions for the 1000 Ideas program follow FWF's bottom-up principles of competitive research funding, but incorporate several procedural innovations. Projects are selected by the FWF Scientific Board based on the recommendations of a dedicated 1000 Ideas Jury, and a subset of applications is awarded through a partial randomization (lottery) mechanism among proposals deemed worthy of funding. This approach explicitly acknowledges the limitations of conventional peer review in identifying radically new ideas and embeds procedural experimentation as a core element of the program's design.

A further distinctive feature is the use of anonymized project proposals in the assessment process. Reviewers and jury members do not have access to information on applicants' identities, affiliations, or track records, which is intended to shift attention away from reputation and towards the intrinsic quality and originality of the research idea itself. By reducing the influence of status-related biases, anonymization aims to enable fairer consideration of unconventional proposals.

Beyond its direct funding function, the 1000 Ideas program also serves as a pilot for testing these innovative selection processes with a view to exploring their potential applicability in other FWF funding schemes.

Since its launch in 2019, five calls have been implemented, resulting in a total of 1,135 applications, of which 107 projects have been approved. This corresponds to a success rate of around 7.5%.

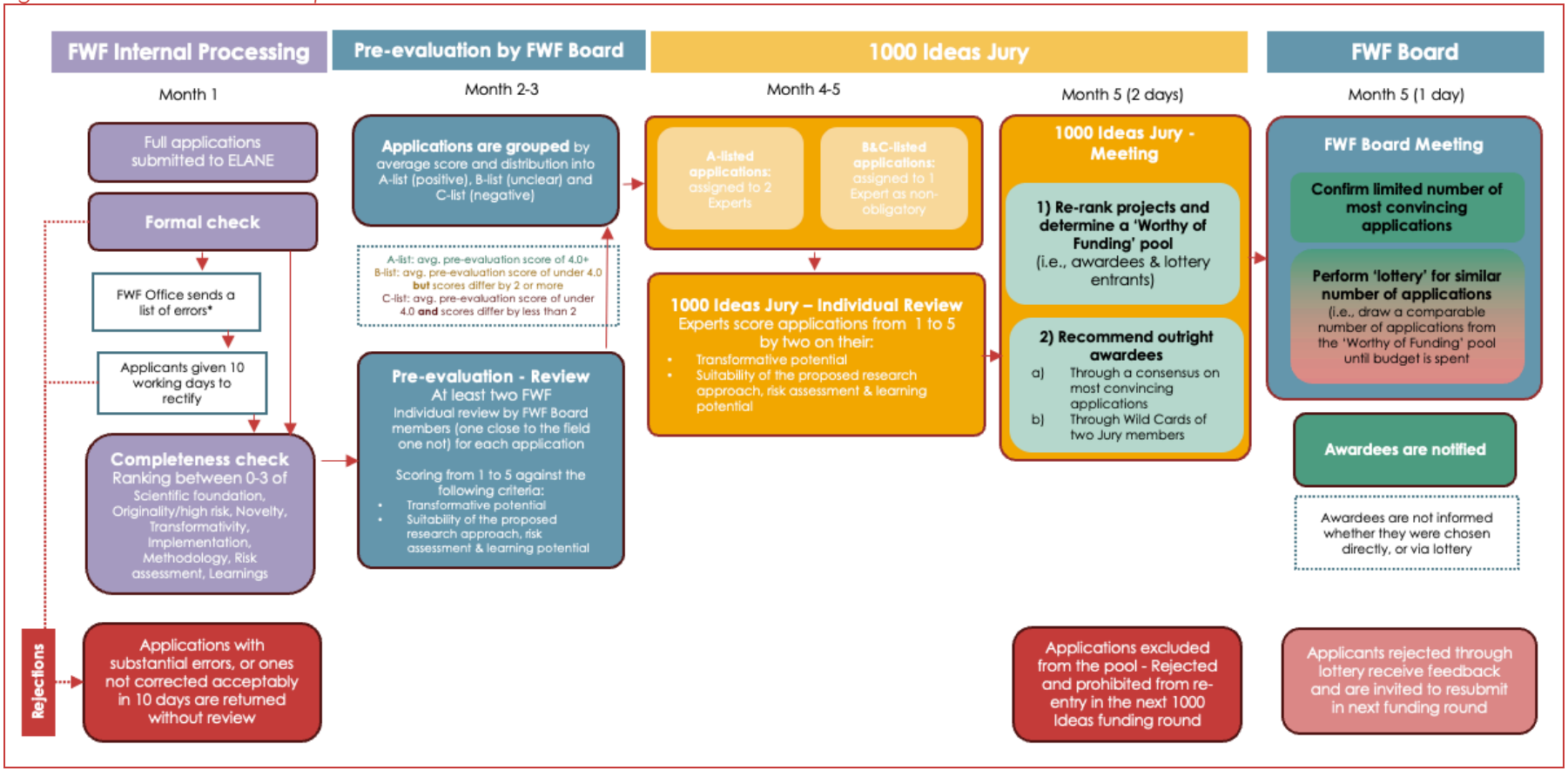
2.2 Process overview

The program uses a multi-stage assessment and selection process. Figure 1 shows an overview of the key steps, actors, and decision points in this process, highlighting how formal review, expert judgement, and partial randomization are combined.

The time from the closing of submissions to the announcement of funding outcomes is about five months. Following submission, applications first undergo a formal check by the FWF Office. Only a narrow set of formal inaccuracies or omissions, limited to specific administrative forms may be corrected at this stage provided that the cost justification is not affected. Where such correctable issues are identified, applicants are given ten working days to submit revisions. Applications containing errors outside these defined categories, or for which corrections are not made within the specified timeframe, are returned without proceeding to scientific review.

Applications that pass the formal check proceed to a pre-evaluation by the FWF Scientific Board. Each such application is reviewed by two FWF Board members who score the application from 1 to 5 on 'Transformative potential', and 'Suitability of the proposed research approach and presentation of risk assessment and learning potential'. Based on the mean score, the Board ranks and groups the proposals into Group A (mean score at least 4), Group B (mean score is below 4, but the two review scores differ by more than two), and Group C (mean score is below 4, and the assessment scores do not differ by more than two).

Figure 2 1000 Ideas Selection process chart



Source: Technopolis based on program guidance

Note: *Following submission, applications undergo a formal check by the FWF Office, during which only a narrow set of administrative errors may be corrected.

In practice, Group A typically comprises more than 10% of applications, though its size varies by call depending on the overall score distribution. For example, in one recent call with around 220 applications, approximately 50–60 proposals were placed in Group A, around 10 in Group B, and the remainder in Group C. The cut-off for inclusion in Group A is not fixed but is adjusted slightly between calls (typically between mean scores of 3.75 and 4.0), depending on how generously or conservatively applications are scored overall in a given year.

The group to which an application is allocated determines the level of attention afforded to it in the Jury review, which takes place in months 4 and 5. Each Group A application is assigned to two Jury members for written evaluation, while Group B and C applications may be reviewed voluntarily by up to one Jury member each. Based on these written evaluations, a cut-off line is established to determine which applications will be discussed during the Jury meeting. The cut-off is set pragmatically, taking into account the overall distribution of scores. Jury members are invited to review and confirm the resulting shortlist and may propose additional applications for discussion if they consider them particularly worthy of attention. The Jury meeting then focuses on this selected subset of applications, with the objective of identifying those deemed 'worthy of funding' and a sub-group recommended for immediate funding.

Shortly after the Jury meeting, the FWF Board meets in Month 5, to confirm the final selection of awardees. According to the program processes, the Board can overturn the Jury decisions on outright awardees and worthy applications, although to date it has never done so. The Board then draws a comparable number of awardees at random from the 'worthy of funding' pool, At present, the drawing is done via sealed letters. Finally, applicants are informed by e-mail whether their application has been successful. They are not told whether their application was selected outright or via lottery.

Applications in the "worthy of funding" pool that are not chosen can be resubmitted in the next call for proposals. They are the only group of rejected applicants who receive feedback (the feedback component having been introduced in 2022). This feedback is an approximately half-page summary of the observations, strengths, and opportunities for improvement from the Jury review. Unsuccessful applications not in the 'Worthy of Funding' pool are not allowed to be re-entered in the following funding round.

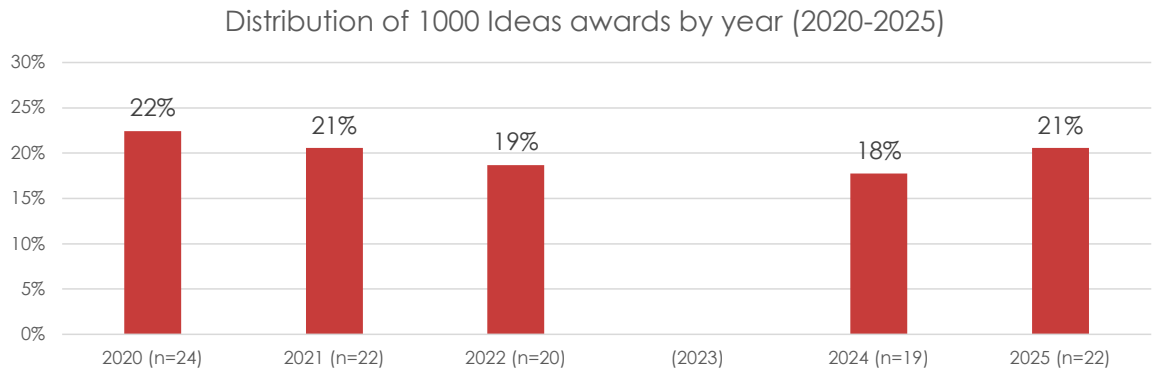
2.3 Composition analysis of the 1000 Ideas program

The following section is based on 1000 Ideas program monitoring data provided by FWF and gives a descriptive overview of the program since its inception. More specifically, it analyses the characteristics of the program in terms of funding and participation patterns, awardees and disciplinary coverage.

2.3.1 Funding by year

This analysis covers the years from 2020 (the first year of 1000 Ideas) to 2025. FWF did not hold a call for 1000 Ideas applications in 2023 due to lack of budget. The number of granted projects per call has varied a little. At a minimum, the program awarded 19 grants in 2024, and at most 24 in 2020.

Figure 3 Count of 1000 Ideas grants over time



The program budget has remained between 2.9 and 3.5 million Euros annually. The award sizes have ranged from 74,000€ to slightly under 190,000€.

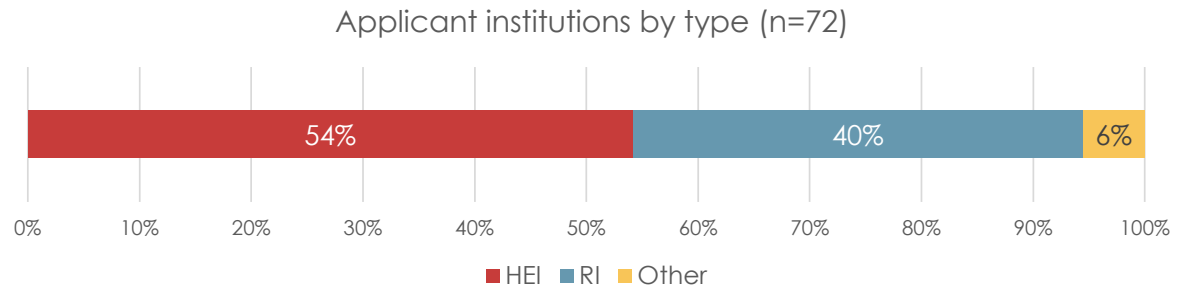
Table 3 Grant values by year

Year	Number of grants	Total value	Mean	Median	Max	Min
2020	24	3.4m €	141,553.73 €	149,185.08 €	153,463.45 €	93,104.55 €
2021	22	3.27m €	148,681.81 €	150,420.15 €	152,126.10 €	130,466.70 €
2022	20	2.9m €	145,192.00 €	151,138.84 €	153,491.55 €	100,655.56 €
(2023)	N/A	N/A	N/A	N/A	N/A	N/A
2024	19	3.25m €	170,828.43 €	181,108.90 €	189,782.25 €	110,651.70 €
2025	22	3.5m €	159,029.85 €	176,603.18 €	179,727.71 €	74,004.00 €

2.3.2 *Program awardees*

During the program implementation, PIs of 72 Austrian research institutions have applied to the program. Higher Education Institutions (HEIs) make up the largest population by institution type, with 39 (54%) HEIs, compared to 29 (40%) Research Institutes (RIs) and four 'other' types of institutions (6%). These four included organizations like a museum, a monastery and clinical hospitals.

Figure 4 Distribution of institution types among 1000 Ideas applicants



Of the 1,135 applications, 966 (85%) were submitted by PIs of HEIs, compared to 159 (14%) submitted by PIs of RIs and ten (1%) submitted by PIs of other types of institutions. RI-produced applications have been slightly more successful, where 13% of RI-generated proposals have

been awarded compared to 9% of those submitted by HEIs. By contrast, none of the applications submitted by the four other types of institutions have been successful.

Table 4 Number of applicants, awards and success rates by institution type and location

	Number of applications	Number of awards	Success rate
Institution type			
HEIs (n=39)	966	87	9%
RIs (n=29)	159	20	13%
Other institutions (n=4)	10	0	0%
Institution location			
In Vienna (n=32)	612	61	10%
Outside Vienna (n=40)	523	46	9%

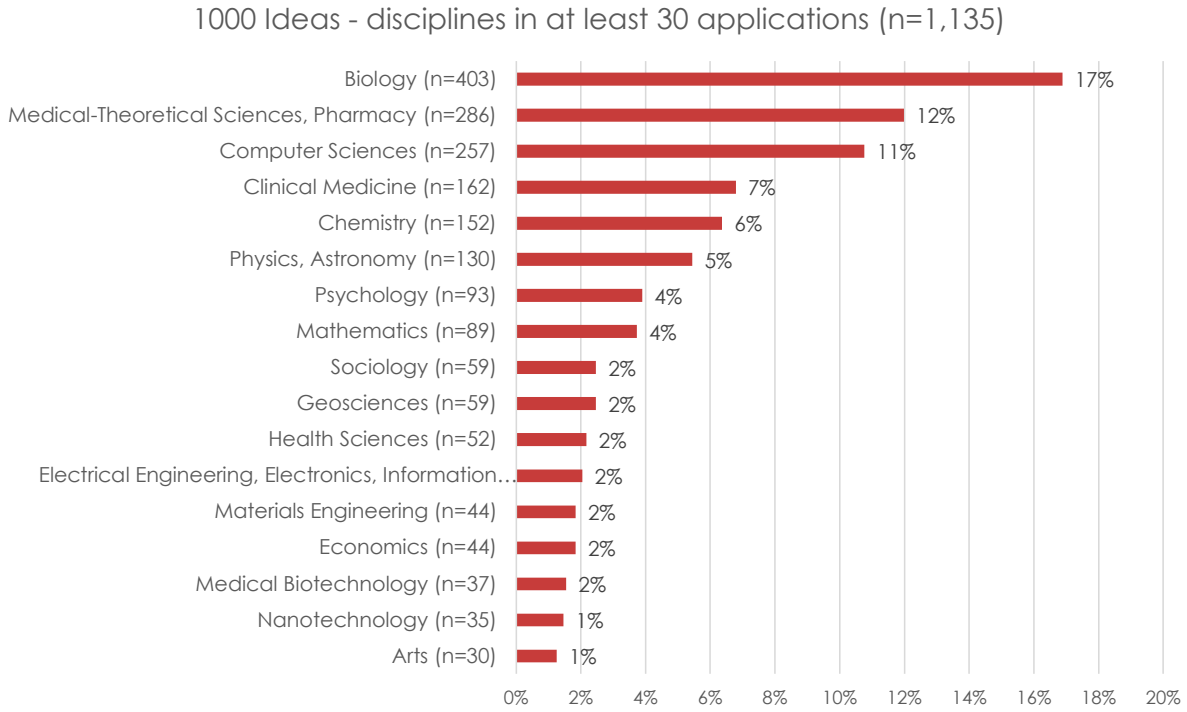
Institution location

Comparing institutions in terms of location, a small majority is based outside of Vienna (56%, n=40, compared to 44%, n=32 in Vienna). However, Vienna-based institutions have submitted a slightly higher volume of applications (n=612, approximately 19 applications on average, compared to n=523 among institutions outside of Vienna, approx. 13 submissions per institution). Based on the award data, institutions between the two location types have a largely even success rate, with 10% of applications from Vienna-based institutions having been awarded compared to 9% of those submitted by applicants outside of Vienna.

2.3.3 *Disciplinary coverage*

Applications to date have listed relevance to 42 disciplines, ranging from media and communication sciences to physics. As shown in Figure 5, the most common fields across all applications align largely with the medical, life, and STEM sciences. For example, nearly a fifth of all applications hold relevance to biology (17%, n=403).

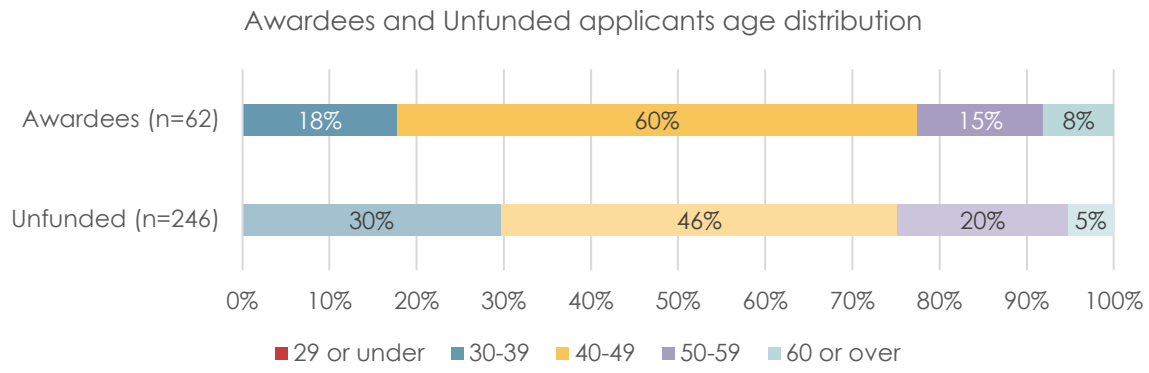
Figure 5 Most commonly featured disciplines in1000 Ideas applications



A year-on-year observation of disciplines shows that life, natural, and STEM subjects consistently account for a large share of 1000 Ideas proposals. Biology, in particular, is included in the largest number of applications each year. Additionally, the ten most common disciplines accounted for a larger share of the total application pool each year: in 2020, the ten most common disciplines accounted for 66% of all submitted applications, and by 2025, 78% of all submitted applications included at least one of the ten most common fields. This may indicate that awareness of the program and its perceived suitability is particularly strong in certain disciplinary communities.

While program data does not include applicant age or career stage, this question was included in the survey questionnaire of awardees and unfunded applicants. Although survey-based evidence is not fully representative of the entire applicant population, it provides valuable complementary insights. As shown in Figure 6, all applicants to the 1000 Ideas program are aged at 30-years-old or above. Sixty percent of the successful applicants are aged 40-49.

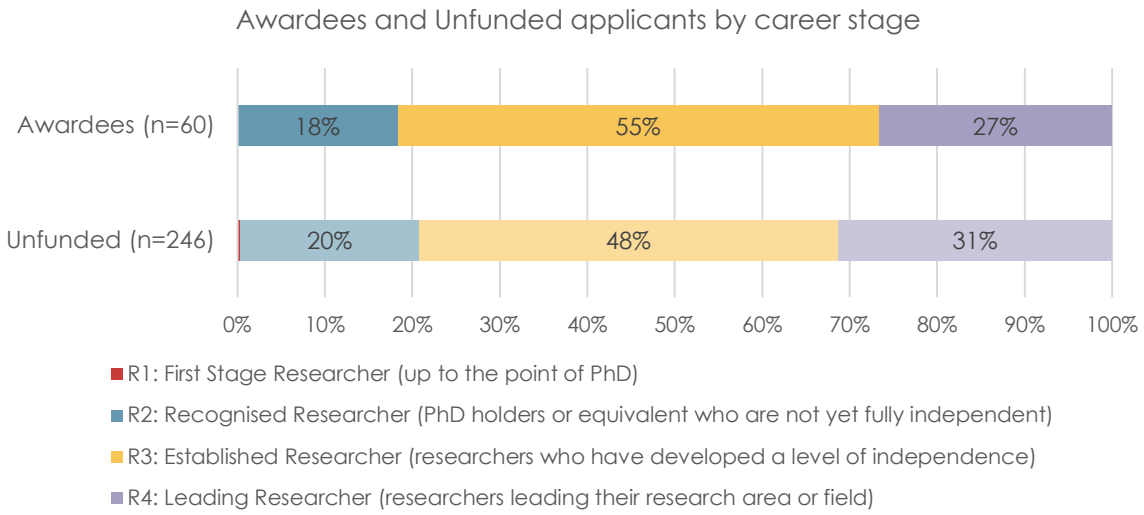
Figure 6 1000 Ideas awardees and unfunded applicants by age



Source: Technopolis survey of 1000 Ideas unfunded applicants and awardees

Figure 7 shows that the majority of applicants to the 1000 Ideas program are established researchers, with relatively few respondents identifying as early-stage researchers. This distribution suggests that the program primarily attracts and supports researchers who have already established a degree of independence and credibility within their fields.

Figure 7 1000 Ideas awardees and unfunded applicants by career stage

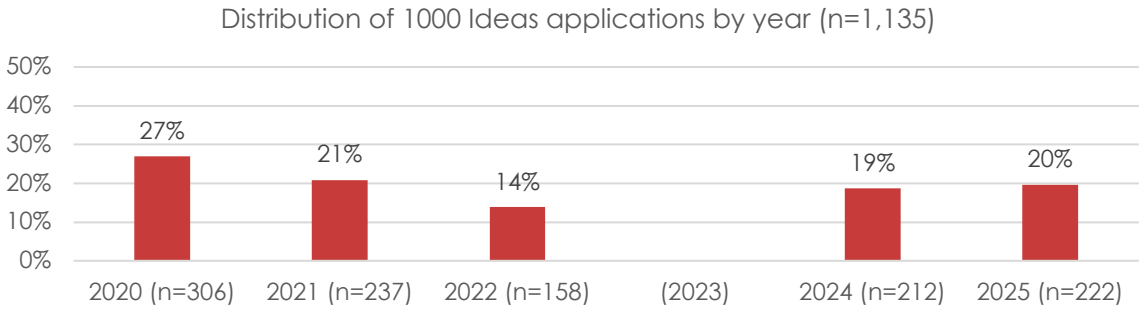


Source: Technopolis survey of 1000 Ideas unfunded applicants and awardees

2.3.4 *Applications and success rates*

Up to and including the 2025 funding call, the 1000 Ideas program has received 1,135 applications. Most were submitted in the program's first year. The number of applications decreased over the following two years, reaching its lowest to date in 2022 (n=158). After a pause in funding in 2023, the volume has risen back to 200+ in 2024 and 2025.

Figure 8 1000 Ideas program applications by year from 2020 to 2025



The average success rate in 1000 Ideas overall has been 9% (107 succeeded out of 1,135 application). his level has been fairly stable over time, as shown in Figure 9. In absolute numbers, the 1000 Ideas program has awarded at minimum 19 (2024) and at most 24 (2020) grants each year.

Figure 9 Application success rates by year from 2020 to 2025

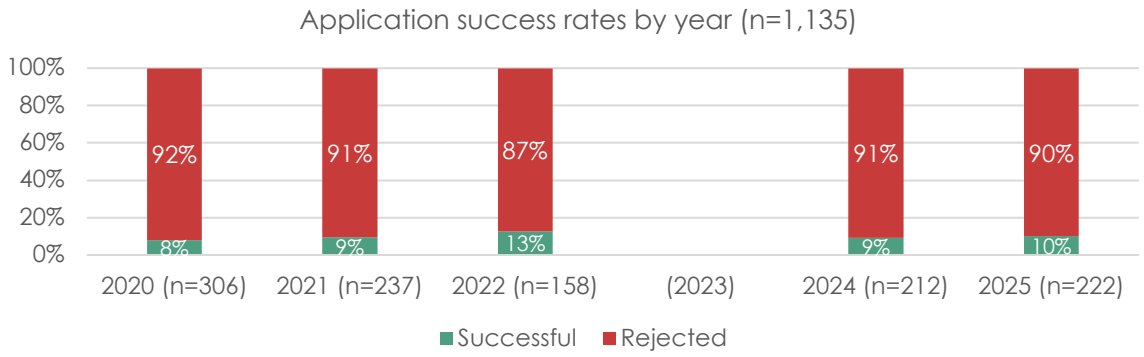


Table 5 highlights a difference between disciplines that dominate the program in terms of volume of awards and those that perform best in terms of success rates. In absolute numbers, awards are concentrated in life sciences and STEM fields, with Biology, Medical-Theoretical Sciences, Computer Science, and Physics accounting for the largest shares of funded projects. This largely reflects the overall application pool, where these disciplines also submit the highest number of proposals. In contrast, disciplines with the highest success rates include more social sciences and humanities (Law (25%), History, Archaeology (23%), Human Geography (22%)). However, we note that these fields are rarer among 1000 Ideas applications. With fields with under 30 applications removed, the highest success rates, as with the highest number of awards, aligns with the application-heavy STEM disciplines, particularly Physics and Astronomy and Geosciences. This pattern suggests that while STEM fields drive the bulk of demand for 1000 Ideas funding, applicants from SSH disciplines, when they apply, are comparatively more likely to be successful, potentially reflecting differences in competition intensity, proposal fit with the program's exploratory objectives, or the relative novelty of proposals within the scheme.

Table 5 Ten most awarded disciplines & ten disciplines with the highest success rates among 1000 Ideas applications

Disciplines by number of awards	Awards	Disciplines by success rate	Success rate
Biology (n=403)	**45**	Sociology (n=59)	**17%**
Medical-Theoretical Sciences, Pharmacy (n=286)	**24**	Physics, Astronomy (n=130)	**14%**
Computer Sciences (n=257)	**23**	Geosciences (n=59)	**14%**
Physics, Astronomy (n=130)	**18**	Biology (n=403)	**11%**
Chemistry (n=152)	**13**	Arts (n=30)	**10%**
Sociology (n=59)	**10**	Computer Sciences (n=257)	**9%**
Geosciences (n=59)	**8**	Chemistry (n=152)	**9%**
Clinical Medicine (n=162)	**8**	Medical-Theoretical Sciences, Pharmacy (n=286)	**8%**
Mathematics (n=89)	**4**	Materials Engineering (n=44)	**7%**
Psychology (n=93)	**3**	Electrical Engineering, Electronics, Information Engineering (n=49)	**6%**

We also note that the value of funding per discipline aligns with the number of awards per discipline. Following the considerable number of grants to applications involving biology,

applications in biological sciences have received more than €7m of 1000 Ideas program funding.

Table 6 Ten most funded disciplines by total value of awarded funding

Discipline	Total funding
Biology	€ 7,070,510.89
Medical-Theoretical Sciences, Pharmacy	€ 3,587,026.31
Computer Sciences	€ 3,420,890.34
Physics, Astronomy	€ 2,800,628.94
Chemistry	€ 2,035,214.27
Sociology	€ 1,487,571.68
Clinical Medicine	€ 1,226,735.72
Geosciences	€ 1,199,968.31
History, Archaeology	€ 830,735.45
Human Geography, Regional Geography, Regional Planning	€ 801,768.46

Source: 1000 Ideas program data

3 Impact of the 1000 Ideas program

This section assesses the impact of the 1000 Ideas program across multiple dimensions, combining survey evidence, qualitative insights, ResearchFish data, bibliometric and econometric analysis, and project-level examples.

The analysis of program impacts follows the logic model introduced in Section 1. In line with the program's intervention logic, the evaluation distinguishes among immediate outputs (e.g., exploratory results, failure and learning, proof-of-concept work), intermediate outcomes (e.g., structural novelty, follow-on funding, shifts in research trajectories), and longer-term impacts that may only materialize over time. The following sections, therefore, assess not only whether 1000 Ideas projects produced influential publications, but also whether they generated the kinds of early-stage knowledge and trajectory shifts that the program is designed to support.

Given the program's high-risk, high-reward orientation, impact is examined primarily through indicators of novelty, notion of failure, behavioral change, additionality, and longer-term research trajectories. Throughout the analysis, the FWF Principal Investigator (PI) program is used as a benchmark, allowing impacts to be interpreted relative to a well-established funding instrument.

3.1 Extent to Which the 1000 Ideas Program Has Enabled Original and High-Risk Research Leading to Groundbreaking Findings

Groundbreaking, highly original, or high-risk research is difficult to define, as it may take diverse forms, mean different things in different disciplines, and often only becomes recognizable in retrospect. This section, therefore, examines a set of qualitative and quantitative indicators to assess both the extent to which the 1000 Ideas program has enabled high-risk research and the degree to which there is emerging evidence of projects leading to, or laying the groundwork for, potentially groundbreaking findings.

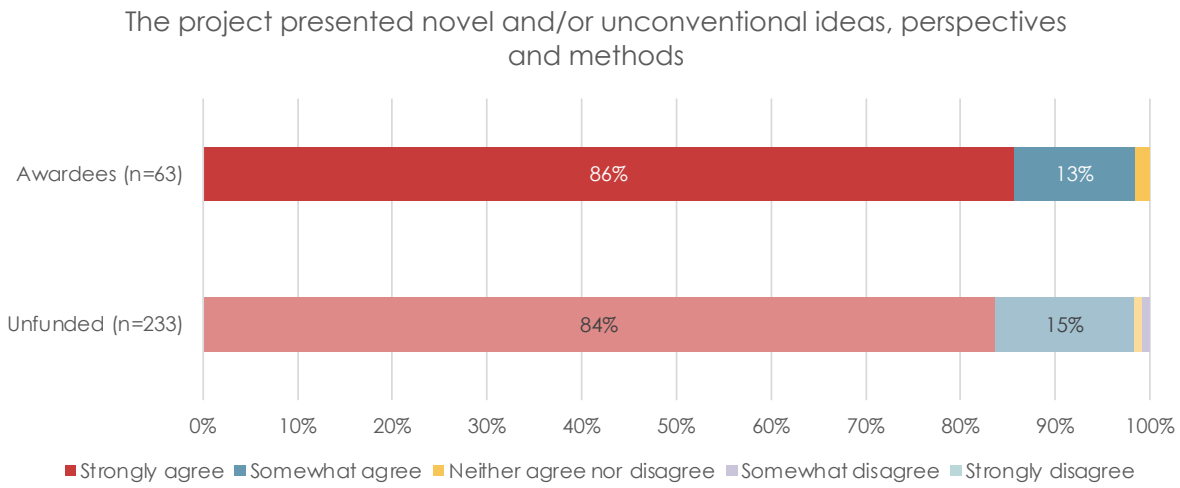
Before turning to the empirical evidence, it is important to recall how FWF defines high-risk, high-reward research and "failure", as these concepts provide the reference against which the program's outcomes can be assessed. The FWF's explanations of high-risk/high/reward and "courage to fail" play a role in shaping how the 1000 Ideas program operates in practice. The program defines HRHR primarily in terms of transformational potential, i.e. the capacity to challenge established paradigms, cross disciplinary boundaries, and open new research domains. Failure is framed not as non-performance but as a legitimate outcome of focusing on novel ideas at an early stage. This framing helps explain some of the key findings in the following sections. The FWF guidance encourages the submission and selection of conceptually uncertain ideas. The risk profile observed from the PI's self-reported understanding and produced outputs aligns with FWF's conceptualization of HRHR.

3.1.1 Evidence on novelty, risk and emerging transformative outcomes

The evidence presented in this section draws on multiple sources at both the application level and outcome level, including survey data from awardees and unfunded applicants, bibliometric indicators of research outputs, econometric analysis of outcome variance, ResearchFish data on early recognition, and qualitative project examples. **Taken together, these data suggest that the 1000 Ideas program attracts a highly novel applicant pool and supports a research portfolio characterized by substantial exploratory ambition, uncertainty, and a non-negligible share of projects with emerging signs of transformative potential.**

Survey responses indicate (Figure 10) that a large majority of both awardees and unfunded applicants perceive their projects as involving novel or unconventional ideas, perspectives, or methods. Among awardees, 86% strongly agreed and a further 13% somewhat agreed that their project presented novel and/or unconventional approaches; responses among unfunded applicants were very similar (84% strongly agree and 15% somewhat agree). This high level of agreement suggests that the applicant pool to the 1000 Ideas program is strongly oriented towards novelty and unconventionality. The program attracts research proposals that self-identify as highly novel, which is a necessary precondition for enabling potentially groundbreaking research.

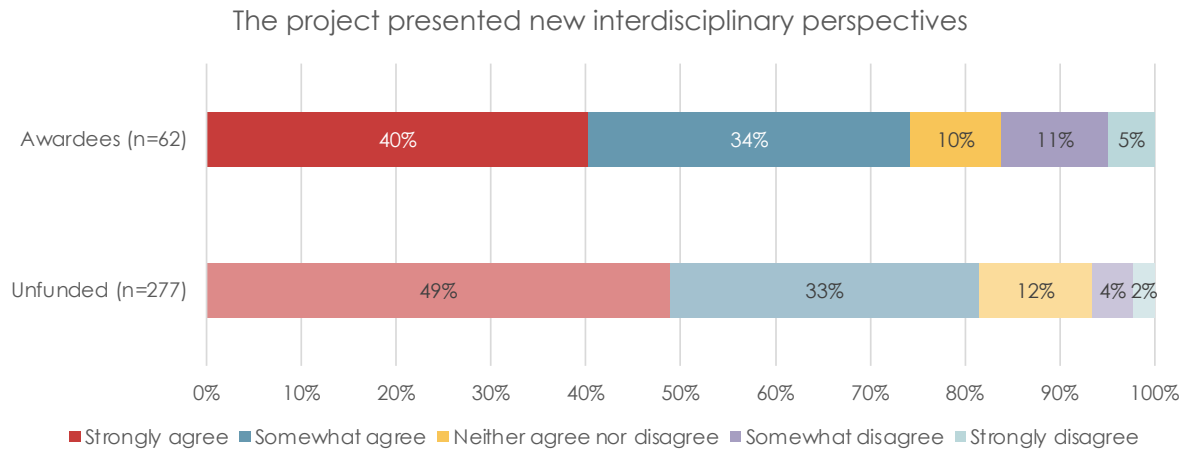
Figure 10 Answers to survey question "Please describe your project idea on the following criteria", novel ideas



Source: Technopolis surveys of 1000 Ideas program awardees and unfunded applicants

Awardees were somewhat less likely than unfunded applicants to report that their project presented new interdisciplinary perspectives (40% strongly agree among awardees compared to 49% among unfunded applicants), although levels of overall agreement were high in both groups (Figure 11).

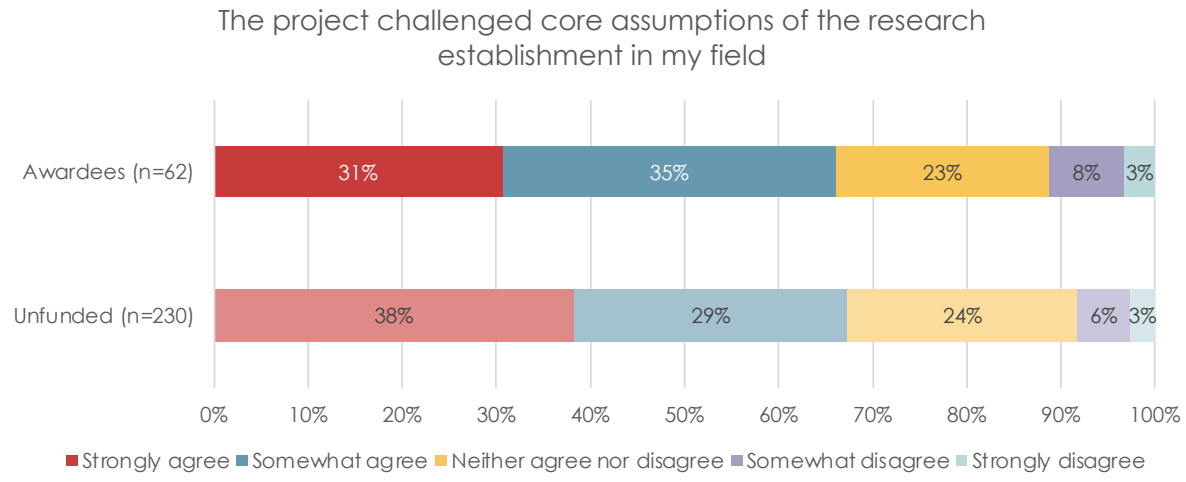
Figure 11 Answers to survey question "Please describe your project idea on the following criteria", interdisciplinary perspectives



Source: Technopolis surveys of 1000 Ideas program awardees and unfunded applicants

In contrast, awardees were less likely to strongly agree that their project challenged core assumptions of the research establishment in their field (31% compared to 38% among unfunded applicants), with a larger share of awardees expressing more neutral views. This pattern suggests that, while funded projects are clearly perceived as novel and high-risk, applicants may less frequently frame them as directly challenging dominant paradigms.

Figure 12 Answers to survey question "Please describe your project idea on the following criteria", core assumptions of the research establishment

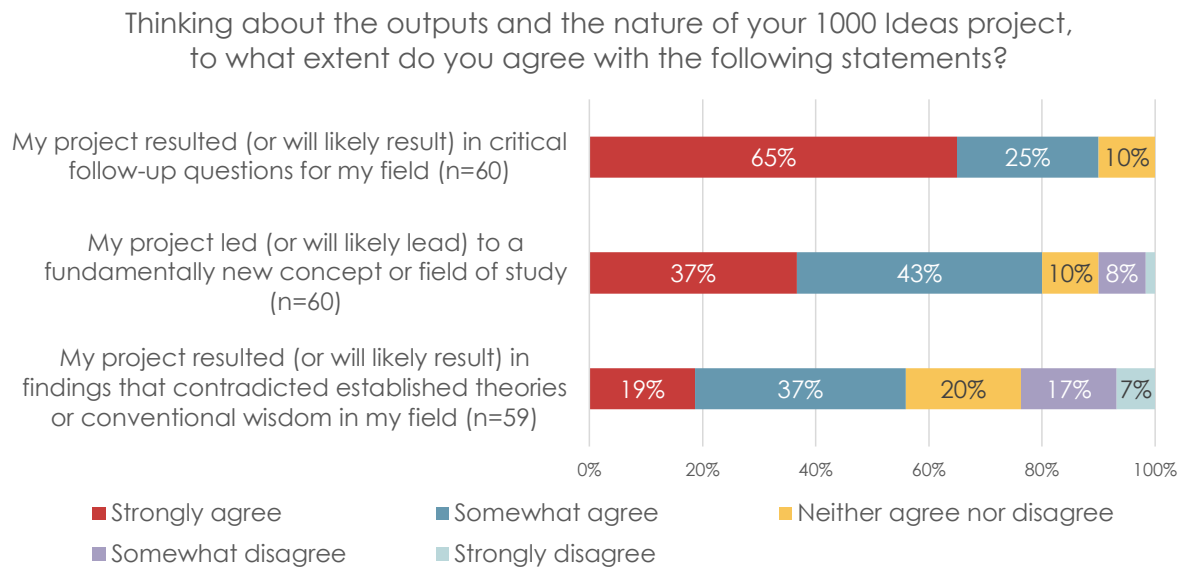


Source: Technopolis surveys of 1000 Ideas program awardees and unfunded applicants

When asked about project outputs, awardees suggest that many funded projects have the potential to shape future research trajectories. Around two-thirds of awardees (65%) strongly agreed that their project resulted, or is likely to result, in critical follow-up questions for their field, indicating a substantial perceived contribution to advancing research agendas. In addition, a large majority agreed that their project led, or is likely to lead, to a fundamentally new concept or field of study, although strong agreement was less prevalent (37%), suggesting that such

outcomes are recognized but remain uncertain or emergent. By contrast, fewer awardees reported that their findings contradicted established theories or conventional wisdom.

Figure 13 Self-reported contribution of 1000 Ideas projects to new concepts, follow-up questions, and challenges to established knowledge



Source: Technopolis surveys of 1000 Ideas program awardees

A cross-analysis linking awardees' self-reported assessments of outputs (as presented in Table 7) with bibliometric indicators provides additional insight into how self-reported perceptions of groundbreaking research relate to observable output characteristics. Awardees who reported stronger agreement that their projects generated fundamentally new concepts, contradicted established theories, or produced critical follow-up questions tended, on average, to produce outputs with higher interdisciplinarity, novelty, and disruption bibliometric scores. In particular, respondents who strongly agreed that their projects generated critical follow-up questions showed consistently higher values across all three bibliometric indicators (see Appendix D for full details). Overall, the results indicate a clear alignment between researchers' subjective assessments of novelty and bibliometric data.

The evaluation also examined whether the 1000 Ideas program produces a higher-variance outcome profile in line with a high-risk/high-reward (HRHR) funding logic (full details of the analysis are in Appendix C). Using residuals from a fixed-effects Difference-in-Difference-in-Differences (DDD) event-study model of field-weighted citation impact (FWCI), post-award dispersion in outcomes was compared across programs (1000 Ideas program and PI program) and funding statuses (funded or unfunded), focusing on post-award years only. The results show that, among funded projects, the dispersion of citation outcomes is significantly higher in the 1000 Ideas program than in the PI program (Panel A in Table 7), indicating a more uncertain outcome profile once funding is awarded. In contrast, no significant difference in dispersion is observed between programs among unfunded applicants, suggesting that baseline outcome variance is similar across applicant pools and that the higher dispersion emerges from program-specific funding conditions rather than pre-existing applicant characteristics. Within the 1000 Ideas program (Panel B), funded projects also display greater dispersion than unsuccessful applicants.

Table 7 Post-award residual variance in Field-Weighted Citations

Panel A. Program comparison (PI vs 1000 Ideas)

Sample	PI (SD)	1000 Ideas (SD)	Equality of variances (p-value, W0)
All projects	2.501	2.971	0.023**
Funded projects	2.454	3.009	0.000***
Unfunded projects	2.521	2.969	0.300

Panel B. Within 1000 Ideas (funded vs unfunded)

Sample	Unfunded (SD)	Funded (SD)	Equality of variances (p-value, W0)
1000 Ideas only	2.969	3.009	0.024**

Note: SD refers to the standard deviation of post-award residuals of field-weighted citation impact (FWCI). Equality of variances is tested using robust variance tests (robvar). Reported p-values correspond to the Brown–Forsythe-type statistic (W0) and are robust to alternative statistics, including the Levene-type test based on absolute deviations from the median (W50) and the Levene-type test based on absolute deviations from the trimmed mean (W10). Residuals are obtained from a fixed-effects DDD event-study model of three-year FWCI including author fixed effects and full interactions between event time, funding success, and program type. Variance comparisons are restricted to post-award years and balanced observation windows. Standard errors in the underlying regression are clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.

Taken together, this provides quantitative support for the interpretation that the 1000 Ideas program enables a research portfolio with a higher-risk/high-reward profile, consistent with a program that accepts greater risk.

In addition to survey evidence and bibliometric indicators, ResearchFish data provide insight into early forms of scientific recognition and reputational effects associated with 1000 Ideas projects. Across the reporting period, awardees recorded 38 instances of awards or formal recognition, reported by 17 projects, with the most common category being invitations to serve as keynote speakers at scientific conferences (21 cases). The majority of recognitions were international in scope (29 out of 38), suggesting that even at an early stage, a subset of projects has achieved visibility within global research communities. These forms of recognition are particularly relevant for high-risk, exploratory research, as they capture emerging influence and agenda-setting effects that may precede conventional bibliometric impact and are not well reflected in citation-based metrics.

While bibliometric and econometric evidence captures important dimensions of novelty and risk, interviews with funded PIs provide complementary insight into how "high-risk" and "original" research is actually enacted in practice. Most interviewees described their projects as "hop-or-drop" endeavors - research trajectories where it was genuinely uncertain whether the proposed approach would work at all. While this level of uncertainty is common in basic research, PIs emphasized that 1000 Ideas allowed them to pursue unusually fragile or speculative pathways. At the same time, **many interviewees were cautious about labelling their results "groundbreaking", noting that paradigm shifts rarely become visible within a short funding window and that recognition often lags behind discovery**.

Interviews with PIs show that novelty in 1000 Ideas projects takes multiple, often intersecting forms. Funded projects typically combine technological, conceptual, and empirical originality in ways that would be difficult to accommodate within conventional funding schemes.

One kind of novelty lies in **applying new technological possibilities**. Awardees developed highly specialized techniques that attracted international attention, leading to invitations into

MSCA networks, visiting researchers, and training roles. In several cases, this tacit knowledge became the main vehicle through which impact was transmitted, even before formal publications emerged. For example, *Sex-based differences in Bronze Age childcare* used a newly developed, non-destructive technique for identifying sex from teeth to investigate social patterns in prehistoric populations. Similarly, *ELPHI: A Next Generation Device for Brain Cancer Treatment* developed an implantable electronic pump that enabled the local delivery of chemotherapeutic agents that cannot cross the blood–brain barrier, opening new therapeutic possibilities that standard clinical approaches cannot address.

Another form of originality comes from **reframing long-standing problems through new disciplinary lenses**. Projects such as *Universal Optimality of the Hexagonal Lattice* revisited classical mathematical questions using tools from sampling theory, while *The Human Mini-Cell* approached the fundamental problem of chromosome essentiality by experimentally reducing the human genome using molecular biology techniques. In both cases, the novelty lies not only in the topic, but in redefining how the problem itself is conceptualized.

Interdisciplinary recombination also featured prominently. Projects such as *Reading the Past from the Surface of the Earth* combined satellite imagery, historical cadastral maps, and computational analysis to reconstruct long-term landscape transformations, creating a research object that no single discipline could have produced alone.

Several projects also broke new ground by **studying previously unexplored phenomena**. For example, *Social Movements, Platforms, and In/Visibility* analyzed the online dynamics of two emergent movements - "Tradwives" and the "4B Movement" - that had not yet been systematically examined in social science research.

Finally, novelty was sometimes rooted in **unusual empirical cases**. *Immortal Titans* used the titan arum, a rarely studied plant species, as a model to investigate whether flowers possess germline-like structures, opening new perspectives on plant development and evolution.

Finally, the following examples informed by project end reports and interviews illustrate how the high-risk/high-reward logic of the 1000 Ideas program translates into concrete research trajectories and outcomes. They highlight the diversity of ways in which funded projects have pursued speculative ideas, encountered uncertainty and failure, and in some cases generated genuinely transformative scientific insights.

Transformative breakthroughs from speculative ideas

Several funded projects illustrate how the 1000 Ideas program enables genuinely high-risk research to produce potentially transformative scientific breakthroughs. For example, one project[2] showed for the first time that a specific biological pathway (the adenosine A2A receptor) plays a crucial role in cerebral malaria. In experiments with mice, switching off this gene changed survival from 0% to 100%, opening up new possibilities for treating the disease. Although this idea was highly speculative at the start, it challenged existing views of how the immune system works in malaria.

Another project[3] answered a long-standing question in genetics by showing that a particular type of histone (H2A.Z) actively controls how genes are switched on and off

[2] 1000 Ideas Project "A2AR as candidate pathological driver of cerebral malaria", Grant DOI 10.55776/PAT1317523

[3] 1000 Ideas Project "Evolutionary Insights into H2A.Z Function in Gene Regulation", Grant DOI 10.55776/ESP213

across many species, changing how scientists understand how genomes are regulated and evolve.

Another project[4] overlaid historical cadastral maps with high-resolution satellite imagery and used AI-driven analysis to identify areas where human-made features, such as buildings, roads, or agricultural plots, had disappeared over time. The AI system was trained to detect traces of past human activity, comparing them to surrounding landscapes that remained untouched. The goal was to uncover subtle signals, such as variations in soil color, minor elevation changes, or specific patterns, that hinted at historical human presence. This approach aimed to create a powerful tool for archaeologists, allowing them to pinpoint potential archaeological sites by recognizing complex, nearly imperceptible signatures in satellite and aerial imagery.

Technological experimentation and paradigm shifts

Other projects demonstrate the program's role in supporting technically risky and methodologically unconventional experimentation. A project[5] developing an electronic ion pump for localized chemotherapy in glioblastoma pursued an untested drug delivery concept using organic electronics in live models. Despite major technical obstacles, the project achieved proof-of-concept results showing superior tumor inhibition compared to standard approaches, leading to follow-up research integrating immunotherapy. Likewise, a quantum physics project[6] measuring entangled photon states in the rotating frame of the Earth successfully observed quantum Sagnac phase shifts, pushing the frontier of measurement sensitivity and opening new research directions at the intersection of quantum mechanics and general relativity.

Another project[7] focused on childhood and gender in prehistoric societies by using sex-specific proteins in dental enamel to determine the biological sex of children, a group previously indistinguishable through skeletal analysis. Researchers applied nanoLC-MS/MS technology to analyze **ancient** teeth, enabling near-nondestructive sex identification for the first time. The data revealed critical insights into sex-specific mortality, health disparities, and burial practices, clarifying whether boys or girls faced unequal risks of disease, malnutrition, or cultural neglect. By establishing sex-specific growth curves, the team improved age-at-death assessments for Bronze Age populations and traced when societies began assigning gendered roles to children. The work not only reshaped understanding of ancient social structures but also pioneered new methods in paleoproteomics to explore past lifestyles and diseases.

Using unconventional methods to solving fundamental problems

Other projects highlight the used unconventional methods to approach fundamental problems. For instance, a project[8] set out to study optimal structures in high-dimensional spaces. While no universally optimal arrangement exists in three dimensions, research suggests the hexagonal lattice in two dimensions, where particles form regular hexagons, may hold this rare property. To address this, the project employed an unconventional

[4] 1000 Ideas Project "Reading the Past from the Surface of the Earth", Grant DOI 10.55776/TAI591

[5] 1000 Ideas Project "ELPHI: A Next Generation Device for Brain Cancer Treatment", Grant DOI 10.55776/TAI245

[6] 1000 Ideas Project "Influence of Earth´s rotation on entangled photon pairs", Grant DOI 10.55776/TAI483

[7] 1000 Ideas Project "Sex-based differences in Bronze Age childcare", Grant DOI 10.55776/TAI759

[8] 1000 Ideas Project "Universal Optimality of the Hexagonal Lattice", Grant DOI 10.55776/TAI6

approach, leveraging sampling theorems from signal processing, a method typically used in wireless communication. By reformulating the optimality problem as a sampling challenge, the team introduced innovative mathematical tools, bridging fields like quantum physics and information theory. This not only advanced the understanding of geometric arrangements but also opened new avenues for exploring fundamental questions at the intersection of pure mathematics and applied sciences.

Source: Project end reports of 1000 Ideas awardees and interviews

To conclude, the evidence indicates that the 1000 Ideas program has been successful in enabling the implementation of particularly original and high-risk research ideas. At the application level, the program attracts a pool of proposals that are self-identified as novel and unconventional. At the outcome level, awardees report generating critical follow-up questions and new conceptual directions, and these perceptions align with bibliometric indicators of higher interdisciplinarity, novelty, and disruption among projects with stronger self-reported claims of breakthrough potential. Econometric analysis further demonstrates that funded 1000 Ideas projects show a higher dispersion of citation outcomes than both unsuccessful applicants and funded projects in the PI program, consistent with a high-risk/high-reward research profile. Combined with qualitative evidence showing that many projects build new methodological capabilities, challenge conservative fields, and develop emerging research areas, the findings indicate that while few projects can yet be labelled fully "groundbreaking," the program is demonstrably laying the foundations for transformative research trajectories in the way a high-risk, early-stage funding instrument is intended to do.

3.1.2 Jury and Board perspectives on judging high-risk, high-gain ideas

Interviews with FWF Board and Jury members indicate that assessing high-risk, high-reward proposals is an uncertain and demanding process. Reviewers described the 1000 Ideas call as receiving a wide range of proposal quality, ranging from highly speculative or weakly prepared submissions to genuinely original and ambitious ideas. Lower entry barriers and short applications shift much of the burden from applicants to reviewers, who must rapidly distinguish between ideas that are risky but meaningful and those that are implausible.

Board members emphasized that the program is designed to accept uncertainty but not to fund proposals that lack any internally coherent logic. High risk, in this sense, is acceptable when paired with high potential gain. As one Board member put it:

"We are not funding it because it is risky, but despite it being risky — and because the payoff is big."

Judging this balance is particularly challenging in interdisciplinary and unfamiliar domains. Reviewers noted that ideas outside their own expertise often appear more exciting but also harder to evaluate, whereas proposals closer to their own field are assessed more critically because their limits are more visible. The absence of reviewer exchange and the reliance on only two pre-reviewers further increase uncertainty. In this context, partial randomization is recognized as a necessary corrective - once proposals pass a plausibility threshold, distinguishing among them becomes increasingly difficult, making transparent randomness preferable.

The difficulty of distinguishing between "bold but meaningful" and implausible ideas, especially in interdisciplinary and unfamiliar domains, illustrates a selection environment in which uncertainty is structurally embedded. This aligns with the survey evidence of high perceived novelty and risk, the interview accounts of exploratory research trajectories, and the

econometric finding that funded 1000 Ideas projects show greater outcome variance than comparable PI projects. In this sense, the challenges described by reviewers are not a flaw of the system but an expression of the program's attempt to operate at the frontier between the known and the unknown.

3.2 Failure as a sign of program success

In funding programs that explicitly aim to support high-risk, high-reward research, the possibility of failure is not only unavoidable but an integral part of the program logic. Rather than indicating poor performance, instances of failure may signal that genuinely exploratory and uncertain research pathways are being pursued. This section, therefore, examines how failure is understood and experienced within the 1000 Ideas program and to what extent it can be interpreted as an indicator of alignment with the program's objectives.

In this evaluation, "failure" is not defined as the absence of outputs or poor performance, but is understood, in line with the survey questions and qualitative evidence, as the non-realization of the originally proposed research approaches, hypotheses, or empirical strategies. It primarily refers to situations in which projects could not be implemented as planned due to technical, methodological, data-related, or conceptual obstacles, or because initial assumptions did not hold.

This section draws on interview and survey evidence (at both application and project implementation stages) and qualitative responses from awardees to assess how failure is perceived, experienced, and managed within the 1000 Ideas program, and to what extent observed patterns of failure are consistent with a high-risk/high-reward funding logic. **Overall, the evidence suggests that failure is widely anticipated. It happens, and is frequently reframed as a productive and informative component of exploratory research.**

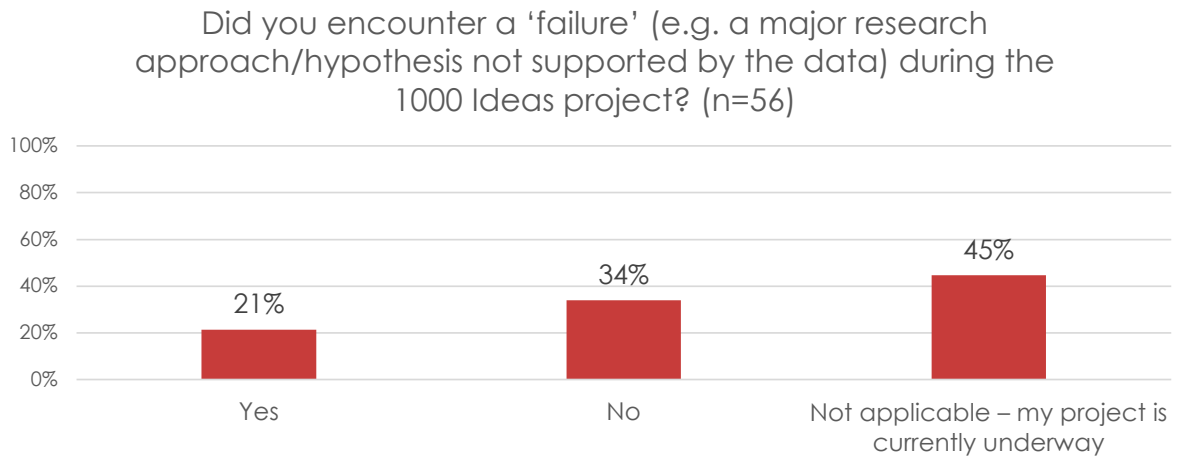
Survey responses indicate that **both awardees and unfunded applicants perceive their project ideas as involving substantial risk, including a significant possibility of failure**. Among awardees, a majority (51%) strongly agreed, and a further 29% somewhat agreed that their project presented a risky idea with a significant chance of failure, resulting in 80% overall agreement. This confirms that, for awardees, funded projects are widely understood to entail a meaningful risk of non-success, consistent with the program's objective of supporting high-risk research.

Awardees were more likely than unfunded applicants to strongly agree that their project carried a significant risk of failure (51% compared to 40%), while overall agreement levels were similar (80% among awardees versus 82% among unfunded applicants). This suggests that funded projects may have been perceived by applicants as involving a higher degree of risk intensity. In the context of a program explicitly designed to enable high-risk, exploratory research, **the presence and acceptance of a substantial perceived failure risk among funded projects can be interpreted as an indicator that the program is operating in line with its intended logic**, where the possibility of failure is an inherent and expected feature rather than a sign of poor performance.

In terms of project implementation, survey responses indicate that **failure is a notable but not dominant feature of 1000 Ideas projects** to date. Around one-fifth of respondents (21%) reported having encountered a failure during their project, reflecting the inherently uncertain and experimental nature of the research supported by the program. A further 34% indicated that they had not experienced a failure, while 45% reported that the question was not applicable because their project was still underway. Taken together, these findings are consistent with the program's high-risk orientation - failure is present for a share of projects and

should be interpreted as an expected and potentially informative outcome in a funding scheme designed to support exploratory and high-uncertainty research.

Figure 14 Incidence of research 'failure' during 1000 Ideas projects



Source: Technopolis survey of 1000 Ideas program awardees

Qualitative explanations provided by respondents show that **"failure" in the context of the 1000 Ideas program reflects the inherent uncertainty and adaptive learning processes characteristic of high-risk research**. Reported failures were frequently linked to unforeseen technical constraints, data access problems, or methodological limitations that only became apparent during implementation and could not reasonably have been anticipated at the proposal stage. In many cases, these challenges prompted substantial revisions to research designs, methods, or technologies, with several respondents emphasizing that initial approaches did not work but ultimately led to alternative strategies, reoriented research questions, or unexpected and valuable results. Importantly, a recurring theme is that respondents often reframed apparent failures as productive changes - unsuccessful experiments, abandoned designs, or partial implementation were described as generating new insights, confirming the risky nature of the original idea, or enabling more robust or innovative outcomes than originally envisaged. Interviews strongly echo this pattern. Several PIs described how their initial hypotheses or planned research paths turned out to be wrong, but this did not stop the project; instead, it led to different and sometimes more promising directions:

"The basic hypothesis was correct. The path to get there was completely different. Then you try it out. It was completely different than expected."

This pattern reinforces the interpretation of failure not as an indicator of weakness, but as an integral and, in some cases, generative element of a funding scheme explicitly designed to support exploratory, high-uncertainty research. Quotes from survey responses below further illustrate these points.

We were failing on so many levels, yet we could finally make things work. The knowledge about the high-risk character of the funding scheme put us at great liberty, which I thoroughly enjoyed.

In a certain sense I got nearly everything wrong what I had written in my proposal. The conducted research in the end was somehow closer to previous work than expected, but on

the other hand produced highly surprising results extending the field in an unexpected direction.

1000 Ideas program awardees survey responses

While a share of respondents reported encountering failure or were still in the process of testing high-risk approaches, subsequent responses indicate that such outcomes were often accompanied by valuable scientific insights. Survey responses indicate that **projects frequently generated scientific value even when initial hypotheses or research approaches were not supported**. Among respondents for whom the question was applicable, nearly two-thirds agreed that their project produced valuable insights for the scientific community or their research field, with almost half (48%) strongly agreeing and a further 17% somewhat agreeing. Notably, no respondents expressed disagreement with this statement, underscoring a shared perception that apparent "failures" often yielded meaningful learning, redirection, or clarification of research boundaries. This pattern reinforces the interpretation of failure within the 1000 Ideas program as a productive outcome rather than a negative one, consistent with the program's emphasis on exploratory research and its explicit tolerance for uncertainty.

In the interviews, this understanding was strongly reinforced. Awardees rarely described their projects as "failures" in absolute terms, even when core hypotheses or approaches did not work as planned. Instead, they framed such outcomes as methodological or conceptual learning processes. As one PI put it:

"It was a learning success but not a publishing success. We couldn't answer the scientific question because the methodology isn't advanced enough. But we made progress with the methodology."

This highlights that in the context of high-risk research, failure is often experienced not as the absence of results, but as the redirection of research pathways based on what did not work.

The evaluation also explored the potential wider negative effects of the "failure", in particular, publication opportunities. The survey responses indicate mixed views on whether the high-risk/high-reward nature of 1000 Ideas projects makes publication more difficult, particularly in cases of negative, null, or inconclusive findings. Around one-third of respondents (34%) agreed that publishing such results is challenging, including 12% who strongly agreed, suggesting that a significant minority experience barriers associated with the exploratory and uncertain nature of their research. However, nearly half of respondents (49%) disagreed with the statement, with a notably large share (31%) strongly disagreeing, indicating that many awardees do not perceive publication difficulties as a major constraint despite the high-risk profile of their projects. The remaining 17% expressed neutral views.

Several awardees emphasized disciplinary differences - while respondents in the humanities and social sciences noted that "failure" is less clear-cut because research almost always yields interpretable outcomes, applicants in experimental and technical fields described negative or inconclusive results as common but difficult to publish. Across fields, respondents repeatedly cited conservative publication practices, reviewer gatekeeping, and resistance to speculative or controversial claims as major barriers, especially when results contradicted prevailing assumptions or did not yield clear positive findings. Negative results were perceived as scientifically valuable but largely unrewarded by high-impact journals, often requiring either reframing, pairing with positive findings, or submission to alternative journals.

Overall, the findings suggest that while publication challenges are a real concern for some high-risk projects, particularly where outcomes are inconclusive, this is not a universal experience among 1000 Ideas awardees. There is some indication that existing academic

publishing norms are not aligned with the program's high-risk, exploratory objectives but perceptions vary considerably depending on project characteristics, disciplinary norms, and publication strategies.

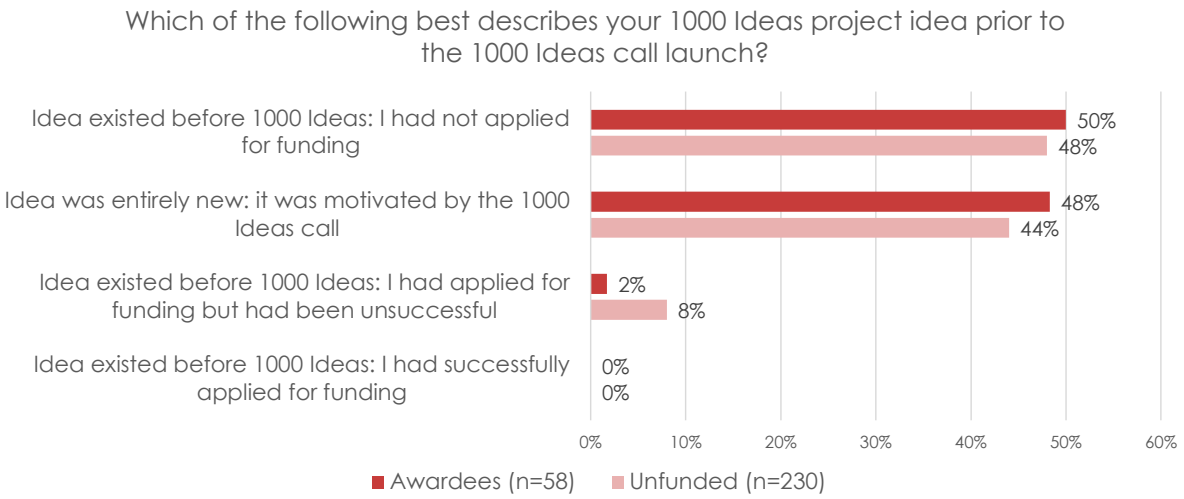
Taken together, the evidence indicates that **failure within the 1000 Ideas program functions less as a negative outcome and more as a core operational feature of its high-risk/high-reward design.** Failure is widely anticipated at the application stage, encountered by a meaningful minority of projects during implementation, and frequently interpreted by awardees as a source of learning, redirection, and methodological innovation rather than as wasted effort. Interview evidence reinforces this interpretation by showing that many projects evolve away from their original plans, gain methodological or conceptual advances, and face publication delays or resistance precisely because they challenge established approaches. Importantly, **most projects reporting failure also report generating valuable scientific insights, suggesting that the program succeeds in shifting research activity into more uncertain but productive space.**

3.3 Additionality and Follow-On Funding Outcomes of the 1000 Ideas Program

This section assesses the additionality of the 1000 Ideas program by examining whether it enables research that would not otherwise have taken place, and whether it leads to distinctive outputs, follow-on funding, and longer-term behavioral changes. Evidence is drawn from multiple sources, including survey data from awardees and unfunded applicants (capturing application-level counterfactuals and motivations), ResearchFish monitoring data (outcome-level effects), and econometric analysis of bibliometric indicators providing causal evidence on post-award research trajectories.

Survey evidence from both awardees and unfunded applicants indicates that the **1000 Ideas program predominantly attracts research ideas that would not otherwise have been pursued through conventional funding instruments**. Among awardees, nearly half (50%) reported that their project idea already existed but had not previously been submitted for funding, while a further 48% indicated that the idea was entirely new and motivated by the 1000 Ideas call. A very similar pattern is observed among unfunded applicants, with 48% reporting pre-existing but previously unfunded ideas and 44% reporting entirely new ideas. In both groups, only small minorities indicated that their ideas had previously been submitted unsuccessfully to other schemes (2% of awardees and 8% of unfunded applicants), and none reported prior successful funding. This suggests that the program attracts exploratory and early-stage ideas that researchers perceive as unsuitable or premature for standard funding channels.

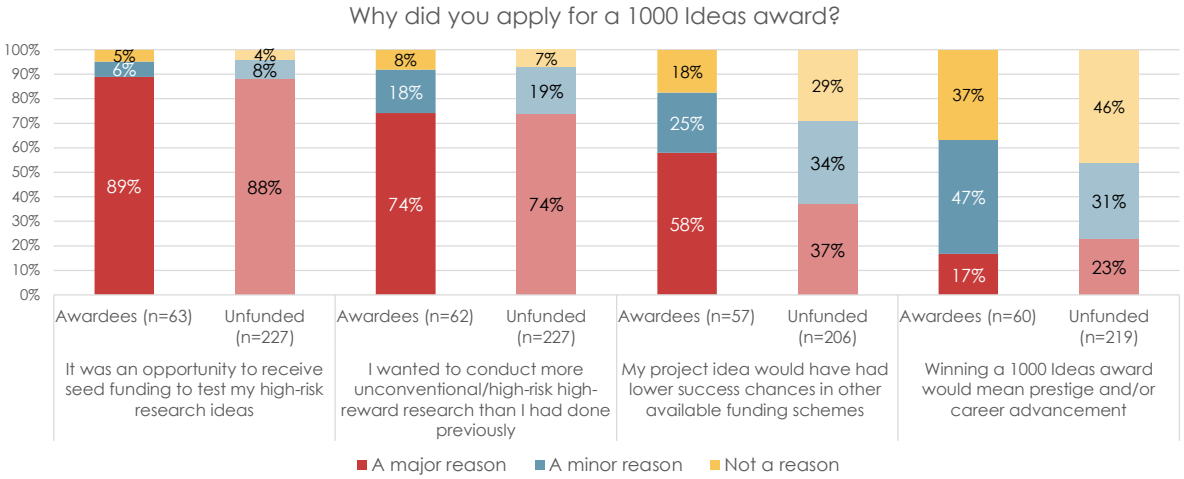
Figure 15 Origin of project ideas among 1000 Ideas Awardees and Unfunded Applicants



Source: Technopolis surveys of 1000 Ideas program awardees and unfunded applicants

Applicants' stated motivations further reinforce this interpretation. For both awardees and unfunded applicants, the opportunity to receive seed funding to test high-risk research ideas was by far the most important reason for applying, cited as a major reason by 89% of awardees and 88% of unfunded applicants. Similarly, around three-quarters of both groups reported a desire to conduct more unconventional or high-risk, high-reward research than they had previously undertaken. While awardees were somewhat more likely than unfunded applicants to report that their project would have had lower chances of success in other funding schemes (58% compared to 37%), this factor remained salient for a substantial share of applicants overall. In contrast, considerations related to prestige or career advancement played a comparatively minor role for both groups.

Figure 16 Reasons for applying to the 1000 Ideas program

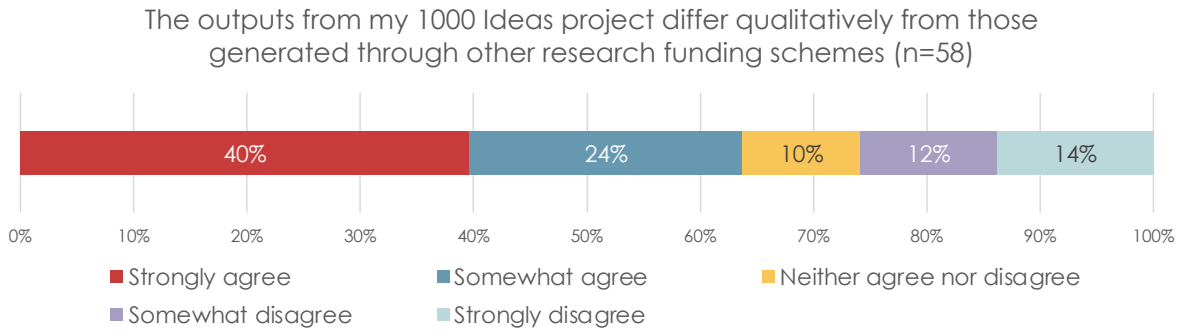


Source: Technopolis survey of 1000 Ideas program awardees and unfunded applicants

Survey responses also provide indicative evidence of output additionality, with a majority of awardees perceiving the outputs of their 1000 Ideas projects as qualitatively different from those typically generated through other research funding schemes. Around two-thirds of respondents agreed with this statement, including 40% who strongly agreed, suggesting that for many funded researchers the program enabled forms of research output that they consider distinct from those produced under conventional funding instruments. At the same time, a non-

negligible minority reported neutral or negative views, indicating that such qualitative differences were not experienced uniformly across projects.

Figure 17 Awardees' assessment of whether 1000 Ideas project outputs differ qualitatively from those of other research funding schemes



Source: Technopolis survey of 1000 Ideas program awardees

Awardees who reported differences in outputs most frequently characterized their 1000 Ideas results as more exploratory or speculative and as making more methodological or conceptual contributions (both 63%), as well as exhibiting a higher level of innovation or novelty (61%). A substantial share also highlighted greater uncertainty or variability in outcomes (43%) and weaker alignment with established publication channels (27%), reinforcing the interpretation that the program supports research trajectories that deviate from conventional expectations and entail higher risk alongside the potential for distinctive contributions.

Follow-on funding is a key outcome in the program's intervention logic, signaling the transition from exploratory outputs to more mature research trajectories. **The 1000 Ideas program awardees have secured follow-on funding, and the program has a good follow-on funding potential**, even if concrete outcomes are still emerging for many projects. While 14% of awardees report having already secured follow-on funding to build on the results of their 1000 Ideas project, a large majority (81%) indicate ongoing or planned efforts to do so. This includes 19% who are actively applying for follow-on funding and a further 62% who plan to apply in the future, reflecting the exploratory nature of many funded projects and the time lag typically associated with translating high-risk research into fundable next steps. Only a small minority (5%) reports neither having secured nor intending to seek follow-on funding.

Follow-on funding associated with 1000 Ideas projects is substantial in monetary terms. Among the 12 awardees who reported having already secured follow-on funding, the total amount received exceeds €6.2 million. In addition, 16 respondents reported actively seeking follow-on funding amounting to more than €7.4 million in total. ResearchFish data provides an even more optimistic picture, with €47.5 million in secured funding reported[9].

[9] The discrepancy between survey-reported and ResearchFish-reported follow-on funding is likely driven by differences in reporting scope, interpretation, and incentives. While the survey asked respondents to estimate funding directly attributable to their 1000 Ideas project and relied on self-assessment at a single point in time, ResearchFish allows for cumulative and ongoing reporting and may include broader funding streams only partially linked to the original project. In addition, ResearchFish entries are often completed by institutional administrators and may capture larger collaborative grants or downstream funding that individual researchers do not necessarily attribute solely to the 1000 Ideas project.

Both realized and anticipated follow-on funding typically exceeds the original 1000 Ideas grant size by a considerable margin, indicating strong leverage effects and supporting the interpretation of the program as an effective catalyst for subsequent, larger-scale research funding.

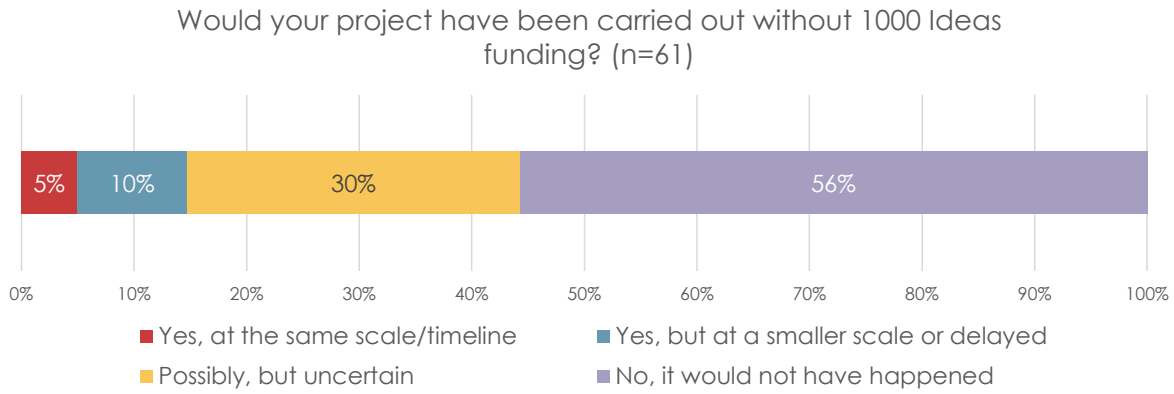
Respondents describe the program as a first step for subsequent applications to major national and European funding schemes, including FWF stand-alone projects, ERC grants, EIC Pathfinder calls and Horizon Europe proposals. This aligns with ResearchFish data, where FWF, ERC and Austrian Research Promotion Agency are most frequently reported and most significant in value sources of follow-on funding. Several examples highlight how exploratory results, preliminary data, samples, or proof-of-concept work generated under 1000 Ideas were instrumental in shaping more mature, large-scale proposals. At the same time, some respondents noted that changing research trajectories, time constraints, or strategic shifts limited immediate application, showing that follow-on effects are not always linear.

For most awardees, the program functions as intended as a seed funding mechanism that lays the groundwork for subsequent funding applications.

In terms of longer-term behavioral effects, the survey results indicate a **strong behavioral additionality effect of the 1000 Ideas program on awardees' future research orientation**. A clear majority of respondents (90%) reported that receiving 1000 Ideas funding increased their willingness to pursue unconventional or risky research in the future, with more than half (57%) indicating a significant increase. Only a small minority (10%) reported no change in their willingness to engage in high-risk research. This finding suggests that the program not only supports individual high-risk projects but also has a broader confidence- and capability-building effect, encouraging researchers to continue exploring unconventional ideas beyond the funded project. In this sense, 1000 Ideas appears to contribute to longer-term shifts in researchers' attitudes towards risk-taking, reinforcing its role as a catalyst for exploratory research trajectories. Behavioral changes among awardees, such as increased confidence to pursue risky research, are early indicators of a broader system effect - researchers oriented toward exploration.

Taken together, the preceding evidence on follow-on funding, behavioral change, and output characteristics is reinforced by applicants' counterfactual assessments of whether their projects would have gone ahead without 1000 Ideas support. The survey responses provide strong evidence of **high additionality** associated with the 1000 Ideas program. A clear majority of awardees (56%) report that their project **would not have been carried out at all** without 1000 Ideas funding. A further 30% indicate that the project might have happened, but only with considerable uncertainty, suggesting that the program reduced risk and enabled earlier or more decisive pursuit of novel ideas. Only small minorities report that the project would have proceeded anyway, either at the same scale and timeline (5%) or at a smaller scale or with delays (10%).

Figure 18 Additionality of the 1000 Ideas program

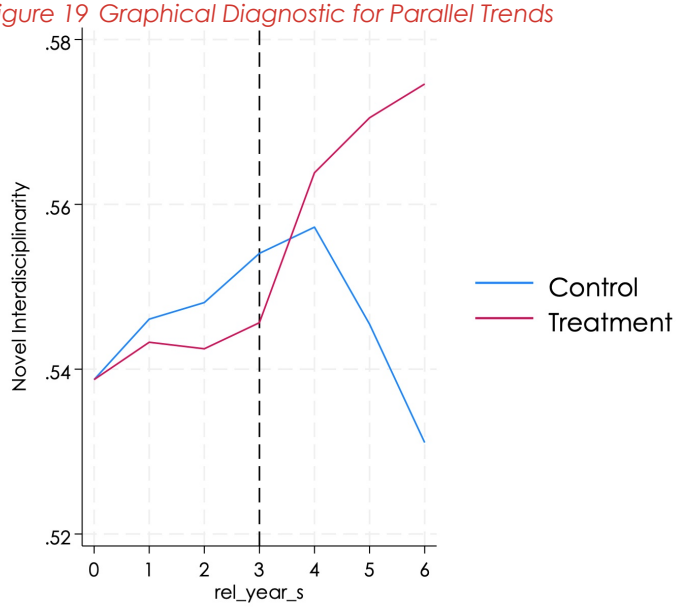


Source: Technopolis survey of 1000 Ideas program awardees

In addition to self-reported counterfactual assessments, at the outcome level, the evaluation also draws on econometric analysis of longitudinal bibliometric data to estimate the causal impact of 1000 Ideas funding on research outcomes (see Appendix C for full details on econometric analysis). Using a Difference-in-Differences (DiD) design, the analysis compares funded and unfunded applicants before and after the funding decision, thereby isolating post-award effects while controlling for pre-existing differences and common time trends.

This approach allows the evaluation to move beyond perceptions reported in the survey and assess whether 1000 Ideas funding leads to systematically different research trajectories compared to what would have occurred in the absence of funding. Across outcomes, the estimates indicate limited average post-award effects on citation-based impact, disruption, cosine-distance novelty, and patent citations. By contrast, **novel interdisciplinarity** exhibits a positive and statistically significant treatment effect, with funded 1000 Ideas projects showing a higher post-award increase in interdisciplinarity relative to unsuccessful applicants. Figure 19 shows parallel pre-award trends between funded and unfunded applicants for novel interdisciplinarity. At the same time, the figure shows a clear post-award divergence, with funded applicants exhibiting stronger growth, providing an initial visual indication of program effects.

Figure 19 Graphical Diagnostic for Parallel Trends



This pattern is consistent with the program's exploratory and early-stage focus. While high-risk research can ultimately lead to highly influential publications, such effects typically materialize over longer time horizons. In the short term, 1000 Ideas is more likely to affect the direction and composition of research, such as the novel combination of disciplines, than to generate immediate citation- or patent-based impacts.

Overall, the evidence provides strong support for high additionality of the 1000 Ideas program. At the application level, most funded projects would not have been pursued without the program, indicating substantial input additionality. At the outcome level, awardees report qualitatively different, more exploratory forms of output and significant leverage into larger follow-on funding, while econometric analysis shows that 1000 Ideas causally shifts research trajectories towards greater interdisciplinary recombination. In addition, the program generates clear behavioral additionality by increasing researchers' long-term willingness to pursue risky and unconventional ideas.

3.4 Other outcomes

The evaluation also examines a broader set of research outputs and outcomes reported through ResearchFish and reflected in open-ended survey responses. These data provide insight into other forms of activity and impact, as well as into overall output volumes.

The ResearchFish data indicates that 1000 Ideas projects have generated diverse outputs, with a strong emphasis on early-stage knowledge development and research networking rather than applied or commercial outputs. Understandably, publications are the most frequently reported output (224 in total). High counts of engagement activities (93) and collaborations and partnerships (46) suggest that many projects were active in community building, interdisciplinary exchange, and interaction with broader stakeholder groups. Outputs typically associated with later-stage or application-oriented research, such as patents, software products, or licensing, are rare, which aligns with the short duration, limited scale, and high uncertainty of the funded projects.

Table 8 Count of outputs reported in ResearchFish

Outcome	Count
Collaborations & Partnerships	46
Engagement activities	93
Further funding	22
Influence on policy	9
Postdoc theses	2
PhD theses	3
Publications (all)	224
Publications (ref)	121
Research data	27
Research tools	14
Software/technical products	1
Patents/licensing	1
Artistic and creative products	9

Source: ResearchFish

Beyond direct funding outcomes, the survey comments also point to important capacity- and career-related effects. Postdoctoral researchers developed independent research profiles, helped to establish new research profiles, secured further positions or longer-term posts, and leveraged the project to build new collaborations or participatory research activities.

3.5 Output Profiles of 1000 Ideas Projects in Comparison to FWF Principal Investigator Projects

This section examines how the output profile of 1000 Ideas projects compares with that of FWF Principal Investigator (PI) program projects, in order to assess whether the program generates a distinct type of research output consistent with its high-risk, high-reward objectives. The primary evidence is an econometric analysis of longitudinal bibliometric indicators, complemented by qualitative evidence from open-ended survey responses and interviews with the PIs, enabling both perception-based and causal comparisons of post-award research trajectories across funding programs. Together, these sources allow the evaluation to assess not only how researchers experience the differences between programs but also whether systematic differences in output patterns can be observed empirically.

Across the open-ended responses to the survey of awardees (n=24), PIs consistently emphasize the high perceived value and additionality of the 1000 Ideas program, particularly in enabling research that would not otherwise be pursued. Many respondents frame the program as uniquely suited to supporting high-risk, high-reward and exploratory research, often contrasting it positively with more conventional funding instruments such as FWF stand-alone projects (the PI program), which are seen as better suited for resource-intensive work.

In terms of comparative output additionality, the evaluation applied econometric Difference-in-Differences (DiD) and Difference-in-Difference-in-Differences (DDD) models to compare post-award research outcomes of successful 1000 Ideas projects with those of successful FWF Principal Investigator (PI) projects (see full details in Appendix C). The approach controls for pre-award differences, allowing for a causal comparison of output trajectories across funding schemes.

The results show no robust average post-award effects of 1000 Ideas funding on citation-based impact, disruption, or semantic novelty relative to PI projects. In contrast, novel interdisciplinarity shows a positive and statistically significant effect, indicating that 1000 Ideas projects experience a stronger post-award increase in interdisciplinary recombination than standard PI projects.

Estimated effects for patent and policy citations are weak and not consistently robust across models, suggesting that the program does not systematically generate more applied or policy-oriented outputs than conventional funding instruments within the observed time window.

These findings indicate that the main distinctive output profile of the 1000 Ideas program lies not in higher short-term impact or visibility, but in the structural characteristics of the knowledge produced. The program fosters stronger interdisciplinary recombination, consistent with its role as an exploratory, high-risk seed funding instrument aimed at opening new research directions rather than maximizing immediate scientific or technological returns.

Robustness tests show that these results are not driven by differential publication behavior or sample selection, supporting the interpretation that observed differences reflect genuine program effects rather than artefacts of data construction.

OECD analyses[10] of high-risk/high-reward funding stress that these programs should be assessed primarily on their capacity to generate exploratory knowledge and open new research trajectories, rather than on short-term output or impact metrics. The econometric results for 1000 Ideas align closely with this thinking, showing effects concentrated in interdisciplinary recombination, while citation and patent-based impacts, typically associated with later stages of research, currently remain limited.

The evidence indicates that the distinctive contribution of the 1000 Ideas program lies less in generating higher short-term impact and more in shaping the nature of knowledge production itself. Compared with standard PI projects, 1000 Ideas projects do not, on average, achieve higher citation, disruption, or applied impact in the short term, but they do exhibit significantly stronger interdisciplinary recombination, consistent with an exploratory and experimental research logic. This suggests that 1000 Ideas fulfils a complementary role within the FWF portfolio by opening new conceptual and disciplinary pathways rather than optimizing established research trajectories. In line with international policy perspectives on high-risk funding, the program's added value is therefore best understood in terms of directionality and diversity of research, rather than immediate performance on conventional output metrics.

Interview evidence reinforces the interpretation that the output profile of 1000 Ideas projects differs from that of conventional PI funding in structurally important ways. Awardees consistently described their 1000 Ideas projects as more exploratory, less milestone-driven, and

[10] https://www.oecd.org/content/dam/oecd/en/publications/reports/2021/05/effective-policies-to-foster-high-risk-high-reward-research_131670a3/06913b3b-en.pdf

less tightly oriented toward predefined publication outputs than their other funded research. Rather than optimizing for specific deliverables, projects were described as spaces for methodological experimentation, learning, and adaptive re-orientation as new findings emerged. One PI characterized the experience as being "let off the leash", allowing researchers to explore the idea rather than adhere to a fixed research plan. This greater freedom was seen as enabling more dynamic, curiosity-driven and responsive research processes, which participants contrasted with the micro-planning and compliance requirements typical of larger PI projects. In this sense, the qualitative difference between 1000 Ideas and standard PI funding lies not primarily in the volume of outputs, but in the way knowledge is produced. 1000 Ideas supports exploratory, process-oriented research in which methods, learning, and conceptual reframing are often more important than immediate publication outcomes.

4 Process evaluation

This section examines how the design and implementation of the 1000 Ideas program function in practice and how applicants experience the key elements of the funding process. It focuses on two main aspects: first, overall satisfaction and experiences with the application and assessment process, including clarity, efficiency, and perceived quality of evaluation; and second, a more in-depth analysis of the program's distinctive process innovations, anonymization and partial randomization, and their role in risk-taking, and the achievement of the program's objectives.

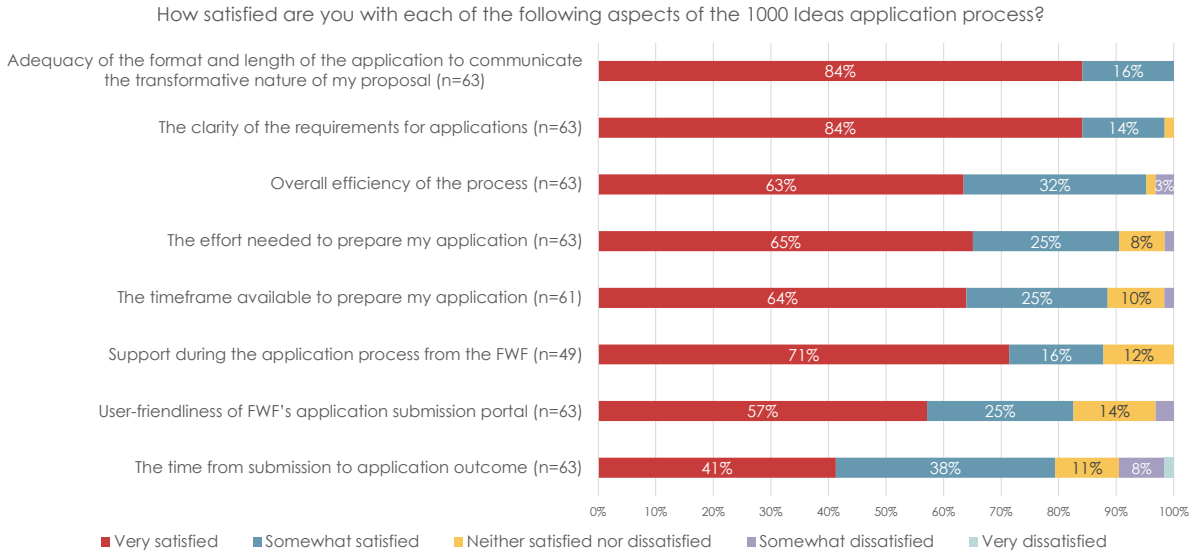
4.1 Overall experience with the application process

4.1.1 Overall satisfaction

This section examines applicants' experiences with the 1000 Ideas application process, drawing on survey responses from both awardees and unfunded applicants and interviews with funded PIs. It focuses on satisfaction with key aspects of the process, including application format, clarity of requirements, efficiency, and timelines, in order to assess how well the process supports the program's objectives.

Survey responses indicate **generally high levels of satisfaction among awardees with most aspects of the 1000 Ideas application process**, particularly those related to the suitability of the application format for communicating unconventional research ideas. More than four-fifths of awardees reported being very satisfied with both the adequacy of the format and length of the application (84%) and the clarity of the application requirements (84%). Satisfaction was also high with respect to the effort required to prepare an application and the timeframe available for preparation, with around two-thirds of awardees reporting being very satisfied. While views on overall process efficiency and the user-friendliness of the submission portal were somewhat more mixed, the majority of awardees still expressed positive assessments. The time from submission to application outcome shows lower levels of satisfaction, suggesting some scope for improvement in decision timelines.

Figure 20 Awardees' satisfaction with the application process



Source: Technopolis surveys of 1000 Ideas program awardees

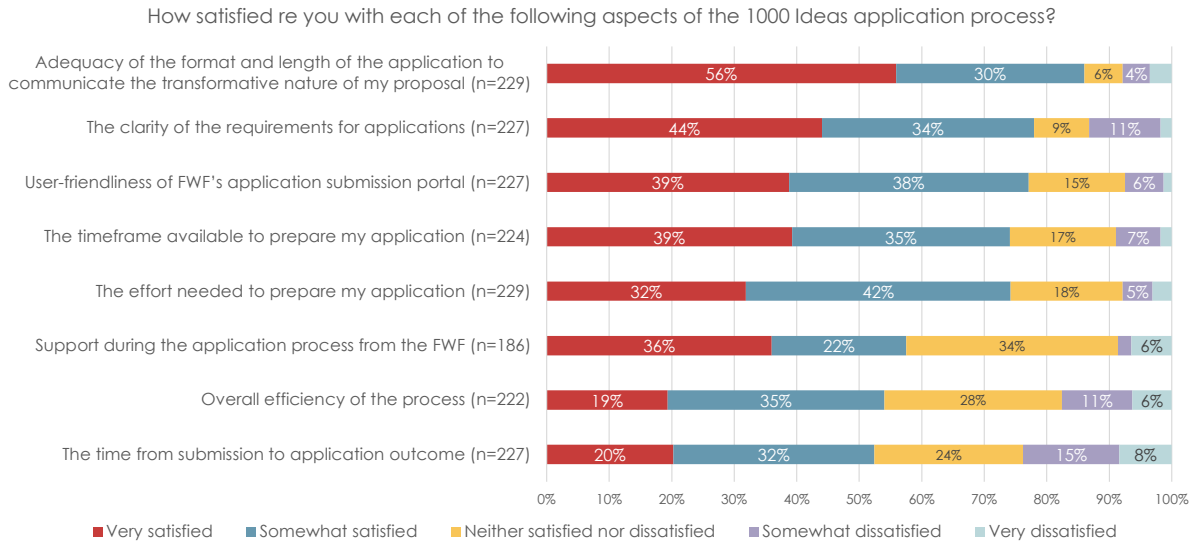
This high level of overall satisfaction with the application process is also supported by interviews with PIs. The interview partners described the process as clearly communicated. The requirements of only 7-page applications and no need to tailor the CVs to demonstrate necessary capacities were described as adequate for the small amount of funding that can be granted in this funding scheme.

"We were really satisfied. […] I always hoped for a process like this. We had more freedom and the application process was relatively straightforward. The application required limited resources and the reporting was very pleasant."[11]

1000 Ideas program awardees interview quote

Unfunded applicants were also broadly positive about the application process, though their satisfaction levels were consistently lower than those of awardees across all dimensions. In particular, fewer than half of unfunded applicants reported being very satisfied with the clarity of requirements, the application format, or the user-friendliness of the submission portal, with a sizeable share expressing neutral or negative views. Perceptions of overall process efficiency and the time to decision were notably less favorable among unfunded applicants, with only around one-fifth reporting high satisfaction and a minority expressing dissatisfaction. Support from the FWF during the application process also emerged as a more polarizing aspect for unfunded applicants, with over one third indicating neutral views.

Figure 21 Unfunded applicants' satisfaction with the application process



Source: Technopolis surveys of 1000 Ideas program unfunded applicants

Taken together, the findings suggest that while the application process is generally regarded as fit for purpose, especially in enabling applicants to communicate unconventional ideas, there are clear differences in experience between funded and unfunded applicants.

[11] German original: „Wir waren wirklich zufrieden.[…] Ich habe mir immer gewünscht, dass es läuft wie das hier gelaufen ist. Man war freier und es war eine relative unkomplizierte Antragsstellung, Ressourcenaufwendung war wenig für Antrag, Berichtswesen sehr angenehm".

Differences in satisfaction between awardees and unfunded applicants may partly reflect outcome-related perception effects, whereby successful applicants tend to evaluate the process more positively, while unsuccessful applicants adopt a more critical perspective. Such differences are common in competitive funding programs and should therefore be interpreted with caution, alongside substantive feedback on specific process elements.

Beyond differences in satisfaction with the application process, the survey data also provide insight into how funded and unfunded applicants perceive the fit of their projects with conventional funding schemes, which helps to contextualize some of the observed differences in applicant experiences and the role of processes in achieving the program objectives. Survey data show that **awardees were somewhat more likely than unfunded applicants to report that their project would have had lower chances of success in other funding schemes** (58% compared to 37%). A quote from an interview with one of the funded PIs illustrates the typical challenges and how these were not relevant with the 1000 Ideas program.

"You always have to come with strong preliminary experiments to have a chance. If you don't have that... [it becomes difficult]. For the 1000 ideas [project], the basic experiments 'You can do this [...]' were enough. No other funding body would have supported us. Also, you have to be an expert in the field. [And that means] being an expert in both [fields]. It gets difficult when you apply a new technology to a new disease."[12]

1000 Ideas program awardees interview quote

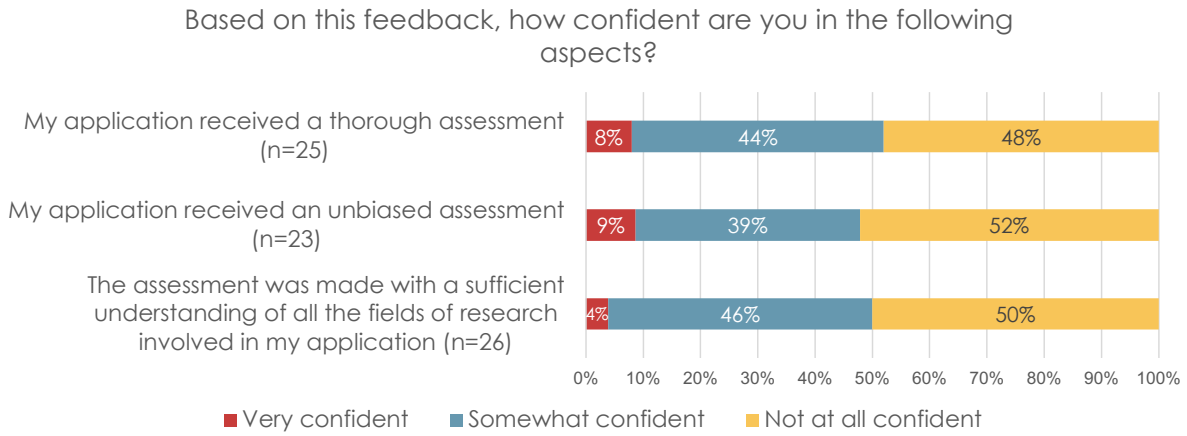
This pattern is consistent with the program's objective of supporting research ideas that fall outside the scope of conventional funding instruments and suggests that the selection process may have been effective in identifying projects with a particularly high-risk or unconventional profile. While this finding is based on self-reported perceptions and may be influenced by hindsight or justification effects among awardees and should therefore be interpreted with caution, it nonetheless provides indicative evidence that funded projects were, on average, perceived by applicants themselves as less compatible with standard funding schemes than those that were not selected.

4.1.2 Perceptions of assessment quality and feedback among unfunded applicants

Survey responses from unfunded applicants indicate **mixed and often low confidence in the quality of the assessment underlying the feedback they received**. Fewer than one in ten respondents reported being very confident that their application received a thorough or unbiased assessment, while around four in ten expressed only moderate confidence. Notably, around half of respondents indicated that they were not at all confident that their application had been assessed thoroughly, without bias, or with a sufficient understanding of all relevant fields involved in their proposal. These findings suggest that, from the perspective of unfunded applicants, the assessment process, at least as reflected in the feedback received, did not consistently convey a sense of depth or interdisciplinary competence.

[12] German original: „Man muss immer mit starken Vorexperimenten kommen um eine Chance zu haben haben. Wenn das nicht da ist… [wird es schwierig]. Für das 1000 Idee [Projekt] haben die grundlegenden Experimente "Man kann das […]"gereicht. Das hätte uns kein anderer Fördergeber gefördert. Man muss sonst auch noch der Experte sein auf dem Feld. [Und zwar] für beides Experte sein. Wenn man eine neue Technologie auf eine neue Krankheit anwendet ist das schwierig.

Figure 22 Unfunded applicants' view on feedback received



Source: Technopolis survey of 1000 Ideas program unfunded applicants

In the context of a program designed to support unconventional, interdisciplinary, and high-risk research, these perceptions are particularly salient. While some degree of dissatisfaction among unfunded applicants is common in competitive funding schemes, the relatively high levels of low confidence reported here point to potential challenges in how assessment decisions and feedback are communicated. This underscores the importance of ensuring that assessment procedures and feedback mechanisms clearly demonstrate expertise, careful consideration, and fairness, particularly when innovative elements such as anonymization or partial randomization are used and may already complicate applicants' understanding of how decisions are reached.

The survey responses show how unfunded applicants respond to an unsuccessful outcome and how they position the 1000 Ideas program within their broader funding strategies. Around **one third of respondents (33%) report that they have reapplied or would reapply to 1000 Ideas with the same idea**, indicating a relatively high willingness to engage repeatedly with the program despite strong competition and low success rates. This points to both sustained confidence in the program's objectives and an acceptance among applicants that multiple submissions may be necessary in an oversubscribed scheme. At the same time, a sizeable share of respondents indicates alternative trajectories - 37% report that they would not pursue the idea further and would instead focus on other activities, while a further 31% would seek alternative funding either by reducing the novelty or risk profile of the idea, submitting it largely unchanged, or scaling down its scope. Taken together, these patterns reflect the high application pressure facing the program and suggest that repeated submissions are a rational response to intense competition, while also highlighting a degree of filtering, whereby some high-risk ideas are either adapted to fit more conventional funding schemes or abandoned altogether following an initial unsuccessful attempt.

Among applicants who were unfunded at the lottery stage and chose to resubmit, the survey data indicate considerable variation in how proposals were adjusted but overall suggest **relatively moderate levels of revision**. Just under one-fifth of respondents (17%) reported resubmitting the exact same proposal without changes, while a further 24% made only superficial revisions. The largest group (29%) undertook some revisions, indicating limited but non-trivial adaptation in response to the previous outcome. Notably, 21% reported reapplying with a completely different idea, suggesting that for a substantial minority, the initial rejection prompted a strategic reorientation rather than incremental refinement. Only a small share

(10%) invested in extensive revisions. Taken together, these patterns are consistent with the logic of a lottery-based selection process, where rejection is only weakly informative about proposal quality, applicants appear less inclined to invest heavily in major revisions and instead either resubmit with minor changes or pivot to a different idea, reinforcing the interpretation of repeated submissions as a rational response to high application pressure and probabilistic selection.

4.1.3 Adequacy of Funding Size and Duration for High-Risk Research

Open-ended survey responses reveal mixed views regarding the size and duration of the 1000 Ideas award, reflecting a tension between its role as exploratory seed funding and the practical demands of high-risk research. While many respondents considered the funding amount substantial relative to typical seed grants and sufficient to initiate unconventional lines of inquiry, others emphasized that the budget and timeframe were often too limited to fully realize ambitious or methodologically complex projects.

Several survey respondents noted that establishing new methods, infrastructure, or interdisciplinary collaborations requires significant upfront investment, and that limited resources sometimes constrain the scope of empirical work or delay the generation of publishable results. At the same time, some respondents argued that the modest scale of the award is appropriate given the program's experimental nature and high uncertainty, suggesting that its primary value lies in enabling proof-of-concept research and positioning projects for larger follow-on funding rather than delivering fully mature outcomes.

Moreover, some interview partners (awardees/FWF Board members) criticized the funding size and duration of the projects. The interviews show that projects that used experiments encountered difficulties in collecting, analyzing, and publishing data as planned. This is particularly true when they had only limited experience with the methods/organisms/samples used in the project. Some projects spent the entire period addressing methodological challenges and managed to collect data but not analyze it. Moreover, interview partners note that the funding would need to be adjusted to the particularly high inflation in Austria.

These findings suggest that while the size and duration of the 1000 Ideas awards do impose constraints for some projects, particularly those involving complex experimental set-ups or unfamiliar methods, these limitations must be interpreted in light of the program's role as high-risk seed funding. The program is not designed to deliver fully mature research outputs within a single grant, but to enable researchers to test daring ideas, establish feasibility, and generate the preliminary results and expertise needed for subsequent, larger-scale funding. Within this logic, the majority of projects appear to operate as intended, with concerns about time and resources reflecting the inherent tension between exploratory ambition and small-scale funding rather than a systematic mismatch. Moreover, the more critical views expressed in interviews represent important but relatively isolated experiences, underscoring the need for careful expectation management rather than a fundamental redesign of the funding model.

4.1.4 Overall conclusions on satisfaction with program processes

Overall, the evaluation finds that the 1000 Ideas application process is widely perceived as fit for purpose and well aligned with the program's high-risk, exploratory objectives**,** particularly among funded applicants. Awardees express very high satisfaction with the application format, clarity of requirements, and proportionality of effort, consistently highlighting the short, focused proposal format and reduced emphasis on track record as key strengths that enable unconventional ideas to be articulated effectively. Interview evidence reinforces this picture, with PIs valuing the low administrative burden and the perceived openness of the process compared to conventional funding schemes.

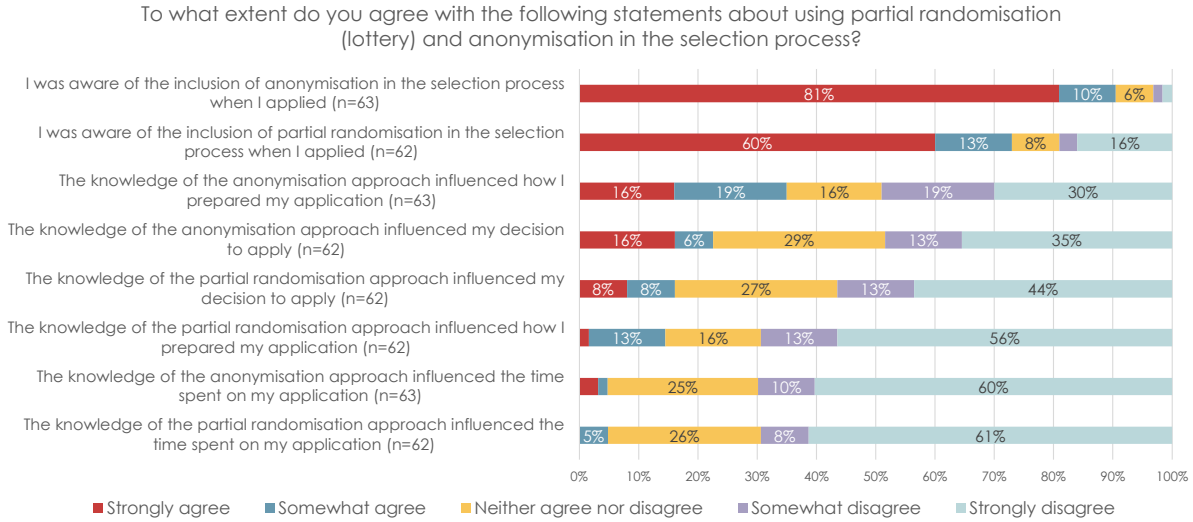
At the same time, satisfaction levels among unfunded applicants are systematically lower, particularly regarding assessment quality, feedback, decision timelines, and process transparency. While some degree of outcome-related perception bias is likely, the relatively high levels of low confidence reported by unfunded applicants point to communication challenges in conveying assessment decisions and feedback, particularly in a context where anonymization and partial randomization complicate applicants' understanding of how outcomes are determined. The evidence suggests that, although the process is broadly effective in supporting the program's strategic aims, further improvements could be made to feedback mechanisms, transparency of decision-making logic, and selected administrative aspects (such as deadlines and data management requirements) to strengthen perceived legitimacy, especially among unsuccessful applicants.

4.2 The role of partial randomization and anonymization

4.2.1 Awareness and influence on the proposal

Survey results indicate that **awareness of the innovative process elements, particularly anonymization and partial randomization, was generally good, with lower awareness about randomization**. Among awardees, 81% reported being aware of the use of anonymization and 60% of partial randomization, while corresponding figures among unfunded applicants were slightly lower but still substantial (77% and 48%, respectively). Awareness of anonymization in the selection process was high among both awardees and unfunded applicants. In contrast, awareness of the partial randomization (lottery) element was notably lower, with only 60% of awardees and 48% of unfunded applicants reporting that they were aware of this feature at the time of application. Given that partial randomization is a central and distinctive element of the program's selection logic, this level of awareness appears modest and suggests that communication of this aspect of the process may not have been fully effective.

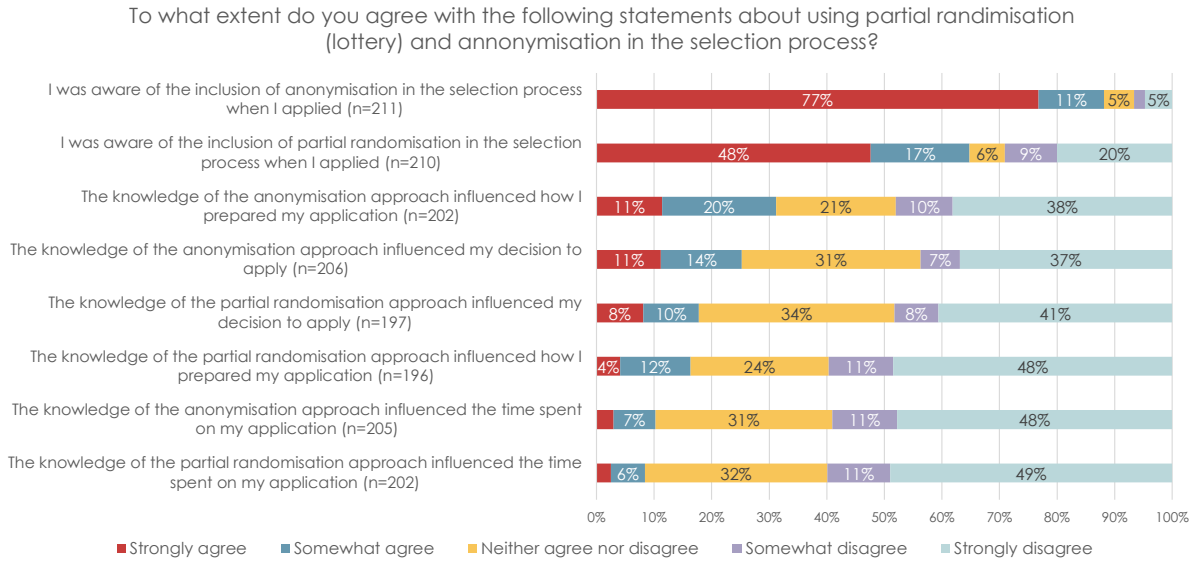
Figure 23 Awardees' views on randomisation and anonymization



Source: Technopolis survey of 1000 Ideas program awardees

These results are also validated in the interviews, where all interview partners were aware of the anonymization element, which was not the case for the lottery element. The anonymization has a more direct impact on the applicants as they do not have to provide CVs, and applications are rejected for formality issues if they are not sufficiently anonymized. Not all interview partners were aware of the randomization, and if they were, they were also not sure about the details of the process.

Figure 24 Unfunded applicants' views on randomization and anonymization



Source: Technopolis survey of 1000 Ideas program unfunded applicants

The survey data show that **knowledge of anonymization and partial randomization had only a limited influence on applicants' reported behavior**. This was also validated in the interviews with funded awardees. For both awardees and unfunded applicants, only small minorities indicated that these elements influenced their decision to apply or how they prepared their application. In particular, a majority of respondents in both groups disagreed that partial randomization affected their decision to apply or the way they prepared their proposal, with especially high levels of strong disagreement regarding its influence on time spent on the application. However, these findings need to be interpreted in light of the relatively modest awareness of the lottery element, especially among unfunded applicants. Where applicants were not fully aware of, or did not fully internalize, the role of partial randomization in the selection process, its limited behavioral impact may reflect low salience rather than a considered assessment of its relevance. Anonymization appears to have had a slightly greater, though still modest, influence, particularly on application preparation, which may be partly explained by higher levels of awareness and clearer implications for how proposals are written.

The interviews point to a shared perception among PIs, but also FWF board members, that anonymization is most relevant for establishing post-docs. All of the interviewed post-docs highlighted that they think their applications have benefited from an anonymous review, in which the idea was judged rather than the person conducting the project. As broadening the research portfolio is essential during the post-doc phase for securing stable academic positions, anonymous applications are seen to leap their capacities to establish themselves as experts in new fields. However, interview partners involved in the selection process noted that the use of anonymization further increases the overall risk profile of the program, as it limits the assessment of feasibility and the applicants' capacity to implement the proposed research.

"I think the question is most relevant for early-career researchers/PIs, who may experience discrimination in the funding system. It is also easier for them to maintain anonymity than it is for established researchers"[13]

1000 Ideas program awardees interview quote

Overall, the findings suggest that while selection process innovations did not substantially alter applicant behavior, their influence may have been constrained by uneven awareness and understanding among applicants.

International comparison: Applicant response to randomization in funding programs

Comparable evidence from other programs that use partial randomization suggests that such mechanisms often have limited influence on applicant behavior**, particularly the time spent and approach taken when preparing proposals. The clearest program-level survey evidence comes from the** Health Research Council of New Zealand's Explorer Grants**, where a survey of applicants found that most respondents reported the lottery** did not change the approach **they took to the application (69%) and** did not change the time spent **preparing it (75%)**[14]**. These results are broadly consistent with the 1000 Ideas findings that partial randomization has only a modest reported effect on application behavior.**

The 1000 Ideas finding of only moderate awareness of the lottery element (particularly among unfunded applicants) is important for interpretation: where awareness and understanding are incomplete, the observed limited behavioral impact may partly reflect low salience rather than a considered judgement that randomization is an irrelevant consideration when applying.

Taken together, the evidence suggests that anonymization and partial randomization were largely perceived as background features of the selection process rather than as decisive or behavior-shaping elements.

4.2.2 Overall attitudes towards randomization and anonymization

Survey responses from both awardees and unfunded applicants indicate that **anonymization is generally perceived more positively than partial randomization**. Across both groups, **anonymization is most strongly associated with enhanced equal opportunities and the prevention of conflicts of interest and unconscious bias**, with most respondents selecting these attributes. Anonymization is also seen as contributing, to a lesser extent, to improved opportunities for risky research and thematic diversity. Negative attributes, such as an increased risk of selecting low-quality projects or reduced reputation gains, are rarely associated with anonymization, suggesting broad acceptance of this mechanism as a fairness-enhancing feature of the assessment process. The survey responses in that regard were also validated in the interviews.

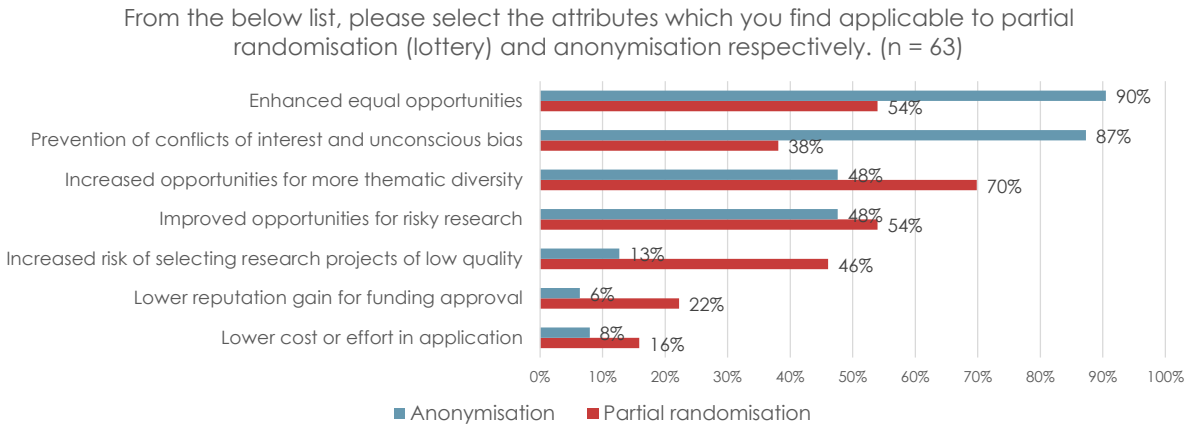
Views on partial randomization are more polarized. While **a substantial share of respondents associates the lottery element with improved opportunities for risky research and greater thematic diversity**, many also link it to potential downsides. In particular, around half of

[13] German original: „Ich glaube am wichtigsten ist die Frage für early-career, Forschende/ PIs, die vlt die Erfahrung haben benachteiligt zu sein in dem ganzen [Fördersystem]. Für die ist es auch einfacher die Anonymität zu wahren als für die Etablierten."

[14] https://link.springer.com/article/10.1186/s41073-019-0089-z?utm_source=chatgpt.com

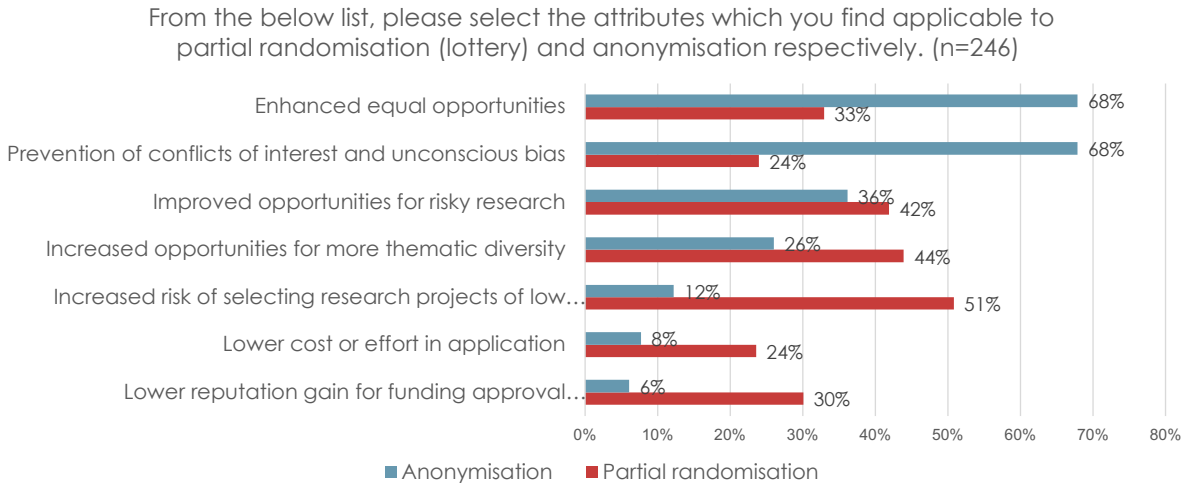
unfunded applicants and nearly half of awardees selected **"increased risk of selecting research projects of low quality"** as an attribute of partial randomization, and a sizeable minority associated it with lower reputation gains from funding approval. At the same time, partial randomization is less strongly associated with fairness-related attributes, such as preventing bias or conflicts of interest, than anonymization. This suggests that while respondents recognize the potential of randomization to broaden the range of funded research, concerns remain about its implications for perceived quality and prestige.

Figure 25 Awardees' views on partial randomization and anonymization



Source: Technopolis survey of 1000 Ideas program awardees

Figure 26 Unfunded applicants' views on partial randomization and anonymization



Source: Technopolis survey of 1000 Ideas program unfunded applicants

International comparison: Applicant perceptions of randomization in funding programs

Evaluation of the Volkswagen Foundation's "Experiment!" program, **which also employed partial randomization show strong associations between the lottery element and** enhanced equal opportunities and bias reduction, **alongside substantial concerns about** the risk of

selecting lower-quality projects and reduced reputation gains[15]. **This pattern closely resembles the 1000 Ideas findings, where respondents link partial randomization both to opportunities for risky or diverse research and to potential quality- and prestige-related trade-offs.**

Survey results indicate that both partial randomization and anonymization are viewed as more appropriate and acceptable within the specific context of the 1000 Ideas program than for other grant types, reflecting an understanding among respondents that these process innovations are particularly suited to funding high-risk and unconventional research and can be used for a relatively small award size. Anonymization enjoys especially strong support, with 85% of unfunded applicants and 97% of awardees regarding it as an acceptable method for allocating 1000 Ideas funding, compared to lower, though still substantial, levels of support for its use in other grant schemes.

In their open response comments, many respondents emphasized that **anonymization is particularly appropriate for high-risk, exploratory funding such as 1000 Ideas, as it shifts attention toward the research idea itself**, helps mitigate biases related to reputation, seniority, gender, or institutional affiliation, and enables researchers, both early-career and established, to venture into new or unconventional research directions. This is also validated in the interviews.

Some open-ended survey responses point to a broader perceived relevance of the assessment innovations introduced in the 1000 Ideas program beyond the specific program context. A number of respondents argued that these mechanisms could usefully be extended to FWF's PI program, which they perceive as already containing substantial elements of implicit randomness due to reviewer selection, disagreement, and subjective judgment. From this perspective, introducing explicit randomization after a quality threshold has been met is seen as a more transparent and potentially fair way of allocating funding among equally strong proposals, especially in highly competitive programs where small differences in review scores can have large consequences.

Survey responses indicate that **maintaining anonymity in the project description was generally perceived as manageable.** Among awardees, a large majority (84%) agreed that maintaining anonymity was simple, with over half strongly agreeing, suggesting that for funded applicants, the anonymization requirements were largely workable in practice. Two-thirds (67%) of unfunded applicants agreed that anonymization is manageable; however, they were considerably less likely to strongly agree and more likely to report difficulties, with roughly one-fifth expressing some level of disagreement. This may reflect differences in disciplinary context, proposal maturity, or retrospective perceptions shaped by funding outcomes. Overall, while the findings support the feasibility of anonymization within the 1000 Ideas program, they also indicate that for a non-negligible share of applicants, particularly among those not funded, maintaining anonymity posed practical challenges. Open answers as well as interview results reveal that this might be especially in small fields, at advanced career stages, or where proposals necessarily build on prior work and track records that make identification likely. Moreover, interview partners mention that they trust in the FWF's capacity to select appropriate reviewers and that the reviewers who feel biased would refrain from judgment.

[15] https://www.volkswagenstiftung.de/sites/default/files/documents/Fo%201%2B2%202020_Roebbecke_Simon_0.pdf?utm_source=chatgpt.com; https ://repository.fteval.at/id/eprint/767/1/5718_Simon.pdf?utm_source=chatgpt.com

Several comments stressed that **for larger or more conventional grants, information on applicants' qualifications, track record, and institutional context is essential for assessing feasibility**, suggesting that anonymization may be less suitable outside exploratory funding schemes.

The interviews of reviewers showed that their understanding of whether it is in practice possible to maintain full anonymity differed depending on the field and their anchoring in the Austrian system. While there are certainly "fingerprints" from certain groups, infrastructure or model organisms that certain groups in Austria use, the 1000 Jury has limited involvement in the Austrian ecosystem, and these do not affect Jury decision-making. Furthermore, the Jury has established an explicit rule to not look up people online, trying to find out more about the applicant. However, the FWF Board members are part of the same research communities in Austria and if fields that are very small it may be difficult not to guess at least the institution. The jury chair expressed concerns, as the first filtering step is completed by people from the Austrian system.

"A lot of the [1000 Ideas Jury] panel is not in the Austrian Ecosystem. My bigger concern is the board members. They are more likely to know the personalities. It seems to me that the outside world is told that decisions are made by an international panel. To try counteract the known problems of people having friends. […] [But the] filtering step [is done] by the Board, and so it is not quite true that process is completely through the international panel. [But] our panel would not be eager to get all the proposals. I can understand for pragmatic reason."

"[Anonymization is] not possible in my field. There are seven others who can do the things I can do. So you can work it out for yourself. Once I guessed right and once I was wrong. [So it is] not totally anonymous."[16]

Quotes from interviews with reviewers

Partial randomization is also broadly accepted within the 1000 Ideas program context, with 57% of unfunded applicants and 81% of awardees viewing it as acceptable for 1000 Ideas, but it is **more contested when considered for wider application across other funding instruments**. Qualitative responses from awardees as well as interview partners reveal polarized views on partial randomization, with many respondents framing it as a **pragmatic and even corrective response to perceived shortcomings of conventional peer review**. A recurring theme is the view that traditional selection processes already contain a substantial element of randomness, given reviewer disagreement, grade inflation, and inconsistent outcomes across resubmissions.

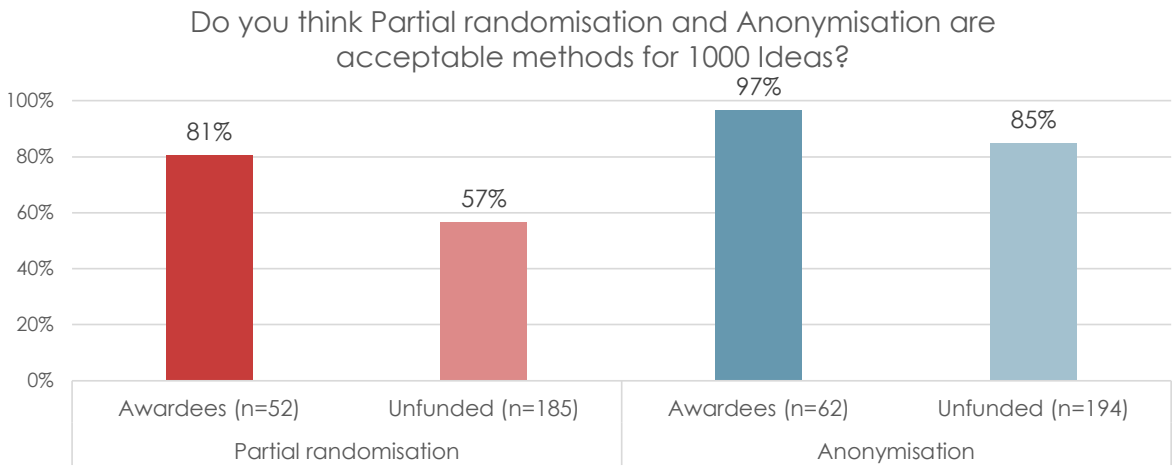
In this context, partial randomization, when applied only after proposals have passed a clearly defined quality threshold, is seen by many respondents as a fairer and more transparent way of allocating scarce funding, particularly for thematically open, high-risk calls such as 1000 Ideas.

At the same time, a minority of respondents expressed principled reservations or outright opposition to the use of randomization in research funding. These concerns centered on the **perceived incompatibility of lotteries with notions of scientific excellence, responsibility for the use of public funds, and the substantial intellectual effort required to prepare grant applications**.

[16] German Original: "In meinem Fachbereich nicht möglich. Den Dingen, die ich kann, da gibt es sieben andere. Dann kann man sich ausrechnen. Einmal habe ich es erraten und einmal war ich daneben. Eh nicht so ganz anonym."

Respondents were particularly skeptical about extending partial randomization to larger or more conventional funding schemes, such as the PI projects, where feasibility, track record, and expert judgement were viewed as essential components of quality assurance. Interview partners also mention that revisions done to applications after peer review help to strengthen the quality of the proposals. Including a lottery weakens the importance of peer review. Several comments in the survey and also the consensus of the interview partners, stressed that randomization should never substitute for rigorous expert assessment, but could be acceptable only as a final tie-breaking mechanism once clear distinctions between high- and low-quality proposals have been made.

Figure 27 Awardee and Unfunded applicant views on the use of partial randomization and anonymization



Source: Technopolis survey of 1000 Ideas program awardees

To summarise, **anonymization is widely perceived as an appropriate and largely uncontroversial assessment innovation within the 1000 Ideas program**. It is strongly associated with enhanced equal opportunities and the prevention of conflicts of interest and unconscious bias, and only rarely linked to negative attributes such as lower quality selection or reduced reputation gains. High levels of acceptability, particularly for the 1000 Ideas context, reflect a shared view that anonymization is well suited to high-risk, exploratory funding, where shifting attention away from prior reputation and track record can enable researchers to pursue new or unconventional directions. However, it faces practical limitations, notably the difficulty of achieving full anonymity in small fields, at advanced career stages, or where proposals necessarily build on prior work. Therefore, anonymization fits best exploratory or staged funding schemes and can be less suitable for larger or more conventional grants where feasibility assessments depend more heavily on information about the research team and institutional context.

Views on partial randomization are more polarized, combining recognition of its potential benefits with persistent concerns about legitimacy and quality assurance. Many associate the lottery element with improved opportunities for risky research and greater thematic diversity, but a substantial share also link it to an increased risk of selecting lower-quality projects and to lower reputation gains from funding approval. These attitudes closely mirror findings from international evaluations, such as the Volkswagen Foundation's *Experiment!* program. Overall, support for partial randomization is strongly conditional - many regard it as a pragmatic and fair solution to the well-documented inconsistencies and biases of conventional peer review, but only when applied after a clear quality threshold has been met. A minority also expressed principled opposition to the idea of random allocation in research funding, especially for larger

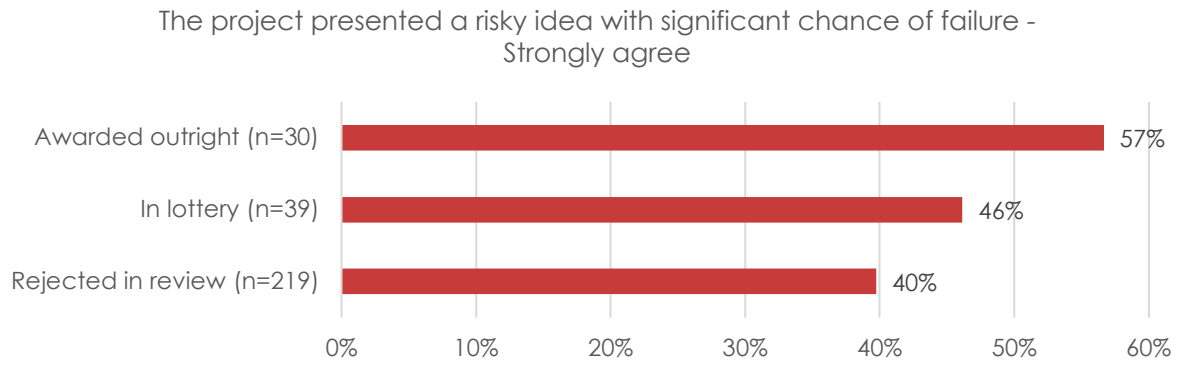
budgets or more conventional grant schemes, arguing that expert judgement and track record remain essential for responsible use of public funds. **Overall, partial randomization is broadly accepted as an appropriate mechanism within the specific context of the 1000 Ideas program, but remains considerably more contested than anonymization and is viewed with greater caution when considered for wider application.**

4.2.3 The Effects of Partial Randomization on Selection Outcomes

This section moves beyond perceptions and attitudes to examine the observable effects of the randomization on the composition and outcome profile of funded projects.

Survey evidence provides a first indication that the selection procedure differentiates proposals along the intended risk dimension. Figure 28 shows the share of applicants who strongly agreed that their project involved a risky idea with a significant chance of failure, disaggregated by selection outcome. Proposals awarded outright report the highest perceived risk (57%), followed by those entered to the lottery (46%), while proposals rejected before partial randomization report substantially lower levels (40%). This suggests that the lottery functions within a pool of already high-risk proposals rather than across the full applicant population.

Figure 28 Share of applicants strongly agreeing on the riskiness of their idea by review outcome



Source: Survey of 1000 Ideas awardees

The econometric analysis examined **whether randomization alters selection decisions by reducing reliance on conventional performance indicators and increasing the weight placed on characteristics aligned with the program's exploratory objectives**, such as novelty, disruption, and societal relevance. Identification exploits variation in lottery exposure among fundable proposals, allowing the influence of pre-award characteristics on funding success to differ under randomized versus discretionary selection.

To this end, logit models of funding success are estimated that interact standardized pre-award indicators with an indicator for lottery selection. Coefficient estimates are complemented by average marginal effects to aid interpretation in terms of changes in funding probabilities. Together, these models provide direct evidence on whether the lottery mechanism alters selection gradients in line with an intent to reduce reliance on established performance metrics and broaden the set of funded research trajectories.

The tables below report estimates from logit models of funding success that allow the effect of pre-award characteristics to differ depending on whether a proposal is subject to the lottery component. All continuous predictors are standardized (z-scores), and standard errors are clustered at the applicant level. Table 9 reports logit coefficients and interaction terms; Table 10 reports average marginal effects (AMEs), expressed as changes in the probability of

funding associated with a one-standard-deviation increase in each predictor, separately by lottery status.

Across specifications, Table 9 shows that selection gradients differ between proposals decided under discretionary evaluation and those decided under lottery conditions. Under non-lottery selection, success is more strongly aligned with conventional or conservative signals, while novelty-oriented dimensions tend to be weakly selected against or not rewarded. **Under lottery selection, these patterns shift: the association between past citation impact and success is attenuated, and novelty-related dimensions exhibit a more positive relationship with funding.**

The AME results make these differences more transparent in Table 10. For proposals not subject to lottery selection, a one-standard-deviation increase in novelty-related indicators is associated with lower or unchanged funding probabilities. By contrast, **among proposals in the lottery component, the estimated marginal effects for interdisciplinarity and disruption are positive and statistically significant, consistent with randomization reducing reliance on conventional performance signals and increasing the relative prominence of exploratory characteristics at the selection stage.** Effects for cosine distance and policy citations are less precisely estimated, but the direction of estimates is generally aligned with this interpretation.

Partial randomization mechanism changes how observable proposal characteristics relate to funding decisions. In particular, **the lottery reshapes selection by weakening the role of established impact metrics and increasing the likelihood that applicants with more novel past research profiles are funded.**

Table 9 Logit estimates of funding success with lottery interactions (selection-stage effects)

Logit regressions predicting funding success					
	(1)	(2)	(3)	(4)	(5)
Successful Application					
Field-Weighted Citation Impact	-0.029				
	(0.169)				
Selected in Lottery=1 # Field-Weighted Citation Impact	-0.872*				
	(0.493)				
Novel Interdisciplinarity		-0.363**			
		(0.162)			
Selected in Lottery=1 # Novel Interdisciplinarity		0.957***			
		(0.333)			
Disruption Index			-0.067		
			(0.354)		
Selected in Lottery=1 # Disruption Index			3.089		
			(1.889)		

Cosine Distance				-0.358*	
				(0.198)	
Selected in Lottery=1 # Cosine Distance				0.529	
				(0.497)	
Av Policy Citations					-7.224***
					(2.693)
Selected in Lottery=1 # Av Policy Citations					5.611
					(3.927)
N	792.000	780.000	686.000	783.000	792.000

Note: Continuous covariates are standardized (z-scores) using the full estimation sample.
Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.

Table 10 Average marginal effects of pre-award characteristics on funding success, by lottery status

Logit AMEs: Effect of 1-SD increase in predictor on funding probability, by lottery status					
	(1)	(2)	(3)	(4)	(5)
	FWCI"	Novel Interdisciplinarity	Disruption Index	Cosine Distance	Policy Citations
main					
Selected in Lottery=0	-0.002	-0.020**	-0.004	-0.020*	-0.380***
	(0.009)	(0.009)	(0.020)	(0.011)	(0.146)
Selected in Lottery=1	-0.158**	0.105**	0.526*	0.033	-0.309
	(0.066)	(0.051)	(0.276)	(0.087)	(0.570)
N	792.000	780.000	686.000	783.000	792.000
Note: Continuous covariates are standardized (z-scores) using the full estimation sample. Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.					

4.2.4 Conclusions on the role of anonymization and randomization

The evidence suggests that anonymization and partial randomization play a supportive but indirect role in achieving the core objectives of the 1000 Ideas program. Neither mechanism appears to substantially shape applicant behavior at the preparation or application stage. However, this does not imply that these innovations are ineffective; rather, their primary effects operate at the level of selection fairness and portfolio composition rather than individual applicant behavior.

Anonymization emerges as the more robust and widely accepted of the two process innovations. It is strongly associated with enhanced perceptions of fairness, equal opportunities, and bias reduction, and is seen, especially by postdoctoral researchers, as facilitating access to funding for unconventional ideas that might otherwise be disadvantaged by reputation. In this sense, anonymization directly supports the program's objective of widening access to high-risk research and enabling researchers to explore new fields without being constrained by prior track records.

Partial randomization, by contrast, is more contested but remains broadly acceptable within the specific context of 1000 Ideas. While concerns persist regarding perceived quality assurance and legitimacy, many respondents and interview partners view the lottery as a pragmatic and transparent mechanism for allocating funding among proposals that meet a clear quality threshold, particularly in a highly competitive environment.

Survey evidence shows that outright-funded proposals report the highest levels of perceived risk, followed by lottery-funded proposals, while rejected proposals report substantially lower levels.

The econometric analysis provides outcome-level evidence consistent with the intended effects - funded 1000 Ideas projects exhibit a more uncertain and dispersed impact profile than comparable PI program projects, and selection under the lottery places relatively greater weight on novelty-oriented characteristics. **This suggests that the lottery mechanism does reshape selection in ways that favor exploratory research trajectories.**

Taken together, anonymization and partial randomization primarily function as structural enablers of high-risk research, increasing tolerance for uncertainty, reducing reliance on reputation, and broadening the range of funded research directions. The econometric evidence strengthens this interpretation by showing measurable differences in how research characteristics relate to funding outcomes and in the risk profile of funded projects.

5 Conclusions

This concluding section synthesizes the main findings of the evaluation and provides answers to the core evaluation questions in Table 11. Drawing on survey data, interviews, bibliometric and econometric analyses, it assesses the extent to which the 1000 Ideas program has achieved its objectives in supporting high-risk, high-reward research, generating additional and distinctive outputs, and implementing appropriate selection and funding processes. The section also reflects on the program's overall effectiveness as a seed funding instrument and on the suitability of its innovative assessment procedures for identifying and enabling new research trajectories.

Taken together, the evidence shows that the 1000 Ideas program performs well at the output and outcome levels of its intervention logic, generating exploratory knowledge, structural novelty, and substantial follow-on funding. While longer-term impacts such as field-defining publications or new technologies are still emerging, the program is creating the conditions under which such impacts can occur.

The final section of this report (next Section 6) presents key recommendations regarding program design options and adaptations.

Table 11 Headline conclusions on each evaluation question

Evaluation question	Headline conclusion
1: To what extent have particularly original or high-risk research ideas been implemented in the 1000 Ideas program to date, i.e., have some 1000 Ideas projects led directly to groundbreaking new findings or laid the groundwork for such findings?	The evidence indicates that the 1000 Ideas program has been successful in supporting particularly original and high-risk research ideas and in enabling research trajectories that would be unlikely to emerge through conventional funding instruments. At the application level, both awardees and unfunded applicants overwhelmingly self-identify their proposals as novel and unconventional, and awardees consistently perceive their projects as involving substantial risk and uncertainty. At the outcome level, survey responses, project reports, and qualitative examples show that many funded projects have generated fundamentally new concepts, critical follow-up questions, and speculative insights that challenge existing research directions. Quantitative bibliometric and econometric evidence further supports this assessment by showing that the program produces a distinctive high-risk/high-reward outcome profile. While average short-term effects on citation-based impact or applied outputs remain limited, which is an expected pattern for early-stage exploratory funding, the econometric analysis demonstrates that 1000 Ideas projects show higher post-award dispersion in outcomes compared to standard PI program projects, indicating greater uncertainty and variability consistent with HRHR logic. Difference-in-Differences results also show a positive causal effect on novel interdisciplinarity, suggesting that the program shifts research trajectories toward more unconventional combinations of disciplines.
2: Does the program meet the requirements to classify as seed funding for high-risk projects, i.e., i) are these projects unlikely to be funded under conventional funding programs	The evidence strongly suggests that the 1000 Ideas program meets the key criteria for classification as a seed funding instrument for high-risk research. At the application level, both awardees and unfunded applicants report that the large majority of project ideas were either entirely new or had existed previously but had not been submitted for funding, indicating that they were perceived as premature, too speculative, or unsuitable for conventional funding schemes. Awardees were significantly more likely than unfunded applicants to report that their projects would have had lower chances of success in other funding programs, and counterfactual survey responses show that over half of funded projects would not have been carried out at all without 1000 Ideas support. This is reinforced by qualitative evidence from interviews and open-ended survey responses, where PIs consistently describe the program as uniquely enabling

Evaluation question	Headline conclusion
and ii) are the results achieved likely to result in further funding?	exploratory ideas that lack preliminary data, methodological maturity, or disciplinary fit required by standard funding instruments such as FWF PI program. At the outcome level, there is strong evidence that 1000 Ideas funding generates conditions conducive to further funding and confirms seed funding logic. A large majority of awardees report having secured or actively pursuing follow-on funding, with reported follow-on amounts substantially exceeding the original 1000 Ideas grants, indicating significant leverage effects. Qualitative examples show applications to major Austrian and European schemes, including FWF PI program projects, ERC grants, EIC Pathfinder and Horizon Europe. Econometric analysis complements these findings by showing that the program causally shifts research trajectories toward greater interdisciplinarity and produces a higher-variance outcome profile, suggesting that it enables the kind of exploratory knowledge generation that underpins subsequent, more mature funding applications. The evidence indicates that 1000 Ideas functions effectively as a seed funding mechanism, both by supporting research unlikely to be funded elsewhere and by enabling securing larger-scale funding.
3: Are there qualitative differences in the outputs generated in the 1000 Ideas program as compared to FWF's Principal Investigator Projects?	The evidence indicates that there are qualitative differences in the outputs generated by the 1000 Ideas program compared to FWF's Principal Investigator program, particularly in terms of the exploratory character and outputs/knowledge produced. Survey responses from awardees show that a clear majority perceive the outputs of their 1000 Ideas projects as qualitatively different from those typically generated through conventional funding schemes, most frequently describing them as more exploratory, speculative, and conceptually or methodologically oriented. Awardees also highlight higher levels of uncertainty and weaker alignment with established publication channels, suggesting that the program supports research trajectories that deviate from mainstream disciplinary expectations and prioritize idea generation, proof-of-concept work, and agenda-setting over immediate consolidation or exploitation. Econometric evidence based on Difference-in-Differences and Difference-in-Difference-in-Differences models comparing 1000 Ideas and PI projects show no systematic differences in short-term citation impact, disruption, or applied outputs such as patents or policy citations. However, 1000 Ideas projects show higher post-award growth in novel interdisciplinarity, indicating a stronger tendency toward recombining knowledge across disciplinary boundaries. In addition, funded 1000 Ideas projects display greater dispersion in citation outcomes than PI projects. These findings suggest that the distinctive output profile of 1000 Ideas lies not in higher immediate impact, but in enabling different forms of knowledge production characterized by interdisciplinary recombination.
4: Have 1000 Ideas projects failed and how were the failures dealt with?	A proportion of 1000 Ideas projects have experienced forms of failure, and these failures align with the program's high-risk, high-reward logic. Survey data show that around one-fifth of awardees (21%) reported encountering a failure during project implementation. At the application level, both funded and unfunded applicants perceived their project ideas as highly risky, with around 80% of awardees agreeing that their project involved a significant possibility of failure. This indicates that failure is not an unintended side effect, but an anticipated feature of t the program. Qualitative evidence suggests that failures were generally dealt with in a constructive manner. Open-ended survey responses and interviews show that failures typically stemmed from unforeseen technical constraints, data access problems, or methodological limitations that only became apparent during implementation. In many cases, these setbacks led to substantial redesigns of

Evaluation question	Headline conclusion
	research strategies, alternative methodological approaches, or reframing of research questions, often resulting in valuable scientific insights. Many awardees explicitly described failure as a productive learning process rather than a negative outcome, and nearly two-thirds reported that their projects generated valuable insights even when initial hypotheses were not confirmed. Overall, the evidence suggests that failure within 1000 Ideas functions as an integral part of exploratory research, contributing to learning and in some cases to more innovative outcomes than originally envisaged.
5: Is the FWF's selection procedure for this program (including the lottery procedure and double-blind review process) suitable for identifying radically new, particularly original, or high-risk research ideas? To what extent could the procedure be improved?	The FWF's selection procedure for the 1000 Ideas program is suitable for identifying radically new, original, and high-risk research ideas, particularly in comparison to conventional peer review processes. Survey and interview evidence shows that anonymization is widely perceived as enhancing fairness and reducing biases related to reputation, seniority, gender, or institutional affiliation, thereby shifting attention towards the quality and originality of the research idea. This appears especially important for early-career researchers and postdocs, for whom anonymous assessment lowers barriers to proposing unconventional ideas outside established research profiles. Econometric evidence further supports the relevance of partial randomization - analyses of proposals that passed the quality threshold and entered the lottery indicate that under partial randomization, conventional performance metrics (such as past citation impact) play a weaker role in funding success, while novelty-oriented characteristics (notably novel interdisciplinarity and disruption) become more strongly associated with selection outcomes. This suggests that the lottery mechanism increases the likelihood that proposals with unconventional profiles are funded. The evidence also points to several areas for improvement. Awareness of the lottery element is only moderate, particularly among unfunded applicants, suggesting that communication about the role and logic of randomization could be strengthened. While randomization is accepted by most respondents within the specific context of 1000 Ideas, it remains more contested than anonymization, with concerns about perceived quality assurance and legitimacy. Potential improvements therefore include clearer communication about the selection logic, especially the role of randomization; more systematic feedback to unsuccessful applicants to strengthen perceived assessment quality; and consideration of hybrid models that retain anonymization and lottery elements for exploratory schemes while allowing feasibility-related information to be assessed at later stages or in larger funding instruments.
6: Is a separate program necessary to achieve the stated objectives or could they also be integrated into another existing program? What adaptations might be necessary?	The evaluation finds that maintaining 1000 Ideas as a stand-alone program is both necessary and justified in order to achieve its high-risk, high-reward objectives. The program's distinctive design - small, time-limited awards, protection of researchers' careers, tolerance of failure, anonymised assessment, and partial randomisation - creates a protected space in which radically novel and uncertain ideas can be pursued without the feasibility and performance pressures that dominate mainstream funding schemes. While selected elements such as anonymised first-stage screening or limited use of randomisation could beneficially be adapted within other FWF programs, fully integrating 1000 Ideas into standard funding lines would risk weakening its core HRHR logic, weakening its safeguarding mechanisms, and reintroducing conservative selection dynamics. A dedicated stand-alone program therefore remains the most effective way to support exploratory research.

6 Recommendations: Program Design Options and Required Adaptations

6.1 1000 Ideas works best as a stand-alone program

The evidence from this evaluation supports **maintaining the 1000 Ideas program as a stand-alone funding instrument within the FWF portfolio.** A core reason for maintaining 1000 Ideas as a separate program is its acceptance of risk, including the possibility of failure. The program's high-risk focus is reflected in concrete design choices, including relatively small grants, short project durations, the exclusion of PhD positions, and the requirement that PIs hold at least a 50% position at an Austrian research institution. These features allow scientific risk-taking while limiting risks to the funder (e.g., loss of significant funding amounts) and to researchers' careers. By contrast, mainstream funding programs typically involve larger budgets, longer timeframes, doctoral training, and strong expectations regarding feasibility, which would make it difficult to preserve the 1000 Ideas program's approach if it were integrated.

International experience shows that high-risk/high-reward funding is most effective when ring-fenced in dedicated instruments (e.g. Volkswagen Foundation's *Experiment!*, ERC Proof-of-Concept, HRC New Zealand Explorer Grants), rather than embedded as features within conventional schemes.

Across survey responses, interviews, and outcome analyses, the 1000 Ideas program emerges as a distinct intervention. Many survey respondents emphasized the importance of retaining the program in its current form, describing it as a unique program for exploratory research that does not fit the logic of standard competitive grants.

The program's process innovations, anonymization and partial randomization also support stand-alone implementation. Evidence from this evaluation indicates that these mechanisms reshape selection outcomes and portfolio characteristics in ways consistent with the program's objectives. At the same time, anonymization increases program risk by limiting feasibility assessment and weakening reliance on track record. This trade-off is appropriate in a small, experimental program but would be more problematic in programs that provide larger grants.

From an outcome perspective, the program's seed-funding role is clearly evidenced. Most funded ideas would not have been pursued under conventional schemes, and a substantial share of projects have already led, or are expected to lead, to follow-on funding from larger Austrian and European funding instruments. In monetary terms, the volume of secured and anticipated follow-on funding exceeds the original 1000 Ideas grant amounts, indicating leverage effects. This suggests that a relatively small, time-limited investment at an early stage can unlock significantly larger research resources once feasibility has been established. Econometric analysis shows that research results in novel interdisciplinary combinations and higher outcome variance. Together, these findings indicate that the program operates most effectively within a dedicated funding framework that tolerates uncertainty and failure. It would be difficult to sustain these conditions in mainstream funding instruments.

To conclude, the evidence indicates that the effectiveness of 1000 Ideas is linked to its stand-alone design. Its objectives, risk profile, selection mechanisms, and outcomes would be difficult to preserve into other funding programs.

6.2 Required adaptations

Overall, the evaluation finds that the 1000 Ideas program is well aligned with its objectives and widely valued by awardees. At the same time, the evidence indicates several areas for improvement that could strengthen the program's effectiveness, transparency, and sustainability.

First, **communication and transparency around selection mechanisms**, particularly partial randomization, could be improved. While anonymization is well understood and broadly accepted, awareness of the lottery element is more limited, especially among unfunded applicants. Clearer communication about the purpose, timing, and scope of randomization seems to be necessary. FWF could emphasize its role as a fairness-enhancing mechanism applied only after a quality threshold. This could help manage expectations, reduce uncertainty, and improve perceived legitimacy.

International experience (e.g. Volkswagen Foundation's Experiment!) shows that applicants better accept randomization when it is framed as a positive, intentional choice point. For example, the Experiment! Guidance document includes the following language:

> ***During the test phase an equal number of grants (15-20) is drawn in a lottery from the same pool of applications, which meet the program requirements and the quality criteria, under the supervision of the Foundation's legal officer. In this way bias is omitted and ideas that otherwise would be easily overlooked are given a chance. The lottery as a new selection element as well as the entire review process will be evaluated. The grantees will not know whether their idea was received enthusiastically by the scientific jury or whether they were lucky.*** *All projects are treated equally.*

Source: Volkswagen Foundation's Experiment! Guidance for applicants.

The evaluation finds that the FWF's current definitions of high-risk/high-reward and "courage to fail" are coherent and internally consistent, and they differentiate 1000 Ideas program from mainstream funding schemes. To avoid misalignment between expectations and assessment and to encourage reporting of failure, the **FWF may wish to make even more explicit in applicant guidance and in reporting frameworks that learning, redirection and negative results** are valued outcomes of HRHR funding.

Second, **to strengthen the integrity of anonymization while preserving a workable review process, FWF could introduce a more clearly separated and professionally supported pre-screening stage that is explicitly designed to be identity-blind.** The evaluation finds that while anonymization is a core and well-accepted feature of the program, it is partially undermined by the current role of FWF Board members at the filtering stage. International experience from schemes such as the SNSF Spark Fund, the Volkswagen Foundation's "Experiment!" program, and New Zealand HRC Explorer Grants, shows that eligibility and plausibility screening is handled either by program officers or by international panels rather than by actors embedded in the national research system. In the case of 1000 Ideas, shifting the initial filtering of applications away from Austrian Board members toward either FWF staff trained in anonymized triage or a small international pre-panel would reduce the risk of recognition of applicants in small fields, without requiring the jury to read all submissions.

One option could be to have a pool of external international reviewers assess anonymized applications using a short, structured assessment grid aligned with the program's HRHR logic (e.g. transformative potential, plausibility, learning value in case of failure, clarity), similar to what is currently conducted by the FWF Board. Each application is reviewed independently by

at least two reviewers. Reviews result in scores (1-5 as currently) and/or "advance / do not advance" recommendations. Scores and recommendations are aggregated by the FWF staff and discussed by a small international calibration group or pre-panel, which produces a ranked shortlist. The FWF Scientific Board would retain a governance and oversight role, focusing on process integrity, conflict-of-interest management, and portfolio balance, rather than acting as a scientific filter at this anonymised stage.

Options for the final selection stage

- **Option A – Retain the international jury as the final decision-maker:**
  The international jury reviews only the shortlisted applications and conducts the final scientific assessment and funding decisions. A lottery is retained as a transparent tie-breaking mechanism among equally fundable proposals, consistent with the program's HRHR rationale
- **Option B – Streamlined international decision-making:**
  The international pre-panel is expanded or mandated to act as the final jury, merging shortlisting and final selection into a single international assessment stage. This reduces duplication while preserving anonymity and scientific judgement.

Managing reviewer workload is critical for the feasibility of an international pre-screening stage. This could be addressed through a simple, structured assessment framework for rapid triage rather than full peer review. Reviewers would not be asked to produce narrative reports or detailed justifications, but instead to complete a short assessment grid with a limited number of criteria closely aligned to the 1000 Ideas logic (e.g. transformative potential, plausibility, and learning value in case of failure), using simple scoring scales or "advance / do not advance" recommendations. Such an approach reduces time demands, allowing reviewers to assess a moderate number of applications efficiently. This light-touch screening should be effective in filtering out clearly implausible proposals while preserving genuinely high-risk ideas, without overburdening reviewers or compromising assessment quality. At the anonymised pre-assessment stage, feedback should be limited to clearly defined categories indicating why proposals did not advance to jury review. This approach provides transparency without implying that rejection reflects correctable weaknesses.

Third, **feedback from unfunded applicants** requires attention, noting that only a small subset of applicants received any feedback. Survey evidence indicates low confidence among unfunded applicants who received feedback on the depth and fairness of assessments, partly reflecting limited feedback. Feedback after the jury stage should be framed as structured orientation and not detailed feedback. Rather than narrative critiques, applicants would benefit from information on how their proposal was positioned in the selection process (e.g. fundable but not selected due to randomisation), combined with a simple criterion-referenced overview.

Fourth, **the timing and frequency of calls** could be better aligned with the nature of high-risk research. Several interviewees highlighted that long intervals between calls and challenging deadlines are difficult in fast-moving areas. Introducing more frequent submission windows, or two calls per year, could improve meeting the applicants' needs. Also, applicants asked for shifting the deadline, as the submission directly after the Christmas holidays is not ideal and results in administrative and bureaucratic challenges.

Fifth, **funding size and duration remain a point of tension**. While many respondents view the award as appropriate for exploratory seed funding, others note that short durations and limited budgets can constrain method development, interdisciplinary coordination, or publication of early results. Without transforming the program into a mainstream funding instrument, modest

flexibility, such as optional extensions, could improve outcomes while preserving the program's logic. Funding size and duration should remain aligned with the program's seed-funding character. The evaluation does not indicate that a substantial increase in budget or project length is warranted. However, to address operational constraints reported by some awardees, particularly in method-intensive or experimental projects, FWF could introduce limited flexibility mechanisms in addition to what is already possible in line with the general FWF extensions policy to reflect high uncertainty associated with these projects. These might include optional no-cost extensions of up to six or twelve months, granted upon short justification, or modest duration adjustments without proportional budget expansion. Such targeted flexibility would remove implementation pressures while preserving the program's logic.

Finally, the **monitoring system could better reflect the nature of high-risk research**. Respondents noted that existing output tracking tools prioritize conventional publications and underrepresent learning, negative results, conceptual advances, or non-traditional outputs. Important elements of program performance, such as productive failure, redirection of research trajectories, development of new methods, or the opening of novel interdisciplinary spaces, were often visible only through open-ended survey responses and interviews, rather than through systematic monitoring data. While bibliometric indicators and econometric models provided useful evidence on interdisciplinarity and outcome variance, they cannot fully capture learning processes, abandoned pathways, or non-standard outputs. Refining the monitoring framework to record learning outcomes, methodology changes, negative results, would improve monitoring, evaluation and overall understanding of the program. FWF could introduce a structured learning component into final reporting, including specific questions on methodological redirection, productive failure, conceptual advances, and new research trajectories (see box below for potential questions). This would allow systematic capture of the high-risk dynamics identified in this evaluation, without significantly increasing reporting burden. As ResearchFish is being phased out, FWF will need to adapt its output monitoring system. This transition provides an opportunity to redesign reporting tools to better reflect the exploratory and high-risk nature of the 1000 Ideas program.

Did the project encounter major methodological or conceptual obstacles?

- Yes
- No

If yes, briefly describe what was learned (max 250 words).

Did the project generate negative, null, or unexpected findings?

- Yes
- No

If yes, explain their relevance.

In what ways did the high-risk nature of the project influence the research trajectory?
Short reflection (max 250 words).

These improvements focus on strengthening communication, learning, and adaptability rather than changing the program's core design. Addressing them would improve the program's credibility while keeping its distinctive role as a stand-alone, high-risk seed funding instrument within the FWF portfolio.

A note on the funding rates and relevance of partial randomization

The relevance of partial randomization is linked to both funding rates and the degree of uncertainty in assessing transformative ideas. If the program were funded at a level allowing approximately 30–40% of applications to be supported, and if this corresponded to the share of proposals judged above a clear quality threshold, the justification for randomization would weaken. But if budget constraints require selection among proposals of comparable quality and transformative potential, a transparent tie-breaking mechanism remains appropriate. Below such levels, randomization may continue to play a constructive role in managing selection among closely evaluated proposals. International experience suggests that even at moderate approval rates, fine-grained ranking among equally strong high-risk proposals is uncertain. In such contexts, randomization can function not only as a scarcity management tool, but as a mechanism to acknowledge evaluation uncertainty, mitigation for implicit bias and improve fairness.

6.3 Possible alternatives to the stand-alone option

The evidence from this evaluation shows that the 1000 Ideas program functions as a distinctive and purposeful high-risk, high-reward instrument within the FWF portfolio. At the same time, both international practice and feedback from applicants, reviewers and FWF stakeholders raise questions about how far its design features could be adapted, extended, or embedded elsewhere. The following two sections, therefore, explore possible alternatives to the stand-alone model, and the selective use of key process innovations, such as anonymization and partial randomization, in other FWF programs, in order to assess how the benefits of the 1000 Ideas approach could be complemented across the wider funding.

6.3.1 Staged approach

One alternative to maintaining 1000 Ideas as a stand-alone instrument would be to embed high-risk, high-reward (HRHR) seed funding as a first stage within existing FWF funding programs, most notably the Principal Investigator program. Under such a staged model, applicants could submit short, anonymized HRHR proposals as an entry point to the funding system. Proposals that demonstrate exceptional originality or transformative potential would receive small, time-limited seed grants to generate proof-of-concept, preliminary data, or methodological feasibility before being invited to apply for full PI-level funding. This would formalize the role that 1000 Ideas currently plays informally as a stepping stone toward larger grants.

A staged approach has some potential advantages. It would create a pipeline from speculative ideas to fully resourced projects. From the perspective of applicants, such a pathway could provide continuity, enabling successful HRHR seed projects to move into mainstream funding without having to compete against more mature, lower-risk proposals.

However, the evaluation evidence suggests that there are important risks associated with this type of integration. The distinctiveness of 1000 Ideas lies precisely in its separation from mainstream funding, including its short applications, lower feasibility requirements, tolerance of failure, anonymized review, and partial randomization. Embedding HRHR funding inside larger programs would likely introduce pressures for stronger feasibility, track-record demonstration, and alignment with conventional excellence criteria.

There is also a governance risk. In a staged system, HRHR seed funding could become subordinated to the needs of the larger program, with reviewers favoring ideas that

appear likely to mature into conventional PI projects. The experience of awardees interviewed in this evaluation, who valued the freedom from milestones and traditional deliverables, suggests that this would change how researchers engage with HRHR funding.

Overall, while a staged integration model could strengthen the pipeline it would do so at the cost of weakening the protective elements 1000 Ideas currently provides for radical research. Given the strong additionality, distinctive output profile, and high-risk portfolio effects observed in the evaluation, any move toward integration would need to preserve the core design features of 1000 Ideas.

6.3.2 Selective use of innovative processes in other programs

Internationally, funders have increasingly introduced innovative selection mechanisms, such as anonymized review, program manager discretion, or partial randomization in a targeted and selective manner. These mechanisms are typically applied to small, exploratory, or high-risk funding streams where conventional peer review is least effective at identifying transformative ideas.

1000 Ideas interviews suggest that **some elements of these innovations** can be transferred to mainstream funding schemes **in a limited and carefully designed way**. In the survey and interviews, several respondents argued that partial randomization and anonymization could also enhance fairness and transparency in a highly competitive PI program, in which many proposals exceed the quality threshold. From this perspective, introducing explicit randomization among equally strong proposals can make existing "hidden randomness" more transparent and equitable, while anonymization can reduce reputational bias in early screening phases.

If partial randomisation is applied, a portfolio oversight is necessary to ensure portfolio balance across disciplines and socio-demographic characteristics.

It should be noted that the use of anonymization and randomization is not universally appropriate across all funding instruments. For larger, longer-term projects involving substantial budgets, doctoral training, or infrastructure investments, the assessment of the applicant's track record, institutional environment, and delivery capacity is essential.

The key lesson from international practice is that innovative funding processes must be limited to stand-alone HRHR schemes. Anonymization and lottery-based selection function with appropriate rationale where uncertainty, failure and unconventionality are explicitly legitimized. More limited applications, such as anonymized first-stage screening, short exploratory proposals, or lottery selection among top-ranked proposals, could also strengthen fairness and diversity in mainstream programs, but they are most effective when built on the foundation of a dedicated high-risk funding stream such as 1000 Ideas. The evaluation evidence indicates broad support among FWF funding applicants for these assessment innovations, particularly when they are used in ways that are transparent, fair, and aligned with the objectives of the funding program.

Appendix A Survey details

A.1. Survey of unfunded applicants

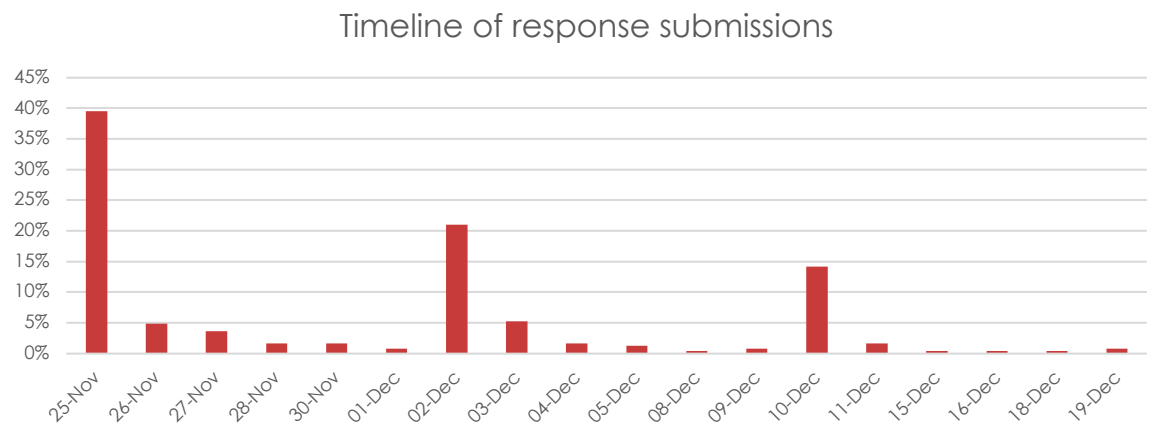
A.1.1. Response rate details

Table 12 Survey of unfunded applicants – headline response rate

Invited	Completed	%
848	248	29.2%

Source: Surveymonkey

Figure 29 Survey of applicants – Timeline of response submissions



Source: SurveyMonkey

A.1.2. Raw response data

1. Please state your age	Count of Response	%
29 or under	0	0%
30-39	73	30%
40-49	112	46%
50-59	48	20%
60 or over	13	5%
Prefer not to say	2	
Grand Total	246	

2. Which of the following most closely approximated your position when you applied for 1000 Ideas funding?	Count of Response	%
R1: First Stage Researcher (up to the point of PhD)	1	0.4%
R2: Recognised Researcher (PhD holders or equivalent who are not yet fully independent)	50	20%
R3: Established Researcher (researchers who have developed a level of independence)	118	48%
R4: Leading Researcher (researchers leading their research area or field)	77	31%
Other (please specify)	2	
Prefer not to say	0	
Total	246	

3. Which of the following best describes your 1000 Ideas project idea prior to the 1000 Ideas call launch? Please select one.	Count of Response	%
Idea existed before 1000 Ideas: I had successfully applied for funding	1	0.4%
Idea existed before 1000 Ideas: I had applied for funding but had been unsuccessful	18	8%
Idea was entirely new: it was motivated by the 1000 Ideas call	101	44%
Idea existed before 1000 Ideas: I had not applied for funding	110	48%
Other / none of the above (please specify)	3	
Grand Total	230	

4. Why did you apply for a 1000 Ideas award?	I wanted to conduct more unconventional/high-risk high-reward research than I had done previously	It was an opportunity to receive seed funding to test my high-risk research ideas	My project idea would have had lower success chances in other available funding schemes	Winning a 1000 Ideas award would mean prestige and/or career advancement
A major reason	167	199	76	50
A minor reason	44	19	70	68
Not a reason	16	9	60	101
Don't know / prefer not to say	0	2	15	2
Total	227	227	206	219

5. If you had applied for funding for your project idea prior to applying to the 1000 Ideas scheme, please select the source(s) to which you had applied. Please select all that apply.	Count	%
I had applied to another FWF program, unsuccessfully	13	6%
I had applied to an Austrian funder other than FWF, unsuccessfully	12	5%
I had applied to a funder from outside Austria (including EU Horizon), unsuccessfully	11	5%
I had applied to another FWF program, successfully	8	3%
I had applied to a funder from outside Austria (including EU Horizon), successfully	7	3%
I had applied for internal funding at my institution, successfully	6	3%
I had applied to an Austrian funder other than FWF, successfully	5	2%
I had applied for internal funding at my institution, unsuccessfully	3	1%
Not applicable: I had not applied for any prior funding for my project idea before applying to 1000 Ideas	168	72%
n	233	

6. Please describe your project idea on the following criteria:	The project presented novel and/or unconventional ideas, perspectives and methods	The project presented new interdisciplinary perspectives	The project challenged core assumptions of the research establishment in my field	The project presented a risky idea with significant chance of failure
Strongly agree	195	111	88	90
Somewhat agree	34	74	67	94
Neither agree nor disagree	2	27	56	28
Somewhat disagree	2	10	13	11
Strongly disagree	0	5	6	2
Total	233	227	230	225

7. How satisfied are you with each of the following aspects of the 1000 Ideas application process?	Very satisfied	Somewhat satisfied	Neither satisfied nor dissatisfied	Somewhat dissatisfied	Very dissatisfied	Total
The clarity of the requirements for applications	100	77	20	26	4	227
Adequacy of the format and length of the application to communicate the transformative nature of my proposal	128	69	14	10	8	229
Support during the application process from the FWF	67	40	63	4	12	186
User-friendliness of FWF's application submission portal	88	87	35	14	3	227
The timeframe available to prepare my application	88	78	38	16	4	224
The effort needed to prepare my application	73	97	41	11	7	229
The time from submission to application outcome	46	73	54	35	19	227
Overall efficiency of the process	43	77	63	25	14	222

8. Did you receive feedback on your proposed idea or approach? Please note: this is excluding requests for corrections or feedback about elements like formatting or missing details.	Count	%
Yes	31	14%
No	198	86%
Total	229	

9. Based on this feedback, how confident are you in the following aspects?	Very confident	Somewhat confident	Not at all confident	Total

The assessment was made with a sufficient understanding of all the fields of research involved in my application	1	12	13	26
My application received an unbiased assessment	2	9	12	23
My application received a thorough assessment	2	11	12	25

10. To what extent do you agree with the following statements about using partial randomization (lottery) and anonymisation in the selection process?	**Strongly agree**	**Somewh at agree**	**Neither agree nor disagree**	**Somewh at disagree**	**Strongly disagree**	**Total**
I was aware of the inclusion of partial randomization in the selection process when I applied	100	36	13	19	42	210
I was aware of the inclusion of anonymisation in the selection process when I applied	162	24	11	4	10	211
The knowledge of the partial randomization approach influenced my decision to apply	16	19	67	15	80	197
The knowledge of the anonymisation approach influenced my decision to apply	23	29	64	14	76	206
The knowledge of the partial randomization approach influenced the time spent on my application	5	12	64	22	99	202
The knowledge of the anonymisation approach influenced the time spent on my application	6	15	63	23	98	205
The knowledge of the partial randomization approach influenced how I prepared my application	8	24	47	22	95	196
The knowledge of the anonymisation approach influenced how I prepared my application	23	40	42	20	77	202

11. From the below list, please select the attributes which you find applicable to randomization (lottery) and anonymisation respectively.	**Partial randomization**	**Anonymisation**
Enhanced equal opportunities - Partial randomization	81	167
Prevention of conflicts of interest and unconscious bias - Partial randomization	59	167
Improved opportunities for risky research - Partial randomization	103	89
Increased opportunities for more thematic diversity - Partial randomization	108	64
Increased risk of selecting research projects of low quality - Partial randomization	125	30
Lower reputation gain for funding approval compared to a conventional selection process - Partial randomization	74	15
Lower cost or effort in application - Partial randomization	58	19

12. Do you think partial randomization is an acceptable method of…	Allocating 1000 Ideas research funds?	Allocating funding for other grant types?
Yes	105	70
No	80	108
Total	185	178

13. Do you think applicant anonymisation is an acceptable method of…	Allocating 1000 Ideas research funds?	Allocating funding for other grant types?
Yes	165	127
No	29	59
Total	194	186

14. Maintaining anonymity in the project description in my proposal was simple.	Count	%
Strongly agree	59	28%
Somewhat agree	81	39%
Neither agree nor disagree	23	11%
Somewhat disagree	32	15%
Strongly disagree	15	7%
Total	210	

15. Would you consider reapplying for 1000 Ideas funding, or alternative funding for the same idea? Please select one.	Count	%
I have applied/would apply for alternative funding without significantly changing my project idea	20	12%
I have applied/would apply for alternative funding with a similar idea but with fewer novel high-risk/high-reward aspects	24	14%
I have applied/would apply for alternative funding with a reduced scope of the same idea	8	5%
I have reapplied/would reapply to 1000 Ideas funding	55	33%
No, I would focus on other things	62	37%
Other (please specify)	27	
Don't know / prefer not to say	14	
Grand Total	169	

16. If your application was rejected in the lottery and you resubmitted it to 1000 Ideas, how much effort did you invest in updating your idea or approach?	Count	%
I made some revisions to my proposal	12	6%
I made superficial revisions to my proposal	10	5%
I reapplied with a completely different idea	9	4%
No changes - I resubmitted the exact same proposal	7	3%
I made extensive revisions to my proposal	4	2%
Not applicable / prefer not to say	160	79%
Grand Total	202	

17. If you have applied for alternative funding for the same (or similar) idea, has this been successful?	Count	%
Yes	30	15%
No	26	13%
I have not applied / prefer not to say	147	72%
Grand Total	203	

18. Please feel free to enter any further comments about your experience of the FWF 1000 Ideas program in the box below.
Open-Ended Response (n=75)

19. Please feel free to enter any suggestions about how to improve any aspect of the 1000 Ideas program or its thematic focus.
Open-Ended Response (n=88)

A.2. Survey of awardees

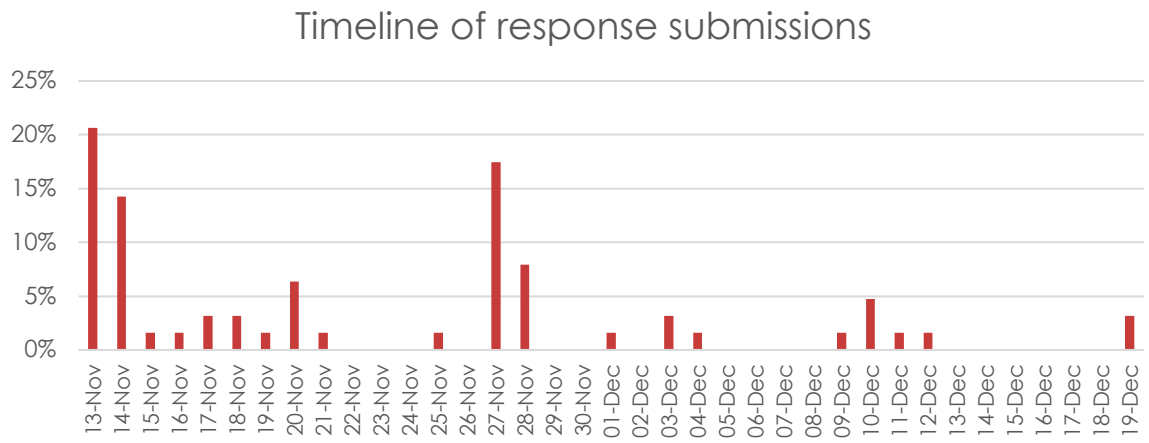
A.2.1. Response rate details

Table 13 Survey of awardees – headline response rate

Invited	Completed	%
102	63	61.8%

Source: SurveyMonkey

Figure 30 Survey of awardees – Timeline of response submissions



Source: SurveyMonkey

A.2.2. Raw response data

1. Please state your age	**Count of Response**	**%**
29 or under	0	0%
30-39	11	18%
40-49	37	60%
50-59	9	15%
60 or over	5	8%
Prefer not to say	1	
Total	62	

2. Which of the following most closely approximated your position when you applied for 1000 Ideas funding?	**Count of Response**	**%**
R1: First Stage Researcher (up to the point of PhD)	0	0%
R2: Recognised Researcher (PhD holders or equivalent who are not yet fully independent)	11	18%
R3: Established Researcher (researchers who have developed a level of independence)	33	55%
R4: Leading Researcher (researchers leading their research area or field)	16	27%
Other (please specify)	1	
Prefer not to say	2	

Total	60	

3. Which of the following best describes your 1000 Ideas project idea prior to the 1000 Ideas call launch? Please select one.	Count of Response	%
Idea existed before 1000 Ideas: I had successfully applied for funding	0	0%
Idea existed before 1000 Ideas: I had applied for funding but had been unsuccessful	1	2%
Idea was entirely new: it was motivated by the 1000 Ideas call	28	48%
Idea existed before 1000 Ideas: I had not applied for funding	29	50%
Grand Total	58	

4. Why did you apply for a 1000 Ideas award?	I wanted to conduct more unconventional/high-risk high-reward research than I had done previously	It was an opportunity to receive seed funding to test my high-risk research ideas	My project idea would have had lower success chances in other available funding schemes	Winning a 1000 Ideas award would mean prestige and/or career advancement
A major reason	46	56	33	10
A minor reason	11	4	14	28
Not a reason	5	3	10	22
Don't know / prefer not to say	0	0	5	2
Total	62	63	57	60

5. If you had applied for funding for your project idea prior to applying to the 1000 Ideas scheme, please select the source(s) to which you had applied. Please select all that apply.	Count	%
An Austrian funder other than FWF, successfully	4	6.3%
Another FWF program, unsuccessfully	2	3.1%
A funder from outside Austria, unsuccessfully	2	3.1%
An Austrian funder other than FWF, unsuccessfully	1	1.6%
Another FWF program, successfully	1	1.6%
Internal funding at my institution, successfully	1	1.6%
Internal funding at my institution, unsuccessfully	1	1.6%
A funder from outside Austria, successfully	0	0.0%
Not applicable: I had not applied for any prior funding for my project idea	52	81.3%

n	63	

6. Please describe your project idea on the following criteria:	The project presented novel and/or unconventional ideas, perspectives and methods	The project presented new interdisciplinary perspectives	The project challenged core assumptions of the research establishment in my field	The project presented a risky idea with significant chance of failure
Strongly agree	54	25	19	32
Somewhat agree	8	21	22	18
Neither agree nor disagree	1	6	14	8
Somewhat disagree	0	7	5	5
Strongly disagree	0	3	2	0
Total	63	62	62	63

7. How satisfied are you with each of the following aspects of the 1000 Ideas application process?	Very satisfied	Somewhat satisfied	Neither satisfied nor dissatisfied	Somewhat dissatisfied	Very dissatisfied	Total
The clarity of the requirements for applications (call documents, criteria, etc.)	53	9	1	0	0	63
Adequacy of the format and length of the application to communicate the transformative nature of my proposal	53	10	0	0	0	63
Support during the application process from the FWF (e.g. in response to questions)	35	8	6	0	0	49
User-friendliness of FWF's application submission portal	36	16	9	2	0	63
The timeframe available to prepare my application	39	15	6	1	0	61
The effort needed to prepare my application	41	16	5	1	0	63
The time from submission to application outcome	26	24	7	5	1	63
Overall efficiency of the process	40	20	1	2	0	63

8. To what extent do you agree with the following statements about using partial randomization (lottery) and anonymisation in the selection process?	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree	Total
I was aware of the inclusion of partial randomization in the selection process when I applied	37	8	5	2	10	62
I was aware of the inclusion of anonymisation in the selection process when I applied	51	6	4	1	1	63

The knowledge of the partial randomization approach influenced my decision to apply	5	5	17	8	27	62
The knowledge of the anonymisation approach influenced my decision to apply	10	4	18	8	22	62
The knowledge of the partial randomization approach influenced the time spent on my application	0	3	16	5	38	62
The knowledge of the anonymisation approach influenced the time spent on my application	2	1	16	6	38	63
The knowledge of the partial randomization approach influenced how I prepared my application	1	8	10	8	35	62
The knowledge of the anonymisation approach influenced how I prepared my application	10	12	10	12	19	63

9. From the below list, please select the attributes which you find applicable to randomization (lottery) and anonymisation respectively.	**Partial randomization**	**Anonymisation**
Enhanced equal opportunities	34	57
Prevention of conflicts of interest and unconscious bias	24	55
Improved opportunities for risky research	34	30
Increased opportunities for more thematic diversity	44	30
Increased risk of selecting research projects of low quality	29	8
Lower reputation gain for funding approval compared to a conventional selection process	14	4
Lower cost or effort in application - Partial randomization	10	5
Total	63	63

10. Do you think partial randomization is an acceptable method of…	**Allocating 1000 Ideas research funds?**	**Allocating funding for other grant types?**
Yes	42	31
No	10	17
Total	52	48

11. Do you think applicant anonymisation is an acceptable method of…	**Allocating 1000 Ideas research funds?**	**Allocating funding for other grant types?**
Yes	60	45
No	2	15

Total	62	60

12. Maintaining anonymity in the project description in my proposal was simple.	**Count**	**%**
Strongly agree	32	51%
Somewhat agree	21	33%
Neither agree nor disagree	2	3%
Somewhat disagree	6	10%
Strongly disagree	2	3%
Total	63	

13. Thinking about the outputs and the nature of your 1000 Ideas project, to what extent do you agree with the following statements?	**Strongly agree**	**Somewhat agree**	**Neither agree nor disagree**	**Somewhat disagree**	**Strongly disagree**	**Total**
My project led (or will likely lead) to a fundamentally new concept or field of study	22	26	6	5	1	60
My project resulted (or will likely result) in findings that contradicted established theories or conventional wisdom in my field	11	22	12	10	4	59
My project resulted (or will likely result) in critical follow-up questions for my field	39	15	6	0	0	60

14. To what extent do you agree with the following statement? The outputs from my 1000 Ideas project differ qualitatively from those generated through other research funding schemes.	**Count**	**%**
Strongly agree	23	40%
Somewhat agree	14	24%
Neither agree nor disagree	6	10%
Somewhat disagree	7	12%
Strongly disagree	8	14%
Total	58	

15. If outputs from your 1000 Ideas project differ qualitatively from those generated through other research funding schemes, in which of the following ways did your 1000 Ideas outputs differ from those of other projects you have had? Please select all that apply.	**Count**	**%**

More exploratory or speculative	31	63%
More methodological or conceptual contributions	31	63%
Higher level of innovation or novelty	30	61%
Greater uncertainty or variability in outcomes	21	43%
Less alignment with established publication channels	13	27%
Other (please specify)	1	2%
Total	49	

16. Did you encounter a 'failure' (e.g. a major research approach/hypothesis not supported by the data) during the 1000 Ideas project?	**Count**	**%**
Yes	12	21%
No	19	34%
Not applicable – my project is currently underway	25	45%
Don't know / prefer not to say	5	
Total	56	

18. To what extent do you agree with the following statement? The high-risk/high-reward nature of my project makes it difficult to publish results—particularly in cases of negative, null, or inconclusive findings.	**Count**	**%**
Strongly agree	7	11.9%
Somewhat agree	13	22.0%
Neither agree nor disagree	10	16.9%
Somewhat disagree	11	18.6%
Strongly disagree	18	30.5%
Total	59	

19. If your research approach/hypothesis was not supported, to what extent do you agree with the following statement? Even when initial research approach/hypothesis were not supported, the project generated valuable insights for the scientific community or my research field.	**Count**	**%**
Strongly agree	25	48%
Somewhat agree	9	17%
Neither agree nor disagree	0	0%

Somewhat disagree	0	0%
Strongly disagree	0	0%
Not applicable	18	35%
Total	52	

20. Did receiving 1000 Ideas funding increase your willingness to pursue unconventional or risky research in the future?	**Count**	**%**
Yes, significantly	34	56.7%
Yes, somewhat	20	33.3%
No	6	10%
Don't know / prefer not to say	1	
Total	60	

21. Have you secured follow-on funding for your award, or do you seek to do so?	**Count**	**%**
Yes, I have gained funding to build on the results of my 1000 Ideas project	8	14%
I have not yet secured follow-on funding for my project, but I am actively applying	11	19%
I have not yet secured follow-on funding for my project, but I plan to apply in the future	36	62%
No, I have not secured follow-on funding, nor do I intend to	3	5%
Don't know / prefer not to say	3	
Total	58	

22. If you have received, or expect to receive, follow-on funding in the research domain of the 1000 Ideas project, please estimate the value of the secured or sought funding (€).	**n**	**Total**	**Mean**	**Median**
Received	12	€ 6,210,000	€ 517,500	€ 385,000
Sought	16	€ 7,432,026	€ 464,502	€ 450,000

23. Would your project have been carried out without 1000 Ideas funding?	**Count**	**%**
Yes, at the same scale/timeline	3	5%
Yes, but at a smaller scale or delayed	6	10%

Possibly, but uncertain	18	30%
No, it would not have happened	34	56%
Total	61	

24. Please feel free to enter any further comments about your experience of the FWF 1000 Ideas program in the box below.
Open-Ended Response (n=24)

25. Please feel free to enter any suggestions about how to improve any aspect of the 1000 Ideas program or its thematic focus.
Open-Ended Response (n=21)

Appendix B Interview details

Interviews took place in the period between October 20205 and January 2026. They were all conducted online, as semi-structured interviews with targeted interview guidelines to explore specific perspectives. Each interview lasted around one hour. In total 15 interviews were conducted with different stakeholders:

- **Scoping Interviews with FWF Staff** – Two interviews were conducted with FWF representatives, who are responsible for the program management of the 1000 Ideas Program as well as the Principal Investigator Program. These interviews helped to obtain perspectives on the programs' distinct characteristics, strategic directions, and operational management. Moreover, were they essential to critically assess the potential overlap and complementarities with other funding programs.
- **Actors involved in the Selection Process** – One interview was conducted with the chair of the 1000 Ideas Jury, who shared observations from the assessment of core aims of the program (high-risk, novelty) and the appropriateness of decision-making procedures and criteria. Three members from the FWF board were interviewed to gain an understanding on the involvement of the FWF board in the early stages of the selection process. Three interview partners where suggested by the FWF staff by criteria of disciplinary coverage and diversity of perspectives on the process.
- **Principal Investigators of 1000 Ideas Projects** – Nine interviews were conducted with award holders. These were selected based on previous analytical steps, supporting the triangulation of evidence, either as following up to interesting survey responses or based on high bibliometric values of novelty and/or disruption. Further was the selection aimed at a balance of gender, academic age, disciplines, number of projects funded, and Austrian research organizations. The interviews served to gain a better understanding with the application process, perspectives on high-risk and novelty of the projects, the outcomes achieved and the challenges encountered.

We present below our full list of interviews and the interview tool and questions used throughout.

B.1. List of interviews

Table 14 List of interviews

Name	Interviewee type	Organization	Position	Interview date	Interviewer
Tina Olteanu, Stefanie Linsboth	Scoping Interview	FWF	Unit head – Program Management Coordination, Program Manager 1000 Ideas	16.10.2025	Anete Vingre, Florentine Frantz, Laura Sutinen
Mario Mandl	Scoping Interview	FWF	Program Manager Principal Investigator Projects	30.10.2025	Florentine Frantz
James Kirchner	Selection process	ETH Zürich	Chair - 1000 Ideas Jury	16.12.2025	Florentine Frantz
Markus Aichhorn	Selection process	Graz University of Technology	FWF Board Member (Theoretical Physics and Astrophysics)	8.1.2026	Florentine Frantz

Name	Interviewee type	Organization	Position	Interview date	Interviewer
Alexandra Lusser	Selection process	Medical University of Innsbruck	FWF Board Member (Biochemistry and Structural Biology)	9.1.2026	Florentine Frantz
Libora Oates-Indruchova	Selection process	University of Graz	FWF Board Member (Sociology and Interdisciplinary Social Sciences)	12.12.2025	Florentine Frantz
Markus Faulhuber	PI Interview	University of Vienna		19.1.2026	Florentine Frantz
Andreas Angermayr	PI Interview	CeMM – Research Center for Molecular Medicine		10.12.2026	Florentine Frantz
Wolfgang Göderle	PI Interview	Uni Graz		12.12.2025	Florentine Frantz
Florian Halbritter	PI Interview	St. Anna Children's Cancer Research Institute		9.1.2026	Florentine Frantz
Anja Hörger	PI Interview	University of Salzburg		8.1.2026	Florentine Frantz
Roland Lammegger	PI Interview	Graz University of Technology		7.1.2026	Florentine Frantz
Milena Karin Leybold	PI Interview	University of Linz		9.1.2026	Florentine Frantz
Katharina Rebay-Salisbury	PI Interview	Austrian Academy of Sciences		15.12.2025	Florentine Frantz
Rainer Schindl	PI Interview	Graz Medical University		4.12.2025	Florentine Frantz

B.2. Interview tools

B.2.1. Interview guideline – Scoping Interview 1000 Ideas Team

Questions about the 1000 Ideas Program

- Could you please tell us more about how the 1000 Ideas program came about and what the rationale was for it? Was there any evidence you used to support the introduction of the program?
- How would you situate the Program in the wider FWF portfolio? Did you envisage links to other Programs?
- We know the official objectives of the program. How would the success of the program look to you? How would you characterize the risk appetite of FWF (and overseeing organizations) in relation to this program?
- Could you tell us more about the decision to introduce anonymous review and randomization? Was it the first program where you used this, and what are your expectations towards these process elements?

- The program aims to fund non-conventional/original research. What, if any, challenges or difficulties did you face implementing a program with such a focus?

Process questions

- Confirm terminology – FWF Board and 1000 Ideas Jury.
- What exactly is being checked in the Formal Check, and what is being checked in the Completeness Check?
- Completeness check ranking of scientific novelty – what does this involve? How many rejections at this stage? What is the feedback? How does ranking (0-3) influence the next step?
- Does the 1000 Jury change for each funding call, depending on applications submitted?
- How are the Board members' and 1000 Ideas jury review different in principle? Both do a desk review, the Jury also has a meeting, but the criteria are the same.
- What is the purpose of the B&C list emerging from the Board members' review, if it is non-obligatory for the Jury?
- Fixed number of projects to fund outright – is this determined by the available budget? How do they calculate the fixed number? At which stage is the limited number implemented? Is it a Jury meeting?
- What happens if the Board disagrees with the Jury's grouping of proposals? Can it disagree, or is the Board meeting mostly formal?
- How long between proposal and decision? We see the process between proposal and final FWF Board meeting is roughly 5 months, but when is the board meeting? Is it as soon as there is a recommendation from the Jury?
- How is the lottery implemented in practice?

B.2.2. Interview guideline – Scoping Interview – PI Program

Introduction

- What is your personal relation/ involvement in/knowledge about the 1000 Ideas Program?
  - What problem in the Austrian system is the 1000 ideas Program the answer to?
  - What are your personal observations of /thoughts about the 1000 ideas selection process/criteria? (e.g. Lottery system, double blind, involvement of FWF Board in the pre-evaluation)

Relationship of 1000 Ideas and PI Projects

- What are the main intersection points with the PI Program and the 1000 Ideas Program?
  - How is the exchange and coordination between Program representatives within the FWF organized?
  - Are there similar reviewers/jury members involved in the evaluation process?
- Which qualitative differences do you perceive between the 1000 Ideas and the PI Program?
  - Are the 1000 Ideas projects perceived to be particularly original/high-risk/creative?
  - Does the understanding of "failure" and "risk" of research projects differ compared to other funding Programs for basic research?

- Are you aware of qualitative differences in the outputs generated in the 1000 Ideas program as compared to FWF's Principal Investigator Projects?
- How do you assess the effort needed to prepare proposals for PI projects and 1000 Ideas projects?
- Do you think the 1000 Ideas Program attracts "different" researchers than the PI projects?
- Is the 1000 ideas Program more attractive for specific disciplines/interdisciplinary research?

- Is there any knowledge about the pathways of finished 1000 ideas projects into PI projects / other FWF projects?
  - Impacts of the involvement of the FWF Board in the pre-evaluation of 1000 projects?
  - Is the evidence/proof of concepts created in the 1000 ideas projects sufficient to convince reviewers?
- How is the 1000 Ideas Program distinct, complementary or overlapping with other Programs? (FWF/Austria/EU)
  - What is the perception of qualitative differences in outputs/applications between 1000 ideas and other FWF funding Programs ?
  - How do you assess the difference between 1000 ideas projects and Programs funding individual researchers ("Personenförderung")?
- Is there anything else, we have not sufficiently covered so far, that you deem relevant for the evaluation of the 1000 Ideas Program

B.2.3. Interview guideline– Selection Process

<u>The 1000 Ideas Selection Process</u>

1.1. Can you describe your experience reviewing for FWF and other funders?

1.2. Please describe your role and contribution in the 1000 Ideas Selection Process? In the assessment of how many applications were you involved?

1.3. Did you find the program briefing documents clear and useful? Was there any information you found particularly relevant or anything that was unclear?

1.4. Do you think the assessment timeline and guidance provided is appropriate?

1.5. How do you assess the quality of the applications? To what extent did the applications reflect the program objectives?

1.5.1. The program aims to fund non-conventional/original research (transformative, research). Did you experience any challenges or difficulties in assessing these elements?

1.5.2. How do you assess whether the project is "high-risk"?

1.5.3. How do the proposals compare to proposals from other reviewing experience?

1.5.4. Do you think there are disciplinary differences/challenges?

1.6. What do you think about the applicant anonymization? Is it easy to maintain anonymity throughout the process?

1.6.1. Were there any challenges in deciding with anonymous applications?

1.7. Do you have any comments about the overall appropriateness and length, format and sectioning of the applications? Would you change anything for future funding rounds?

1.8. What do you think about the partial randomization (Lottery) approach?

1.9. How could the process be improved?

Jury Specific- Questions

1.10. What were the discussions in jury meetings about? What was most difficult for the jury members? Was it challenging to calibrate? How exactly?

1.11. Do you think the decision-making procedure and selection criteria are appropriate to select high-risk ideas?

The 1000 Ideas Projects

2.1. How do you think the 1000 Ideas projects are different from other FWF previous projects?

2.1.2. Do you think it is necessary to have a separate program to fund high-risk projects or could it be integrated into another existing program? Which adaptions might be necessary?

2.2. Do you think these high-risk projects are likely to fail? What do you think failure is?

2.3. Do you think that the novelty of the 1000 Ideas projects can be easily observed in the publications they produce?

2.4. Have you ever considered applying for the 1000 Ideas Program yourself?

Conclusion

3.1. What effects do you think the 1000 Ideas program has on the Austrian research/ research funding landscape?

3.2. If you could change one thing about the 1000 Ideas program or the Selection Process, what would it be?

3.3. Is there anything else you would like us to know?

B.2.4. Interview guideline – PI Interviews

The 1000 Ideas Project

1.1 How did you come up with your idea for the 1000 Ideas project?

1.1.1 Did you come up with the idea and looked for a place to submit it somewhere or did the call inspire you to think about a new idea?

1.2. Why did you apply to the 1000 Ideas Program?

1.2.1. Did you consider other options? Had you already tried?

1.2.2. Do you think it would have been more challenging to get funding for your idea in other funding streams?

1.2.3. Would you have conducted your project also without the 1000 Ideas funding?

1.3. Do you have any prior experience with the FWF?

1.3.1. How do you think this project is different from previous projects?

Application Process and novel selection procedure

2.1 How do you experience the application and selection process of the 1000 Ideas Program? (e.g., clarity, support during the process, application portal, effort, time from submission)

2.1.1 Do you think it is fit to choosing radically new/particularly original ideas?

2.2 What do you think about the partial randomization (Lottery) approach? Did it particularly encourage you to apply? Why?

2.3 What do you think about the applicant anonymization? Do you think it is easy to maintain anonymity throughout the process?

2.4 How satisfied were you with the feedback you received, if applicable?

2.5 How could the process be improved?

Novelty, Risk, and Failure

3.1 What makes your project particularly novel? (Compared to previous projects? In your discipline?)

3.2. What makes your project a high-risk project?

3.3. How did you manage the uncertainty in the project (e.g. specific publication strategies? Back-up options? Collaboration strategies?)?

3.4. Did you encounter a 'failure' (e.g. a major research approach/hypothesis not supported by the data) during the 1000 Ideas project?

3.4.1. How did you deal with a major research approach/hypothesis not supported?

3.4.2 Which insights were generated from this?

Results and Output

4.1. How do you personally assess the output and outcomes of the project (so far)?

(e.g., Publications – Which venues? Conference presentations? Engagement with different stakeholders? Awards?)

4.2. Did you experience any barriers to the progress of publishing? Please explain in detail?

4.3. Do you think it is groundbreaking or laid the groundwork for such findings? Why?

4.4. Are the outputs different to what has been achieved with other funding?

(How? More exploratory? More innovative? More method innovation?)

4.5. Do you foresee follow-up projects in the future? (e.g., FWF PI Projects? ERC, HORIZON?)

4.6. Which effect do you thing the 1000 Ideas project had on your career?

4.6.1. Is there a lower reputation gain if project funded via program using lottery?

Conclusion

5.1. Did the 1000 Ideas funding increase your willingness to pursue unconventional or risky research in the future?

5.2. If you could change one thing about the 1000 Ideas program, what would it be?

5.3. Is there anything else you would like us to know?

Appendix C Econometric analysis of bibliometric data

C.1. Summary of findings

- We find evidence that the program directs research of the funded PIs toward greater novel interdisciplinarity rather than higher average citation impact. Estimates consistently show an increase in interdisciplinary novelty among funded projects, while average citation impact does not systematically change. This points to a qualitative reorientation of research outputs rather than uniform gains in conventional impact metrics.
- The program enables scientific risk-taking rather than uniform performance gains. Funded 1000 Ideas projects exhibit significantly higher dispersion in post-award citation outcomes than comparable PI program-funded projects, while unfunded applicants show no such differences. This pattern is consistent with a higher-risk, higher-reward funding objectives.
- Randomization broadens who gets funded. By weakening reliance on past citation impact and elevating novelty-oriented publication profiles, the lottery shifts selection toward more exploratory research trajectories.
- Program value lies in composition and trajectories, not just averages. Taken together, the evidence shows that the 1000 Ideas program primarily operates by changing what is funded and how research evolves, rather than by delivering uniform improvements in average performance, consistent with its mandate to support exploratory, high-risk research.
- The analysis illustrates the importance of combining Difference-in-Differences designs with dispersion-focused tests when evaluating high-risk research funding, as mean-based evaluations alone miss key program effects.

C.2. Introduction

This document presents the econometric analysis of bibliometric and text-based indicators used to assess the effects of the 1000 Ideas program on research outputs and trajectories. The analysis is designed to address two distinct but related questions. First, it examines whether the program's selection mechanism, including partial randomization, systematically alters the types of projects that are funded. Second, it evaluates whether receiving funding under the 1000 Ideas program leads to measurable differences in post-award research outcomes, relative both to comparable programs and to unfunded applicants.

Evaluating high-risk, high-reward research poses specific methodological challenges. Outcomes are inherently uncertain, may take time to materialize, and are often characterized by high dispersion rather than consistent average gains. When evaluations focus solely on mean effects, they risk understating program value if impacts are realized through a small number of highly successful projects or through changes in the distribution of outcomes rather than shifts in central tendencies. The econometric strategy adopted here explicitly accounts for these features by combining analyses of average effects, outcome dispersion, and selection mechanisms.

The analysis draws on longitudinal bibliometric data covering publications, citations, interdisciplinarity, semantic novelty, disruption, and citations in patents and policy documents. Outcomes are observed before and after funding decisions, allowing changes over time to be compared across groups. To improve internal validity and interpretability, the analysis exploits three complementary sources of variation: (i) comparisons between the 1000 Ideas program and standard FWF PI funding schemes, (ii) comparisons between funded and unfunded applicants within programs, and (iii) partial randomization among proposals deemed fundable under the lottery component of the 1000 Ideas program.

The econometric framework is organized as follows. Section 1.4.1 estimates average post-award effects using Difference-in-Differences (DiD) and Difference-in-Difference-in-Differences (DDD) designs, benchmarking the 1000 Ideas program against standard funding schemes while accounting for pre-award differences in researcher profiles and outputs. Section 1.4.2 examines differences in outcome dispersion, testing whether the program is associated with higher conditional variance in research outcomes, consistent with a higher-risk, higher-reward portfolio. Section 1.4.3 analyses selection-stage effects, assessing how randomization reshapes the relationship between traditional indicators of past performance and the probability of funding, and whether it increases the weight placed on novelty-related dimensions at the point of selection.

Throughout, results from the econometric models are interpreted alongside descriptive evidence presented in Section 1.3. This combined approach ensures that findings are robust, transparent, and appropriately contextualized, while remaining aligned with the program's explicit objective of supporting exploratory research where success is uncertain and unevenly distributed. The analysis therefore focuses not only on whether the program "works on average", but also on whether it changes what is funded, enables different research trajectories, and accepts greater ex-post uncertainty as part of its design logic.

C.3. Descriptives

This section describes the structure of the application data and the key choices made to construct a consistent analytical sample. We focus in particular on repeated applications, observation windows, field composition, and the indicators used in the analysis.

C.3.1. Repeated applicants

Applicants in the FWF data submit multiple applications over time, and this is true for both the 1000 Ideas program and standard Principal Investigator (PI) programs. As a result, the raw data contain multiple observations per applicant, spanning several cohorts and program types. Without careful treatment, this multiplicity would lead to double-counting individuals and conflating program effects with repeated participation.

Table 15 Repeated applicants

Applications per applicant	1000 Ideas	PI program
1	506	1257
2	176	576
3	113	203
4	63	62
5	31	46
6+	45	27

To address this, we follow a conservative and transparent convention: within applicants who succeeded at least once in the 1000 Ideas, we retain only their first successful application. For all other applications, including unsuccessful 1000 Ideas submissions and standard PI program applications, repeated observations are retained. These duplicates are handled explicitly through the inclusion of cohort fixed effects, which absorb systematic differences across application rounds and decision years. The unit of analysis is the author–year, and bibliometric outcomes are therefore aggregated at this level. This approach preserves the richness of

applicants' publication histories while ensuring that comparisons are not driven by repeated exposure or timing effects.

Table 16 Unique number of applications and applicants after the first cleaning convention (cross-sectional)

	1000 Ideas	**PI**	**Total**
N_applications	1071	4415	5486
N_applicants	934	2494	3105

C.3.2.Time-window and panel structure

The timing of publications relative to the funding decision is a central consideration in the analysis, given the lag between grant award, research execution, and observable outputs. Moreover, the econometric strategy relies on a difference-in-differences framework, which requires clearly defined pre- and post-award periods. To capture both pre-award baselines and post-award outcomes, we organize the data in event time, defined as the number of years relative to the funding decision (relative year), and focus on a symmetric window from three years before to three years after the decision year. Table 17 below reports the number of observations by event year and program. Coverage is broadly comparable across the two programs in the pre-award period (years −3 to −1), with similar numbers of observations for 1000 Ideas and PI applications.

Table 17 Number of applications–year observations by relative year and program (panel data

Relative year	**1000 Ideas**	**PI**	**Total**
-3	922	3766	4688
-2	922	3797	4719
-1	924	3800	4724
0	891	3670	4561
1	707	3107	3814
2	562	2471	3033
3	532	2263	2795
Total	5460	22874	28334

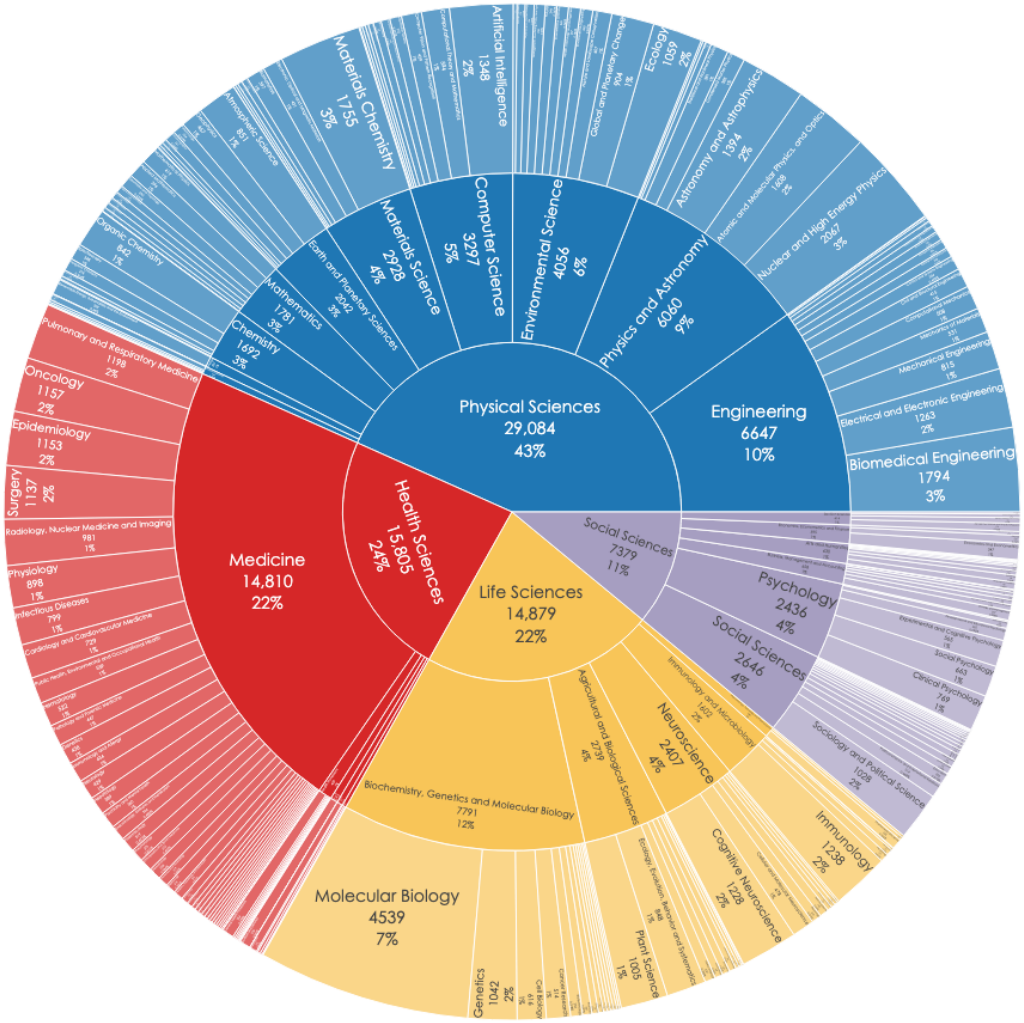
In the econometric analysis, we restrict attention to balanced event-time windows, ensuring equal exposure before and after the decision. As expected, the number of observations declines gradually in the post-award period, reflecting publication lags and right-censoring rather than differential attrition. This decline is visible for both program types and becomes more pronounced from year +1 onwards, implying that estimates based on unbalanced panels may confound treatment effects with differences in exposure time and observability across applications. Moreover, not all applications are observed in every relative year. This reflects the construction of the bibliometric panel and the availability of publication data, rather than attrition or selective dropout. As a result, the cross-sectional number of applications always exceeds the number of observations in any single relative year. This feature is common in event-study settings with bibliometric outcomes and reinforces the need for balanced-panel restrictions in the main econometric specifications.

C.3.3.Scientific fields

The scientific output associated with the analyzed applications covers a broad range of disciplines. Figure 31 summarizes the distribution of publications across scientific fields, organized according to a hierarchical classification from domain to field to subfield. The portfolio spans a wide range of disciplines, with the largest shares concentrated in Physical Sciences, Life Sciences, and Medicine, followed by Engineering and the Social Sciences.

The distribution reflects the broad disciplinary scope of the funding programs under study. Physical Sciences account for the largest share of publications, driven by contributions in physics, materials science, and chemistry, while Life Sciences and Medicine together represent a substantial proportion of outputs across molecular biology, biochemistry, clinical medicine, and related areas. Engineering outputs are primarily concentrated in biomedical and electrical engineering, and Social Sciences contributions are spread across psychology, economics, and related fields.

Figure 31 All publication by topic hierarchy



Importantly, no single field dominates the portfolio to the extent that program-level results would be driven by a narrow disciplinary base. In the Difference-in-Differences analysis, disciplinary differences are addressed through the design of the estimator: identification relies

on within-unit changes over time and comparisons across program and decision cohorts, which difference out time-invariant disciplinary characteristics. Field fixed effects are introduced in the randomization-based specifications, where conditioning on field improves precision without affecting identification.

C.3.4. Indicators

The econometric analysis relies on a set of indicators designed to capture multiple dimensions of research influence and the high-risk, high-reward character of scientific activity. Rather than relying on a single measure of performance, the indicators span scientific impact, novelty, interdisciplinarity, disruption, and broader societal relevance. This multidimensional approach reflects the program's objectives and mitigates the limitations of traditional citation-based metrics when evaluating exploratory research. Specifically, the indicator set covers the following dimensions:

- Scientific impact is proxied using field-weighted citation indicators, which normalize citation counts by publication year and field, allowing comparisons across disciplines with different citation intensities. These measures capture visibility and uptake within the scientific literature while reducing confounding from disciplinary norms.
- Interdisciplinarity is measured using an index of diversity in referenced scientific fields (IDR), reflecting the extent to which publications draw on knowledge from multiple disciplinary domains. Higher values indicate a broader and more heterogeneous knowledge base. This indicator is particularly relevant for the 1000 Ideas program, which explicitly targets research that crosses established disciplinary boundaries.
- Semantic novelty is captured using a cosine-distance measure based on text similarity, which quantifies the semantic distance between a publication and its prior art. Higher values indicate greater conceptual departure from existing work. Unlike citation-based indicators, this measure captures novelty at the level of ideas and language, making it well suited to identifying early-stage or unconventional research that may not yet have accumulated citations.
- Disruption is measured using a citation-network–based index that captures the extent to which a publication eclipses or displaces the work it builds upon. Higher values indicate research that redirects subsequent citations away from prior art, consistent with paradigm-shifting contributions. This indicator complements semantic novelty by capturing downstream effects in the citation network rather than textual distance alone.
- Broader societal and translational relevance is captured using citations in patents and citations in policy documents. Patent citations proxy technological relevance and knowledge transfer to innovation activities, while policy citations capture uptake in regulatory, governmental, or advisory contexts. These indicators are sparse by construction and are therefore interpreted cautiously, primarily as complementary signals rather than core outcome measures.

All indicators are constructed at the publication level and aggregated to the author-year level to match the unit of analysis used in the econometric models. Where appropriate, indicators are normalized to account for differences in publication practices across fields and over time. Unless stated otherwise, indicators are measured separately in the pre-award and post-award periods defined in Section C.3.2. In the econometric analysis that follows, the indicators are used to assess average treatment effects, differences in outcome dispersion, and selection-stage mechanisms. Importantly, no single indicator is interpreted in isolation; instead, patterns across indicators are used to assess whether the 1000 Ideas program changes what is funded, how research develops after funding, and the degree of uncertainty associated with resulting outcomes.

Table 18 reports summary statistics for the main indicators, illustrating substantial heterogeneity and right-skewness across outcomes.

Table 18 Descriptive statistics of key indicators

	count	mean	std	min	max
Field-weighted citations (fwci)	26330.0	1.575	2.824	0.000	97.245
Novel interdisciplinarity (idr)	25792.0	0.542	0.156	0.000	0.891
Semantic distance	27194.0	0.053	0.024	0.004	0.354
Disruption index	15638.0	-0.004	0.016	-0.288	0.556
Average patent citations	28334.0	0.087	0.470	0.000	23.667
Average policy citations	28334.0	0.173	1.561	0.000	153.000

Figure 32 and Figure 33 report simple pre- and post-award mean differences in bibliometric outcomes with 95% confidence intervals. Figure 32 compares successful and unsuccessful applicants within the 1000 Ideas program, while Figure 33 compares successful 1000 Ideas projects to successful PI projects. In both cases, estimates are shown separately for the pre-award and post-award periods, providing a transparent visual benchmark for subsequent regression-based results.

Two patterns are immediately apparent. First, several outcome differences are already present in the pre-award period, particularly for interdisciplinarity, semantic novelty, disruption, and broader impact measures. This confirms that funded projects, especially within the 1000 Ideas program, are not randomly drawn from the applicant pool prior to the funding decision, underscoring the need for a Difference-in-Differences framework that explicitly nets out pre-existing differences when evaluating post-award outcomes.

Second, post-award mean differences do not systematically widen relative to their pre-award counterparts across most indicators. While some post-award differences are positive, most notably for interdisciplinarity and selected broader impact measures, their magnitude is generally modest and confidence intervals are wide. For citation-based indicators such as field-weighted citations, post-award differences largely mirror pre-award gaps, suggesting limited average treatment effects once baseline differences are taken into account.

Finally, these descriptive comparisons reinforce two key motivations for the econometric analysis that follows. They illustrate why simple post-award mean comparisons are insufficient to assess program effects, and they motivate the use of Difference-in-Differences and variance-focused analyses to disentangle selection from treatment effects and to capture changes in dispersion rather than shifts in average outcomes alone.

Figure 32 Mean differences between successful and unsuccessful 1000 ideas applicants

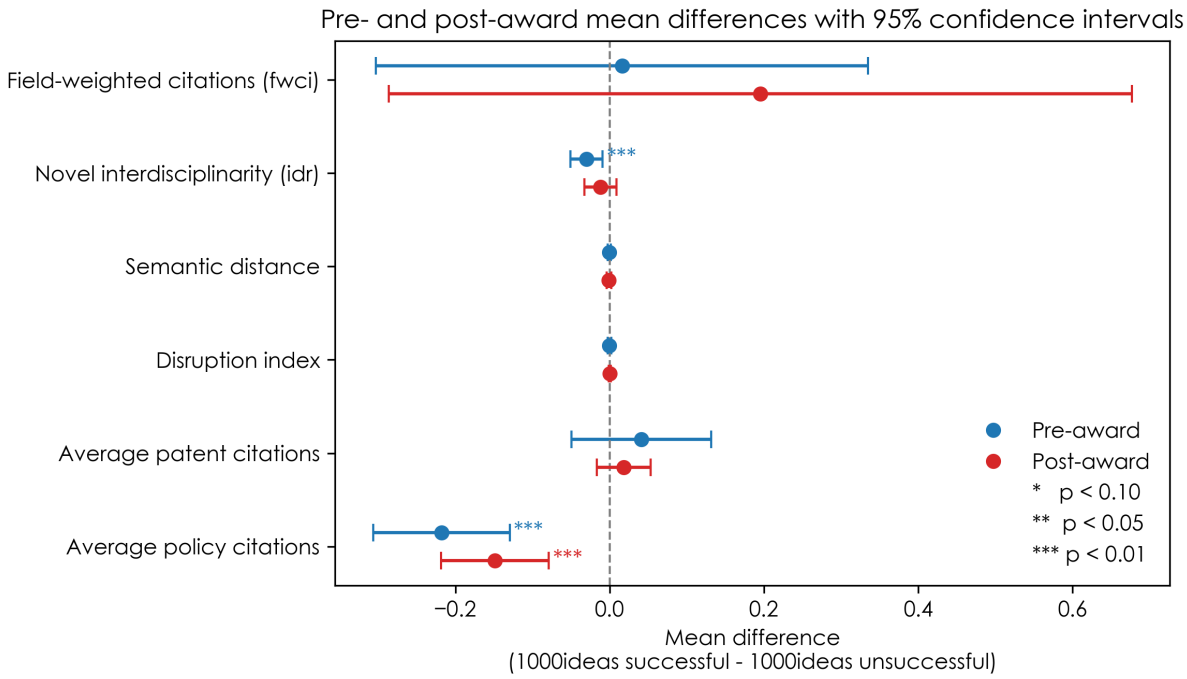
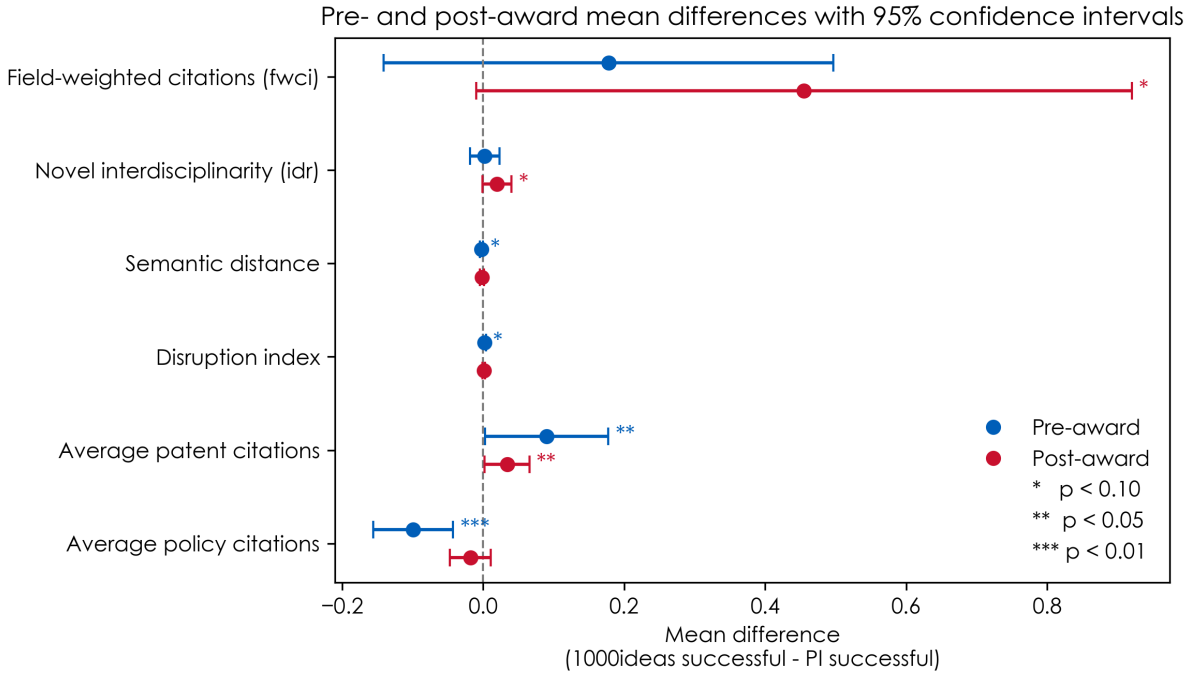


Figure 33 Mean differences between successful 1000 ideas applicants and successful PI applicants



C.4. Econometric analysis

This section presents the econometric analysis used to assess the effects of the 1000 Ideas program on research selection and post-award outcomes. Building on the descriptive evidence in section C.3, the analysis is designed to separate selection effects from post-

award dynamics, and to distinguish changes in average outcomes from changes in the dispersion of results.

C.4.1.Difference-in-Differences estimation

This section evaluates whether receiving funding under the 1000 Ideas leads to changes in research outcomes after the funding decision, relative to appropriate comparison groups. The central challenge is that funded and unfunded applicants differ systematically already before the decision, and that research outputs evolve over time even in the absence of funding. Difference-in-Differences (DiD) methods address this challenge by comparing changes over time, rather than levels, thereby netting out pre-existing differences and common time trends. The DiD framework asks if outcomes for funded projects improve more after the award than outcomes for comparable unfunded projects. Also test whether these post-award changes differ between the 1000 Ideas program and standard PI funding schemes. The method explores variation across time (before vs after the decision), funding status (successful vs unsuccessful), and program type (1000 Ideas vs PI). Therefore, it isolates post-award dynamics that cannot be explained by baseline researcher characteristics, disciplinary composition, or general publication trends.

The main econometric specifications use a balanced event-time sample, restricting attention to applications observed for the full −3 to +3 year window around the funding decision. This restriction ensures comparable exposure periods before and after the award and avoids conflating treatment effects with differences in observability arising from publication lags or right-censoring. Applying this restriction yields a balanced analytical dataset comprising 1,952 applications, corresponding to 1,159 unique applicants. Of these, 363 applications (329 applicants) belong to the 1000 Ideas program, while 1,589 applications (932 applicants) correspond to standard PI funding schemes. All subsequent Difference-in-Differences (DiD) and Difference-in-Difference-in-Differences (DDD) estimates are based on this balanced sample unless stated otherwise.

Table 19 DiD comparing successful and unsuccessful 1000ideas applicants

DiD 1000 Ideas: TET (successful vs unsuccessful)						
	(1)	(2)	(3)	(4)	(5)	(6)
	Field-Weighted Citation Impact	Novel Interdisciplinarity	Disruption Index	Semantic Novelty	Av Patent Citations	Av Policy Citations
ATET						
Treatment effect on the treated (TET)	0.507	0.027**	0.001	-0.002	-0.084	0.206***
	(0.369)	(0.013)	(0.001)	(0.002)	(0.085)	(0.078)
N	2471.000	2445.000	1885.000	2497.000	2541.000	2541.000

Table 19, above, reports the model estimates comparing funded and unfunded applicants within the 1000 Ideas program. Across outcomes, the estimates indicate limited average post-award effects on citation-based impact, disruption, cosine-distance novelty, and patent citations. By contrast, novel interdisciplinarity exhibits a positive and statistically significant treatment effect, with funded 1000 Ideas projects showing a higher post-award increase in interdisciplinarity relative to unsuccessful applicants. The validity of this estimate relies on the parallel-trends assumption, which requires that funded and unfunded applicants followed similar outcome trajectories prior to the funding decision. The reported pretreatment test fails to reject the null hypothesis of parallel trends ($F = 0.17$, $p = 0.68$), and the corresponding event-study plot (Figure 34) shows no evidence of systematic divergence in interdisciplinarity before the award. This supports a causal interpretation of the post-award increase in interdisciplinarity as reflecting program-induced changes rather than pre-existing differences or differential trends.

Figure 34 Graphical Diagnostic for Parallel Trends

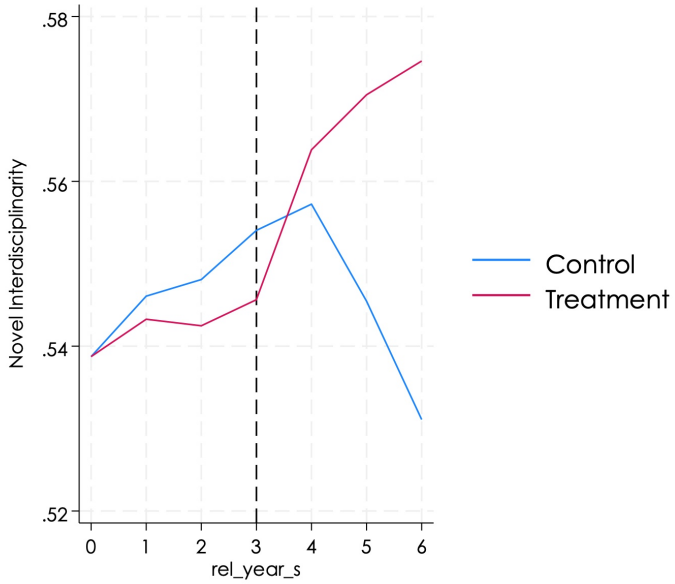


Table 20 DiD comparing successful 1000ideas applicants and successful PI applicants

DiD: 1000 Ideas vs benchmark						
	(1)	(2)	(3)	(4)	(5)	(6)
	Field-Weighted Citation Impact	Novel Interdisciplinarity	Disruption Index	Semantic Novelty	Av Patent Citations	Av Policy Citations
Post x 1000 Ideas	0.603	0.023*	0.000	-0.002	-0.147*	0.107*

	(0.380)	(0.013)	(0.001)	(0.002)	(0.083)	(0.057)
N	3330.000	3301.000	2410.000	3438.000	3479.000	3479.000

Note: Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.

Table 21 Difference-in-Difference-in-Differences (DDD) estimates of post-award effects

DDD: Post x Success x 1000 Ideas						
	(1)	(2)	(3)	(4)	(5)	(6)
	Field-Weighted Citation Impact	Novel Interdisciplinarity	Disruption Index	Semantic Novelty	Av Patent Citations	Av Policy Citations
post1=1 # successful=1 # is_1000=1	0.526	0.024*	0.000	-0.004	-0.102	0.140
	(0.389)	(0.013)	(0.001)	(0.003)	(0.086)	(0.101)
N	13133.000	12995.000	9562.000	13457.000	13664.000	13664.000

Note: Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.

The cross-program Difference-in-Differences (DiD) and Difference-in-Difference-in-Differences (DDD) estimates provide a consistent picture of average post-award effects. Across both specifications, there is no robust evidence of average effects on citation impact, disruption, or semantic novelty. In both models, novel interdisciplinarity shows a positive post-award effect, which remains statistically significant at conventional levels. Estimated effects for patent and policy citations are weak and not consistently robust across specifications. Taken together, these results indicate that while the 1000 Ideas program is associated with gains in interdisciplinary novelty relative, but does not systematically raise average citation impact.

Table 22 Logit Robustness Tests for Reduced (Balanced) Sample

Logit regressions predicting inclusion in balanced panel					
	(1)	(2)	(3)	(4)	(5)
successful	0.090	0.063	0.080	0.037	
	(0.065)	(0.098)	(0.101)	(0.171)	
successful=1 # is_1000=1					0.271

					(0.519)
Industry FE	No	No	Yes	Yes	Yes
Cohort	No	No	No	Yes	Yes
Pre-award outcomes	No	Yes	Yes	Yes	Yes
N	5486	2866	2829	1514	1514
Note: In (7) the omitted category is unsuccessful PI applications (successful = 0, is_1000 = 0). The interaction term 1.successful x 1.is_1000 captures whether the effect of funding success on balanced-panel inclusion differs between the 1000 Ideas and PI programs; for parsimony, only this interaction term is reported in the table. Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.					

Table 22, above, reports logit regressions testing whether inclusion in the balanced −3/+3 event-time sample is systematically related to funding success or program type. Across specifications, funding success is not a statistically significant predictor of balanced-panel inclusion, and the interaction between funding success and participation in the 1000 Ideas program is also insignificant. This holds after controlling for pre-award outcomes, disciplinary composition, and decision cohorts. These results indicate that restricting the analysis to a balanced sample does not induce differential selection by program or funding status, supporting the interpretation that the balanced-panel design primarily reflects exposure time rather than treatment-related changes in publication behavior.

C.4.2.Differences in Citation performance Variance: High-risk/High-Reward component

To assess whether the 1000 Ideas program is associated with greater post-award outcome variance, we complement the analysis of average effects with tests of residual dispersion. Specifically, we examine whether the variability of research outcomes differs systematically across programs and funding statuses after netting out average trajectories and time-invariant researcher heterogeneity. Residuals are obtained from a fixed-effects Difference-in-Difference-in-Differences (DDD) event-study model that allows outcomes to evolve flexibly over time by funding success and program type. We then compare the dispersion of these residuals across groups using robust variance tests, focusing on post-award years only. This approach isolates differences in outcome uncertainty that are not explained by mean effects or selection on observable characteristics, and provides a direct test of whether funding under the 1000 Ideas program is associated with a higher-risk, higher-reward outcome profile.

Table 23 Post-award residual variance in Field-Weighted Citations

Panel A. Program comparison (PI vs 1000 Ideas)

Sample	PI (SD)	1000 Ideas (SD)	Equality of variances (p-value, W0)
All projects	2.501	2.971	0.023**
Funded projects	2.454	3.009	0.000***

Unfunded projects	2.521	2.969	0.300

Panel B. Within 1000 Ideas (funded vs unfunded)

Sample	Unfunded (SD)	Funded (SD)	Equality of variances (p-value, W0)
1000 Ideas only	2.969	3.009	0.024**

Note: SD refers to the standard deviation of post-award residuals of field-weighted citation impact (FWCI). Equality of variances is tested using robust variance tests (robvar). Reported p-values correspond to the Brown–Forsythe-type statistic (W0) and are robust to alternative statistics, including the Levene-type test based on absolute deviations from the median (W50) and the Levene-type test based on absolute deviations from the trimmed mean (W10). Residuals are obtained from a fixed-effects DDD event-study model of three-year FWCI including author fixed effects and full interactions between event time, funding success, and program type. Variance comparisons are restricted to post-award years and balanced observation windows. Standard errors in the underlying regression are clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.

The results indicate that the risk profile of citation outcomes differs systematically across programs once funding is awarded. Among funded projects, residual dispersion in citation impact is significantly higher in the 1000 Ideas program than in standard PI funding schemes, consistent with a higher-risk outcome profile. By contrast, no statistically significant difference in dispersion is observed between programs among unfunded applicants, indicating that baseline risk is similar across applicant pools. Within the 1000 Ideas program, funded projects also display greater dispersion than unsuccessful applicants. Taken together, these findings suggest that the 1000 Ideas program accepts and enables a higher-risk, higher-reward research portfolio, with greater variability in realized outcomes arising from program-specific funding conditions rather than from pre-existing differences in applicant characteristics or trajectories.

C.4.3.Effects of Randomization on Selection

The final component of the econometric analysis examines how partial randomization within the 1000 Ideas program affects selection decisions. Unlike the analyses in previous sections, which focus on post-award outcomes, this section addresses the selection stage itself, asking whether the introduction of a lottery among fundable proposals reshapes the relationship between applicants' pre-award profiles and the probability of receiving funding. To this end, the analysis relies on averages of the bibliometric and text-based indicators measured prior to the funding decision. These indicators summarize applicants' past research trajectories and observable performance at the time of evaluation.

We examine whether randomization changes how proposals are selected. Specifically, we test whether randomization attenuates the role of conventional indicators of past performance, while increasing the weight placed on characteristics more closely aligned with the program's exploratory objectives. These other indicators include interdisciplinarity novelty, semantic novelty, disruption, and broader societal relevance. Identification exploits variation in lottery exposure among proposals deemed fundable, allowing the effect of a given pre-award characteristic on funding success to differ depending on whether the proposal is subject to random selection.

To this end, we estimate logit models of funding success that interact standardized pre-award indicators with an indicator for lottery selection. We complement coefficient estimates with average marginal effects to facilitate interpretation in terms of changes in funding

probabilities. Together, these models provide direct evidence on whether the lottery mechanism alters selection gradients in ways consistent with an underlying intent to reduce reliance on established performance metrics and to broaden the set of funded research trajectories.
The tables below report estimates from logit models of funding success that allow the effect of pre-award characteristics to differ on whether a proposal is subject to the lottery component. All continuous predictors are standardized (z-scores), and standard errors are clustered at the applicant level. The first table reports logit coefficients and interaction terms; the second table reports average marginal effects (AMEs), expressed as changes in the probability of funding associated with a one-standard-deviation increase in each predictor, separately by lottery status.

Across specifications, Table 24, below, shows that selection gradients differ between proposals decided under discretionary evaluation and those decided under lottery conditions. Under non-lottery selection, success is more strongly aligned with conventional or conservative signals, while novelty-oriented dimensions tend to be weakly selected against or not rewarded. Under lottery selection, these patterns shift: the association between past citation impact and success is attenuated, and novelty-related dimensions exhibit a more positive relationship with funding.

The AME results make these differences more transparent (Table 25). For proposals not subject to lottery selection, a one-standard-deviation increase in novelty-related indicators is associated with lower or unchanged funding probabilities. By contrast, among proposals in the lottery component, the estimated marginal effects for interdisciplinarity and disruption are positive and statistically significant, consistent with randomization reducing reliance on conventional performance signals and increasing the relative prominence of exploratory characteristics at the selection stage. Effects for cosine distance and policy citations are less precisely estimated, but the direction of estimates is generally aligned with this interpretation.
The tables provide evidence that the partial randomization mechanism changes how observable proposal characteristics relate to funding decisions. In particular, the lottery reshapes selection by weakening the role of established impact metrics and increasing the likelihood that applicants with more novel past research profiles are funded.

Table 24 Logit estimates of funding success with lottery interactions (selection-stage effects)

Logit regressions predicting funding success					
	(1)	(2)	(3)	(4)	(5)
Successful Application					
Field-Weighted Citation Impact	-0.029				
	(0.169)				
Selected in Lottery=1 # Field-Weighted Citation Impact	-0.872*				
	(0.493)				
Novel Interdisciplinarity		-0.363**			

		(0.162)			
Selected in Lottery=1 # Novel Interdisciplinarity		0.957***			
		(0.333)			
Disruption Index			-0.067		
			(0.354)		
Selected in Lottery=1 # Disruption Index			3.089		
			(1.889)		
Cosine Distance				-0.358*	
				(0.198)	
Selected in Lottery=1 # Cosine Distance				0.529	
				(0.497)	
Av Policy Citations					-7.224***
					(2.693)
Selected in Lottery=1 # Av Policy Citations					5.611
					(3.927)
N	792.000	780.000	686.000	783.000	792.000

Note: Continuous covariates are standardised (z-scores) using the full estimation sample. Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.

Table 25 Average marginal effects of pre-award characteristics on funding success, by lottery status

Logit AMEs: Effect of 1-SD increase in predictor on funding probability, by lottery status					
	(1)	(2)	(3)	(4)	(5)
	FWCI"	Novel Interdisciplinarity	Disruption Index	Cosine Distance	Policy Citations
main					
Selected in Lottery=0	-0.002	-0.020**	-0.004	-0.020*	-0.380***
	(0.009)	(0.009)	(0.020)	(0.011)	(0.146)

Selected in Lottery=1	-0.158**	0.105**	0.526*	0.033	-0.309
	(0.066)	(0.051)	(0.276)	(0.087)	(0.570)
N	792.000	780.000	686.000	783.000	792.000
Note: Continuous covariates are standardised (z-scores) using the full estimation sample. Standard errors clustered at the author level. * p<0.10, ** p<0.05, *** p<0.01.					

C.5. Conclusion

This analysis provides a comprehensive assessment of the 1000 Ideas program along three dimensions: post-award effects, outcome dispersion, and selection-stage mechanisms. Taken together, the results paint a coherent picture of a program that largely operates by reshaping what is funded and by enabling greater scientific risk-taking, rather than by delivering uniform gains in average research impact.

First, the DiD and DDD frameworks analyses show limited evidence of broad average post-award effects on conventional impact indicators such as field-weighted citations. Across specifications and comparison groups, estimated average effects are small and generally statistically insignificant. An important exception is interdisciplinarity novelty: funded projects under the 1000 Ideas program exhibit a robust post-award increase in interdisciplinary novelty relative to appropriate benchmarks. Parallel-trends diagnostics support a causal interpretation of this effect.

Second, moving beyond averages, the variance-based analysis reveals a distinct risk profile associated with the program. Among funded projects, post-award citation outcomes are significantly more dispersed in the 1000 Ideas program than in standard PI funding schemes, while no comparable differences are observed among unfunded applicants. Within the 1000 Ideas program itself, funded projects display higher residual variance than unsuccessful applicants. These findings indicate that the program enables a higher-risk, higher-reward portfolio: realized outcomes are more uncertain, with greater upside and downside relative to conventional funding, consistent with the program's nature.

Third, the analysis of selection mechanisms shows that partial randomization meaningfully alters how applicants' pre-award publication profiles translate into funding decisions. Under discretionary evaluation, funding success is more closely aligned with conventional indicators of past performance, such as citation impact. Under lottery conditions, these gradients are attenuated, and applicants with more novel past research profiles, in particular higher interdisciplinarity, disruption, and semantic distance, become more likely to receive funding. This pattern indicates that the lottery component reshapes selection by reducing reliance on established impact signals and broadening the range of prior research profiles, in line with the program's exploratory objectives.

Several limitations should be noted. Bibliometric indicators capture only observable outputs and may understate longer-term or non-publication impacts. Post-award observation windows remain constrained by publication lags, despite the use of balanced panels and robustness checks. As with any quasi-experimental design, causal interpretation relies on maintained assumptions, which are supported but not provable.

The evidence suggests that the contribution of the program does not primarily take the form of broad increases in citation impact, but it does manifest along two complementary margins. First, the program is associated with a robust increase in post-award novel interdisciplinarity, indicating that funding leads to measurable changes in the nature of research outputs rather than solely their volume or impact. Second, and more distinctly, the program alters the distribution of outcomes by enabling greater dispersion in realized impact among funded projects. Together, these results imply that the program both nudges research trajectories toward greater interdisciplinarity on average and accepts a higher-risk, higher-reward outcome profile, consistent with its exploratory design logic.

Appendix D Results of survey linked to bibliometric output

Survey participants were matched by name to their bibliometric output, allowing for the averages of their interdisciplinarity, novelty, and disruption to be computed. For each indicator, the average was taken using all outputs published in symmetric window from three years before to (up to) three years after the decision year.

These bibliometric indicators provide an insight into the academic output of the awardees and unsuccessful applicants to the 1000 ideas program, but do not provide a reflection on their proposal itself. Another caveat is that these averages are susceptible to bias due to small sample sizes.

6.4 13. Thinking about the outputs and the nature of your 1000 Ideas project, to what extent do you agree with the following statements?

6.4.1 *My project led (or will likely lead) to a fundamentally new concept or field of study*

Awardees who strongly agreed with this statement had more novel bibliometric outputs, but those who responded 'somewhat agree' had higher interdisciplinarity and disruption.

	Count	Average Interdisciplinarity	Average Novelty	Average Disruption
Strongly agree	23	0.50	0.053	-0.0028
Somewhat agree	27	0.53	0.046	0.0050
Neither agree nor disagree	5	0.56	0.041	-0.0029
Somewhat disagree	4	0.61	0.043	-0.0035
Strongly disagree	1	0.46	0.045	-0.0038
Don't know / prefer not to say	3	0.71	0.059	-0.0005

6.4.2 *My project resulted (or will likely result) in findings that contradicted established theories or conventional wisdom in my field*

Respondents who indicated that they somewhat agreed rather than strongly agreed in fact performed more strongly in all three indicators.

	Count	Average Interdisciplinarity	Average Novelty	Average Disruption
Strongly agree	10	0.48	0.044	-0.0018
Somewhat agree	25	0.55	0.053	0.0056
Neither agree nor disagree	10	0.50	0.045	-0.0034
Somewhat disagree	10	0.53	0.038	-0.0020
Strongly disagree	4	0.48	0.052	-0.0041

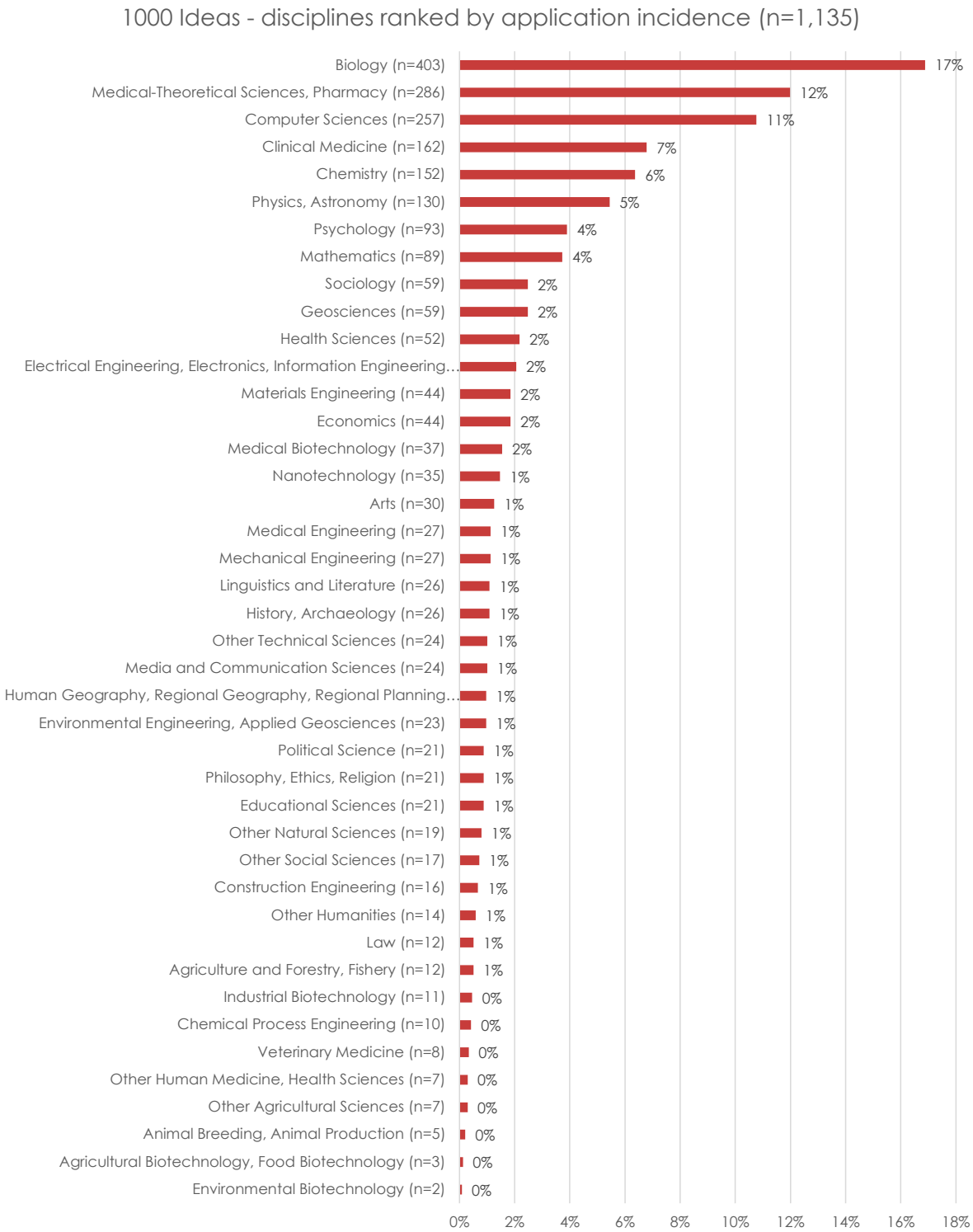
Don't know / prefer not to say	4	0.68	0.059	0.0001

6.4.3 *My project resulted (or will likely result) in critical follow-up questions for my field*

Grant awardees who indicated that they strongly agreed with this statement had higher average interdisciplinarity, novelty, and disruption scores than other respondents to this question.

	Count	**Average Interdisciplinarity**	**Average Novelty**	**Average Disruption**
Strongly agree	36	0.54	0.050	0.0026
Somewhat agree	19	0.51	0.047	-0.0019
Neither agree nor disagree	5	0.50	0.039	-0.0021
Don't know / prefer not to say	3	0.71	0.059	-0.0005

Appendix E Full list of disciplines in 1000 Ideas applications ranked by incidence



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