

Money, Time, and Grant Design

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Abstract

We conduct survey experiments to test how the design of scientific grants—the money and time awarded—can be used to manage researchers. On average, researchers are relatively unwilling to trade off money for time when choosing among grants. However, there is significant heterogeneity in researchers’ preferences. Conditional on receiving a grant, different designs cause only minor changes in researchers’ plans for how they would use the funding. Marginal changes in grant design are more relevant for selection effects (funders’ ability to attract certain researchers) than treatment effects (funders’ ability to change researchers’ strategies).

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1 Introduction

Businesses, governments, and philanthropies that fund research and development (R&D) must decide not only how much to invest in researchers, but how to structure the investments. Often, these investments are in the form of grants: “*upfront payments for the delivery of incompletely specified and non-contractable R&D output*” (Azoulay and Li 2021; pg. 120). There is growing evidence that R&D grants, as currently designed, can have large private and social returns — more grants lead to more innovation (Howell 2017; Azoulay et al. 2019b; Myers and Lanahan 2022; Santoleri et al. 2024). However, it is still unclear how alternative grant designs might be able to influence the rate or direction of innovation.

There are many considerations in grant design, from the application process to the final reporting requirements. In this paper, we focus on the effect of arguably the two most salient features of grant design: the amount of funding awarded (money), and the duration of time the funding is available (time). Incentive-based theories of innovation (e.g., Holmstrom 1989; Manso 2011) suggest that changing these two features could cause researchers to pursue different types of projects. That is, grant design could have a treatment effect on the types of strategies researchers pursue. However, grants are typically self-selected by researchers who have heterogeneous preferences, payoffs, and production functions. Thus, grant design could also influence the composition of researchers through selection effects.¹

Research organizations frequently emphasize the treatment effects of their grant designs.² But it is rarely noted that any correlation between grant design and outcomes observed in practice might instead be due to different designs attracting different types of researchers. The best evidence to date that grant design may be instrumental is due to Azoulay et al. (2011). They evaluate the publication output of biomedical researchers who receive larger, longer, and more flexible grants using matching methods and find patterns that are directionally consistent with theoretical predictions. However, experimental tests that randomize real-world grants are exceptionally rare and may continue to be prohibitively costly.

Separating selection and treatment effects is important here because managers and policymakers are typically working with a scarce supply of researchers (Goolsbee 1998). If a new grant program influences outcomes mainly through selection (i.e., attracting certain types

¹See Azoulay and Li (2021) and Carnehl et al. (2024) for relevant literature reviews.

²When the prestigious Howard Hughes Medical Institute (HHMI) extended their grants from five to seven years, they stated that the extra “*years of stable support allows [researchers] to take more risk and achieve more transformative advances*” (HHMI 2017). When the US National Institutes of Health (NIH) announced the Maximizing Investigators’ Research Award, which offered a new grant design with smaller amounts but longer durations, they stated that “*with more sustained support, investigators will have more freedom to innovate and explore new lines of inquiry*” (NIH 2014).

of researchers), the program’s effects may not persist; outcomes will change as new types of researchers enter the program. With a better understanding of the effects of grant design, managers and policymakers can choose the correct lever to pull — whether to focus on recruitment and selection effects, or incentives and treatment effects.

To make progress on this topic, we use a new survey to estimate researchers’ preferences and beliefs about the effect of grant designs. Our sample is a nationally representative collection of research-active professors spanning all major fields of science across all major academic research institutions in the US (Myers et al. 2023). We use hypothetical experiments embedded in the survey in order to answer the following empirical questions: (1) how willing are researchers to trade off the money and time included in a grant, and (2) conditional on receiving a grant, how do the money and time components of the grant affect researchers’ strategies? The first question informs selection effects, the second informs treatment effects.

First, we investigate selection effects by estimating researchers’ preferences over grant design. There is evidence that researchers respond to funding amounts when considering which grants to pursue (Myers 2020) and the timing of funding is important (Tham 2023; Tham et al. 2024), but there is no empirical work on researchers’ demand for grants with different designs. Science funders have attempted to solicit this sort of preference before using informal or qualitative approaches,³ but not in a quantitative way tied to an economic model as we do here. Researchers surely want more money and more time, but what is the relative importance of those two features? And while there is likely to be heterogeneity in preferences across researchers, what predicts researchers’ preferences and how large is this heterogeneity?

We answer these questions by estimating researchers’ willingness to trade off money and time. We start by assuming that each researcher i ’s indirect utility from a grant of dollar amount A and duration D is of the form $v_i(A, D) = \alpha_i A^\gamma D^{1-\gamma}$, where the key parameter is $\gamma \in [0, 1]$. A smaller γ would indicate a higher willingness to trade off money for time. We directly solicit researchers’ indifference points in a thought experiment using hypothetical grants with randomized attributes. On average, a researcher is only willing to trade off approximately \$50,000 from a grant to extend it by 1 year ($\gamma \approx 0.8$). We consistently estimate magnitudes on this scale under alternative assumptions (e.g., explorations of CES utility functions with different assumptions for the elasticity of substitution). This result implies that a 10% increase in grant size is valued roughly the same as a 45% increase in

³For example, see: [National Science Foundation \(2002\)](#); [Royal Society of New Zealand \(2004\)](#); [European Research Council \(2017\)](#).

grant duration — relatively speaking, money is much more important to researchers than time.⁴

We also test the extent to which researchers have heterogeneous preferences for money and time. We motivate these tests with simple models that predict the strongest preference for money, relative to time, among researchers who are more capital-intensive, can more easily access funding through other sources, are less risk-averse, have higher discount rates, and receive more direct utility from research grant funding (e.g., in the form of salary buyouts). When we split the sample along each of these dimensions, we find results consistent with our predictions. Importantly, these results show that manipulating grant design to change strategies will also induce selection effects that will generate changes in the composition of researchers being funded.

Our second empirical exercise studies how effective grant design could be in influencing researchers’ strategies. We ask researchers what strategic changes they would undertake if they received a hypothetical grant, the amount and duration of which is randomized. This hypothetical grant is posed as a sudden influx of funding, and we provide specific details to eliminate idiosyncratic features of grants that might contaminate researchers’ responses.⁵ This sort of unanticipated grant receipt is rare in practice; however, it allows us to isolate researchers’ responses to grant designs without any selection effects.

In the experiment, researchers respond to the randomized hypothetical grant by choosing from a menu of five subjective options, which are described in discipline-agnostic language, as to how the grant would lead them to change some strategic aspects of their work.⁶ Two options are based on the “risk-versus-speed” trade-off at the core of principal-agent models that seek to balance risk-taking against shirking with the use of long-term incentives (e.g., [Holmstrom 1989](#); [Manso 2011](#); [Lerner and Wulf 2007](#); [Aghion et al. 2013](#); [Tian and Wang 2014](#); [Barrot 2017](#); [Nanda and Rhodes-Kropf 2017](#)). Two options are based on the decision to start new research projects or expand ongoing work, which helps us investigate the degree to which grants may constrain researchers’ directions (e.g., [Jia et al. 2017](#); [Arts and Fleming 2018](#); [Myers 2020](#); [Hill et al. 2025](#)). Lastly, one option focuses on researchers’ willingness to

⁴Of course, this doesn’t imply that researchers’ preferences are perfectly aligned with the social optimum.

⁵For example, we emphasize that the funding may be used for any research project and the funding amount and duration are fixed and non-negotiable in order to rule out **no-cost extensions**.

⁶Specifically, the five options are: “Pursue riskier projects”, “Increase speed”, “Pursue projects less related to your current work”, “Increase the size of ongoing projects”, and “Increase accuracy or reliability”. We do not take a stance on the relative social value of any strategy. Instead our goal is to inform a policymaker or manager who already has preferences over these strategies. “Strategy” is a nebulous concept in the context of basic science and our formulations are not clearly mutually exclusive. But they are informed by extensive interviews with researchers across all major fields in the sample. Furthermore, the summary statistics and results of the experiment are consistent with researchers treating these options as relatively exclusive.

engage in activities that improve the accuracy or reliability of their work, which is motivated by the growing attention placed on so-called replication crises that may be arising because of constraints on researchers' time horizons or budgets (e.g., [Pashler and Harris 2012](#); [Smaldino and McElreath 2016](#); [Kasy 2021](#)).⁷

Our focus is not on the rate of researchers' output, but rather on the direction and nature of that output. Thus, we assume that the level of strategy any researcher employs is fixed (i.e., their choices are perfect substitutes) and focus on how the different designs incentivize substitution across the menu of strategies.

Through a combination of standard regressions and machine-learning methods that search for heterogeneous treatment effects, we find some statistically significant effects. Larger grants increase researchers' willingness to pursue riskier projects. Longer grants also increase researchers' willingness to pursue riskier projects, but only among tenured professors. All researchers are more likely to decrease the speed of their work when receiving longer grants. We also find results consistent with researchers exhibiting decreasing returns to the scale of their projects.

Overall, however, we estimate small treatment effects of grant designs. Realistic changes to designs (e.g., in the range of 15–30%) correspond to only 1–2 percentage point changes in the probability of researchers choosing certain strategies. Notably, our investigations into the potential for heterogeneous treatment effects rarely yield any statistically significant evidence of heterogeneity. Together, this suggests that the practical influence of marginal changes to any one grant that a researcher receives may be quite limited.

Before detailing the experiments, we pay considerable attention to the respondents' attention and the potential for non-response bias. As a test of attention, we compare researchers' self-reported annual salaries to those we observe on publicly available websites (only for researchers at institutions where such data is made public). We find strong agreement between the two, which suggests mostly truthful reporting along this dimension. While our survey experiments involve hypothetical scenarios, we are able to explore grant designs that are plausible but rarely seen in practice. While some of our measures are subjective, they provide a unique, ex-ante observation of researchers' beliefs in contrast to the typical approach in this literature, which relies on censored, ex-post observations (e.g., transformations of publication output).⁸

⁷We also ask researchers how they would spend the grant funds across a menu of input categories to investigate how certain inputs are connected to certain strategies.

⁸We follow a long line of prior work that has used surveys of researchers to uncover their otherwise unobservable features, choices, preferences, or beliefs (e.g., [Levin and Stephan 1991](#); [Fox and Stephan 2001](#);

We address the potential for non-response biases in two ways. First, we use data obtained for both respondents and non-respondents on the amount of research funding flowing to their institutions (sourced from the National Science Foundation) and, for a subset of the population, their publication, and grant flows. We estimate some statistically significant differences; however, these differences are all very small economically speaking, typically close to 5% in magnitude. Our second approach to potential non-response bias makes use of the randomized participation incentives and reminders in the survey. Both the incentives and reminders had a significant influence on researchers’ willingness to complete the survey, which provides us with the variation necessary to implement the sample selection correction method of [Heckman \(1979\)](#).

The paper proceeds as follows: Section 2 provides motivating summary statistics about the current distribution of research grants and some key institutional and financial details; Section 3 details the survey, including the population, sampling process, the survey instrument, and our approaches to testing for, and mitigating the potential of, non-response bias; Section 4 describes the first survey experiment, which solicits researchers’ preferences over grant attributes; Section 5 describes the second survey experiment, which tests how grant designs influence researchers’ strategies; lastly, Section 6 discusses our results in the context of related work and motivates future studies on tools for managing the rate and direction of innovation.

2 Grants in practice

2.1 Distribution of observed grant designs

Historically, the distribution of grants flowing to universities has been difficult to observe. Collecting and connecting data from the dozens of large government agencies and hundreds of smaller organizations is no small task, and surveying researchers about their full history of grants is not reasonable. Thankfully, the “Dimensions” data set provides a new look at what types of grants researchers are receiving by aggregating across a large number of funders who publicly report data ([Digital Science 2018](#)).⁹ Because it relies on publicly reported grants, the Dimensions data is limited in its coverage of research investments at universities by

[Stern 2004](#); [Sauermaun and Cohen 2010](#); [Curty et al. 2017](#); [Levecque et al. 2017](#); [Shortlidge and Eddy 2018](#); [Cohen et al. 2020](#); [Philipps 2022](#)). See [Stantcheva \(2022\)](#) for a full discussion of the trade-offs of survey-based empirical analyses.

⁹As of this paper, Dimensions sources grant data from 233 different US-based funders.

businesses, so here we can focus only on non-business grants.¹⁰

Figure 1 provides a new look at the distribution of grant designs across U.S. academic research institutions. It shows the joint distribution of the size and duration of research grants. For consistency, we restrict our attention to grants awarded to the institutions from which we sourced our survey population, which we detail in the next section. In short, this includes the 150 largest institutions of higher education in the US per the annual amount of R&D funding flowing to the institutions. We start with all 311,477 grants in the Dimensions data from 2000–2012 awarded to any researcher at all institutions in our population. We then restrict our attention to grants from US-based funders (295,004) with funding amount data (259,358). Of these, 222,041 are within the support of our thought experiments: 10 years long or less, and \$2 million or less.¹¹

A few patterns emerge. Grants longer than 6 years are exceptionally rare. Even small, long grants that might resemble some form of insurance are rare; less than 1.5% of grants are longer than 6 years and smaller than \$1 million. Another clear pattern is the positive correlation between the size and duration of research grants. On average, a grant that is one year longer will award about \$500,000 more in total.¹²

Overall, it is clear that the full space of reasonable grant designs has not yet been explored much. The patterns illustrated here may reflect some optimal staging of investments by funders looking to learn information about risky projects (Dixit and Pindyck 1994). However, it may also reflect a missing market for certain grants, possibly driven by the fiscal costs of long-term outlays or rules and regulations that restrict the duration of financial arrangements between funders and grantees.¹³

Below, we use our survey experiments to understand what might happen to the type of science and composition of researchers if funders start to change this distribution. Also, we use this observed data to make some novel inferences about funders’ preferences over grant designs and compare those to our estimates of researchers’ preferences.

2.2 Institutional details

Azoulay and Li (2021) and Carnehl et al. (2024) provide excellent reviews of scientific grants

¹⁰Recent federal survey evidence suggests that business-originated research funding accounts for roughly 5% of all annual research funding at institutions of higher education (US National Center for Science and Engineering Statistics 2021).

¹¹We focus on 2000–2012 to avoid censoring since duration is measured ex-post in this data. Funding is converted to 2022 dollars per the Consumer Price Index (US Bureau of Labor Statistics 2025).

¹²This also holds on a per-year basis; an additional year is associated with \$40,000 more per year.

¹³For example, see [here](#) for the NIH’s policy that limits grant “segments” to a maximum of 5 years.

more broadly. Here, we outline a few important details relevant to our experiments. Loosely speaking, there are four sets of parameters that govern the use of research grants as resources: (1) the total amount of money awarded; (2) the duration over which that money can be used; (3) the rate at which the money is awarded (i.e., dollars awarded per year); (4) rules about who is in control of the money and what it can or cannot be used to purchase. In our experiments, we focus on the first two elements—total amount of money and time—since these are both the most universally applicable and are relatively straightforward to quantify and manipulate. We attempt to shut down variation in the third element by posing all experiments as questions about the receipt of a grant where *all* of the money is awarded and available to the researcher immediately.¹⁴ We attempt to shut down variation in the fourth element by always specifying that the grants are “unrestricted”. Of course, each respondent will interpret this phrasing differently, but pilot tests with researchers indicated that it was a relatively efficient and clear way of communicating that the money would not be tied to any particular project or input.

An important institutional detail is that explicit borrowing and saving research funding appear to be very rare. We are unaware of any instances where a researcher has an explicit contract with their institution where they could, for example, borrow funding from the institution and, upon the receipt of a grant in the future, pay the institution back. None of our conversations with researchers have indicated that the institutions in our sample have the financial vehicles to substitute money across time in this way. Likewise, the reason why the duration element of grant design is important is because there is no “saving” of funds beyond the date of the grant.

Of course, implicit or relational contracts between researchers and their institutions may exist. For example, most modern research grants include “indirect costs” that flow to the institution directly. Thus, one can certainly imagine an equilibrium where researchers who are more productive at obtaining grants can more easily lobby for some funding directly from their institution as an implicit loan. These dimensions are interesting, but conversations with researchers suggested to us that they were of second order relative to the (simpler) issues of how much money and time each grant provides. Still, these (and many other) institutional features loom in the background of all of our respondents’ answers and so we interpret all of our results through the lens of the prevailing institutional environment, which we elaborate

¹⁴We also make it clear that the duration of the grant is fixed and non-negotiable, since it is common in practice to observe “no-cost extensions,” where funders make unused money available after the original duration of the grant. Unfortunately, we are unaware of any data that explicitly would allow us to report summary statistics on these dimensions. Discussions with researchers indicates that, even when grants are observed to flow to researchers in subdivided amounts on some regular basis (e.g., annually) that it is frequently feasible to access the full amount of money granted at any point during the duration specified.

on further in our discussion.

3 Survey, population, and sample statistics

3.1 Population and sampling

We make use of the National Survey of Academic Researchers, which is detailed in [Myers et al. \(2023\)](#).¹⁵ We provide a brief overview of the survey methodology here. The target population is US professors who conduct research at major institutions of higher education. This was formalized by selecting approximately 150 of the largest institutions in the US per their total R&D funding reported in the National Science Foundation’s 2019 Higher Education R&D (HERD) survey ([US National Center for Science and Engineering Statistics 2021](#)) and manually collecting professors’ information from public websites. Recruitment e-mails were sent to a random 50% of the population from October 2022 to March 2023.

The population consisted of 264,036 unique e-mails. We e-mailed a total of 131,672 individuals and 4,388 (3.33%) completed the survey.¹⁶ We then restrict the sample to the 4,175 individuals (95.1% of respondents) who reported being a professor and spending a non-zero amount of time on research. Our final sample consists of professors from engineering, math, and related fields (703), humanities and related fields (786; e.g., history, linguistics), medical sciences (1,165; e.g., schools of medicine or public health), natural sciences (655; e.g., biology, chemistry, physics), and social sciences (866; e.g., economics, psychology, sociology). We use these five aggregate groupings of fields throughout our discussion and empirical analyses given the small sample sizes within narrower field definitions. [Table 1](#) reports some key summary statistics for our sample. The summary statistics related to the survey experiments are reported in the sections below, and [Appendix A.1](#) contains a larger table with all covariates in the survey used in any of our analyses.

It is important to demonstrate that survey participants have some demand for funding for their research since the experiments are irrelevant to any researcher with no demand for funding (e.g., their only input is their own effort). To illustrate this, [Appendix Figure A1](#) plots the distribution of expected research funding over the five years from the time of survey forward for each of the five broad scientific fields. This is the sum of guaranteed funding the researcher has from either prior awards or guaranteed future funding flows plus their expected fundraising over the same period. The vast majority (>75%) of researchers in each

¹⁵An anonymized version of the dataset is publicly available [Myers and Tham \(2024\)](#).

¹⁶This response rate is more than twice what has been obtained from sourcing academic researcher contacts from the corresponding author data contained within the publication record (e.g., [Myers et al. 2020](#)).

field have non-zero funding expectations. Furthermore, each distribution shows significant support over the range studied in the experiments below; research grants are important across all of science.

One key institutional detail to note that is not reflected in these statistics is the rarity of any mechanisms for saving or borrowing grant funds. On the saving side, most grants are only fungible for a fixed period—this is the parameter we focus on when we vary the “duration” of grants in the experiments. In reality, anecdotes suggest that duration can be negotiated on the scale of one to two additional years through so-called “no-cost extensions”. In our experiment, we do our best to shut down this channel by explicitly stating that the duration is non-negotiable. On the borrowing side, we have never heard of any financial mechanism whereby a researcher could receive funds, for example, directly from their institution under expectation that they pay those funds back via future grant fundraising.¹⁷

3.2 Addressing potential survey biases

Attention and representativeness

Ideally, our respondents would report all answers truthfully and these responses would reflect the full population in terms of their preferences over, and responses to, different grant designs. We can never formally test this, but we can take some steps to investigate the possibility of inattention and non-response bias and, in the case of non-response bias, possibly account for it.

As a test of respondents’ attention and their willingness to report truthfully, [Myers et al. \(2023\)](#) compare respondents’ self-reported salaries to their publicly-reported salaries for the subset of researchers at institutions that make such data public. The two show a close degree of alignment with a correlation of about 0.75, and the difference between the self- and publicly-reported salary is less than 30% for roughly three-quarters of observations.¹⁸ This suggests the vast majority of respondents are responding truthfully along this dimension.

[Myers et al. \(2023\)](#) also report two comparisons of the population and sample professors using observable data on institutional funding levels and professor-level grant funding and

¹⁷Of course, institutions will vary in their willingness to grant researchers funding, and implicit or relational contracts between researchers and administrators may exist. But the net effect of this all will shape the responsiveness of researchers to our experimental levers, and this net effect is what is relevant to managers or policymakers seeking to influence researchers amidst the current institutional environment.

¹⁸Discrepancies between self- and publicly-reported salaries can be due to a combination of recall error, inattention, or the time lag between the publicly-reported salary and when the survey was taken.

publication output. In both cases, the respondent sample is quite similar to the population. The average difference in funding amounts between the institutions of respondents and non-respondents is generally in the range of 4–6%, and there are no statistically significant differences in the grant funding or publication output metrics between the two groups. Some of these exercises are replicated in Appendix A.2. Overall, our respondent sample appears very similar to the population along many observable dimensions.

Sample selection correction

Despite the overlap in observable data, our respondents may still differ on other unobservable dimensions. If these unobservable differences are related to our focal parameters of interest (e.g., researchers’ preferences over grant designs), this may generate a non-response bias. To account for this possibility, we implement the sample selection correction approach first developed by Heckman (1979). This approach makes use of excluded variables that cause entry into the sample (i.e., completion of our survey), but are plausibly orthogonal to the parameters of interest.

We generated the variation necessary for this approach with randomized recruitment strategies. During the recruitment process, we randomly allocated each e-mail to one of four different incentive arms and one of three different reminder arms. The four incentive arms were: (1) no incentive, (2) entry into a lottery to win a gift card, (3) the ability to vote for a set of charities to receive a donation, and (4) both the second and third incentives. The three reminder arms involved no, one, or two reminders, respectively.

Appendix A.3 reports the effect of the incentives and reminders. We find that stronger incentives lead to a small increase in completion rates, while the reminders are much more powerful. Both induce a significant increase in completion rates; the completion rate for researchers that receive no incentives and no reminders is roughly 1.9% whereas the completion rate for those who receive both incentives and both reminders is roughly 4.5%.

Using the randomized variation from these incentives, combined with the information on each professor’s rank, institution, and inferred field of study, we then estimated individuals’ propensity to complete the survey conditional on being sampled. From these predicted probabilities, we constructed the inverse Mills ratio and include this variable in all regressions. Assuming that researchers responsiveness to either participation instrument is orthogonal to our focal parameters, this will control for any unobservable differences (in a focal parameter) between our sample and the population (Heckman 1979).¹⁹

¹⁹Obviously, this approach does sacrifice some potential sample size. But using the full set of reminders and incentives in all recruitments would have eliminated our ability to implement any correction for potential

4 Researchers' preferences

4.1 Empirical model

Here, our focus is on researchers' preferences over grant designs and how, when given freedom to choose which grants to pursue, these preferences may lead to selection effects that influence the composition of researchers who pursue particular grants.²⁰

We assume that each grant g is defined by a pair of parameters (A_g, D_g) , which describe the total dollar amount A of the grant and the duration D of the grant, in terms of how many years the researcher has access to the funds. Researcher i 's indirect utility from a grant is:

$$v_i(A_g, D_g) = \alpha_i A_g^\gamma D_g^{1-\gamma} , \quad (1)$$

where α_i is a researcher-specific taste shifter and γ is a common parameter that describes scientists' relative preference for funding amount versus duration.

To motivate the analyses and arrive at our main regression equation, consider a survey experiment where each researcher i observes a pair of grants $g = (1, 2)$ where (A_{i1}, D_{i1}) and (D_{i2}) are randomly specified by the experimenter. The respondent is asked to report the value A_{i2} that makes them indifferent between the two grants such that $v(A_{i1}, D_{i1}) = v(A_{i2}, D_{i2})$. We assume that researchers' responses (\tilde{A}_{i2}) are the truth plus i.i.d. noise, such that $\tilde{A}_{i2} = A_{i2} + \epsilon_i$. This allows us to write:

$$\gamma \log(A_{i1}) + (1 - \gamma) \log(D_{i1}) = \gamma \log(\tilde{A}_{i2} - \epsilon_i) + (1 - \gamma) \log(D_{i2}) . \quad (2)$$

We obtain our main estimating equation by solving Equation 2 for \tilde{A}_{i2} :

$$\tilde{A}_{i2} = \frac{A_{i1}}{\exp\left(\frac{1-\gamma}{\gamma} \log\left(\frac{D_{i2}}{D_{i1}}\right)\right)} + \epsilon_i , \quad (3)$$

which we estimate via non-linear least squares.²¹

Our Cobb-Douglas formulation is a special case of the more flexible Constant Elasticity of Substitution (CES) form, where the Cobb-Douglas form assumes the elasticity of substitution

non-response bias.

²⁰The next analyses (in Section 5) focus on identifying the treatment effect(s) of grant design conditional on receiving the grant.

²¹Note that the design of this grant is designed to be uncorrelated with the design of the hypothetical grant in the first survey experiment. Amongst the pairwise correlations of the two grant attributes in the first survey experiment and the three attributes in this survey experiment, the absolute value of any correlation is never larger than 0.03.

between the two factors is equal to one. However, besides its simplicity, we don't have good a priori evidence as to why we should assume a unit elasticity here. To explore this assumption, we also report results where we estimate the γ parameter from a CES model given a range of alternative assumptions about the elasticity of substitution. To preview, we find similar results across a range of assumed elasticities spanning 0.5 to 2. Furthermore, we find the Cobb-Douglas unit elasticity assumption to perform well in terms of model fit.

Equation 1 is a reduced form representation of a more complex model of researchers' production and consumption decisions detailed in Appendix B.1. Comparative statics derived from stylized simplifications of that model illustrate how the γ parameter should be related to five key features: (1) the capital intensity of a researcher's work (i.e., their returns to scale with respect to funding), where more capital intensive researchers should have a larger γ ; (2) the ease with which the researcher can acquire additional funding through other means (i.e., their fundraising productivity, which determines their expected inter-temporal budget constraint), where researchers more capable at obtaining funding should have a larger γ ; (3) researchers' risk aversion, where more risk-averse researchers should have a smaller γ ; (4) the discount rate, where researchers that discount future periods more should have a larger γ ; and (5) researchers' direct utility from grant funding (e.g., in the form of salary buyout), which should be associated with an increase in γ . These are the dimensions along which we may see heterogeneity across researchers that would signal the importance of selection effects induced by grant designs.

Below, we use empirical proxies to test how much γ varies with these features. We do so by estimating alternative specifications of Eq. 3 where we assume γ is a linear function of a constant term and these features. That is, we assume $\gamma_i = Constant + \beta X_i$, and we estimate the *Constant* and the linear parameter(s) β . In these cases, we standardize the features to have a mean of zero and a standard deviation of one so that the *Constant* term can be interpreted as the estimate of the average γ_i in the sample (and so that our estimate of γ in the homogeneous parameter model should be approximately equal to our estimate for the *Constant*). Furthermore, this allows for an easier comparison of the estimates of the β parameter for each feature since each β will estimate the change in a researchers' γ_i given a one standard-deviation increase in the feature.

4.2 Survey experiment

To generate the data necessary to estimate Equation 3, we present respondents with a hypothetical scenario where an anonymous donor would like to award the researcher with

a single research grant.²² The donor presents the researcher two options: Grant #1 and Grant #2, where the first is set to be shorter than the second. The amount of Grant #1 is shown, but the amount of Grant #2 is not. The respondent is then asked to state the funding amount of Grant #2 that would make them indifferent between both grants.

Appendix A.4 contains a screenshot of how the question was posed to respondents. The framing and verbiage of the question were developed based on pilot tests with researchers from a range of fields and career stages. These pilot tests indicated that any confusion about the question was most likely to arise around the timing of funding flows (e.g., could they spend more money upfront instead of the same amount each year?) and the possibility of “renewals” or “extensions”. This motivated our choices to (i) simplify the funding flow to be a lump sum awarded immediately, which proved to be easy for researchers to understand,²³ and to (ii) eliminate the possibility of renewals or extensions where the funds could be used beyond the stated duration.²⁴

Relatedly, the pilot tests surfaced no potential concern for experimenter demand effects, and we have no hypothesis as to why such demand effects would occur in this experiment.

The amount of Grant #1 is a uniform random draw from {\$100,000, \$250,000, \$500,000, \$1,000,000, \$2,000,000}. The duration of Grants #1 and #2 are uniform random draws from {2, 3, 4, 5} and {6, 7, 8, 9, 10} years, respectively (and independently from the Grant #1 draw). Table 2 reports the summary statistics for the four key variables related to this survey experiment. The randomized elements have the expected statistics (e.g., the mean of the variables are approximately equal the mean of the distributions from which they’re drawn).

Table 2 also starts to illustrate the degree to which researchers are willing to trade off funding amount and duration. With Grant #2 being 4.5 years longer than Grant #1 on average, researchers report that they would be indifferent between both grants if Grant #2 is roughly \$165,000 smaller. This suggests researchers would be willing to trade approximately \$35,000 per year gained on a grant. Below, we report the marginal rates of substitution based on our estimation of the formal model of researchers’ utility.

²²The framing of an anonymous donor was chosen because some researchers may have (heterogeneous) preferences over funders’ identity for reasons related to prestige.

²³Researchers typically request a total budget in grant applications.

²⁴Specifically, the text reads: “Note: All funding awarded in the first year. After expiration, unused funds are forfeited, and no extensions are permitted.”

4.3 Average willingness to substitute money and time

Table 3 reports our estimates of γ obtained by estimating Equation 3 via non-linear least squares. When we estimate a single parameter that represents researchers’ average preferences (Col. 1), we estimate γ to be 0.77. This magnitude implies a marginal rate of substitution of approximately \$68,000 per year at the means of the Grant #1 distribution. This implies researchers value a 10% increase in funding the same as they value a 33% increase in duration. Relatively speaking, dollars appear to be more than three times as important as duration in the context of grant design.

Appendix B.2 reports results where we use the more general CES functional form and a range of assumptions about that elasticity. With elasticities spanning 0.5 to 2.0, we consistently estimate marginal rates of substitution on the scale of \$40,000–\$70,000 per year. Furthermore, our Cobb-Douglas specification appears to be near the local optimum of model fit that we identify when examining the RMSE of the models. These findings give us confidence that our results are not heavily influenced by our assumption on the elasticity of substitution.

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Appendix B.3 includes alternative versions of our main specification (Table 3 Col. 1). We explore specifications that drop outlier observations and include the sample selection correction control (i.e., the standardized inverse Mills ratio). Overall, the results are unchanged. The sample selection control is not statistically significant at conventional levels and the magnitude is small. This suggests that our sample is not comprised of individuals with especially high or low γ values compared to the population.

To visualize the magnitudes implied by our estimates, Figure 2 plots the indifference curves implied by our estimate of γ over the support of the experimental variation. The plot illustrates how, for grants less than roughly \$500,000, researchers are relatively unwilling to sacrifice any of those funds to extend the duration of the grant. Once grant sizes extend beyond \$1,000,000, this tradeoff becomes more meaningful.

4.4 Heterogeneous preferences

As outlined above, researchers may have heterogeneous preferences over money and time. Understanding this heterogeneity can shed light on the different types of researchers that

²⁵In a further exploration of our functional form assumptions, Appendix B.4 reports results from an alternative specification of the researchers’ utility function that does not assume constant returns-to-scale. This is done by leveraging another component of the survey, which we detail further in the Appendix. Still, we find that the relative value researchers place on funding amount versus duration is very similar to the estimates obtained using our main specification.

fundraising can attract using different grant designs (i.e., the selection effects). The model of researchers’ production and consumption in Appendix B.1 motivates five specific features of researchers that should be predictive of their preferences over money and time. To summarize the predictions, we expect the strongest preferences for funding amount (relative to duration) to be amongst researchers who: (1) discount the future at a higher rate; (2) are more funding-intensive in their production functions; (3) are less risk-averse; (4) receive more direct utility from grant funding; and (5) are more productive at fundraising for their research.

To test these predictions, we estimate from versions of Eq. 3 where we assume each researcher’s γ_i parameter is a linear function of proxies for these five features. To proxy for discount rates, we use researchers’ age. To proxy for funding-intensity, we divide researchers’ total expected funding amount by their time spent on research to generate a ratio of dollars (of total funding) per hour (of researchers’ time spent on research) that plausibly reflects how important funding is to the researchers’ work. To proxy for risk aversion, we use a survey question that asked researchers to report (on Likert scale spanning 0–10) how much risk they believe they take in their work. To proxy for researchers’ direct utility from funding, we use the share of their salary that is sourced via “soft money” (i.e., paid for directly by grant funding). Lastly, to proxy for fundraising productivity, we divide researchers’ expected funding in the next five years (excluding any guarantees) by the number of hours they expect to spend on fundraising in that same period to arrive at another ratio of dollars (of expected funds raised) per hour (of researchers’ time spent fundraising).

In Columns 2–6 of Table 3, we report estimates allowing γ_i to vary along each of these dimensions independently. All proxies are standardized so that the *Constant* term estimated is roughly equivalent to the average γ estimate for the full sample and all coefficients can be interpreted as the marginal change in γ_i from the mean associated with a one standard deviation increase in the proxy. In all cases, we confirm the predictions of the theory. Across the five proxies, a one standard deviation increase in the proxy is associated with a 0.012–0.020 increase in γ_i . Column 7 of Table 3 includes all five proxies simultaneously in order to estimate conditional associations. The proxies are clearly positively correlated, but the same qualitative conclusions remain.

In order to provide an illustration of the heterogeneity in researchers’ preferences, we use the estimates from the specification reported in Column 7 of Table 3 to predict each researcher’s γ_i parameter.²⁶ Figure 3 plots the distribution of researcher-specific marginal rates of substitution implied by our predictions for their γ_i . Although the absolute value of these marginal rates of substitution are all relatively low, the Figure illustrates important

²⁶We construct our estimate as the linear prediction: $\hat{\gamma}_i = \widehat{Constant} + \sum_X \widehat{\beta}_X X$.

heterogeneity. Some researchers are willing to substitute funding for time nearly twice as much as others, and our stylized theory of scientific production provides some guidance as to who these researchers are.

In Appendix B.6 we report some additional explorations of heterogeneity in researchers' preferences across major fields of study, their tenure status, and their gender. We find statistically significant differences across fields, but the magnitudes of the average differences across fields are relatively small when compared to the variation illustrated in, for example, Figure 3. We cannot reject a null hypothesis that tenure status and gender are predictive of researchers' preferences over money and time.

How well are funders' preferences for grant designs aligned with researchers' preferences? Having funders undertake a survey experiment such as the one described above was not feasible. However, in Appendix C we undertake two exploratory exercises to provide a preliminary view of funders' preferences. In both cases, we use publicly reported data on grant designs and make a strong assumption that the realized distribution of grant designs reflects only the funders' preferences (i.e., funders are monopsonists). First, we study the NIH's MIRA program (referenced in the Introduction), and compare it to the more traditional grant design at the NIH. Second, we analyze the broader distribution of grant designs across all of science (i.e., the data shown in Figure 1). The results from these exercises should be interpreted with caution, but they suggest that funders' substitution parameter (γ^{funder}) is on the scale of 0.4. Intuitively, funders should be more willing to trade off grant amount for duration since there are plausibly tighter constraints on their annual budgets than on their time horizons. When compared to researchers' preferences, funders appear to be providing researchers with a set of grant opportunities that are longer and smaller than researchers would prefer. Such a disconnect could be due to funders' desire to attract certain types of researchers along the dimensions we explored here. However, it may also be due to a belief amongst funders that they can directly affect the type of science that researchers pursue with their grant designs. We test this hypothesis in the next section.

5 Grant design and research strategy

5.1 Survey experiment

How might grant design affect researchers' strategies? To begin, it is useful to map the two focal grant design parameters, money and time, to the two levers that much of the literature on managing innovation has focused, financial slack and long-term incentives. In

many contexts, financial slack is often operationalized as free cash flow. Here, in the setting of academic research grants, the clear analogue is the amount of funding. Larger grants can enable the acquisition of more scientific inputs because funding for one’s research is quite costly to obtain. Academic researchers spend an average of about 15% of their time fundraising for their work (Myers et al. 2020), and success rates at major funding agencies are on the scale of 10–20% (De Vrieze 2017). Long-term incentives are often operationalized as the duration of employment contracts, the time-horizon of stock options, or the use of “golden parachutes”. Here, the analogue is the duration that grant funds are made available. Longer grants can change researchers’ expectations and uncertainty about their future funding, which may have important consequences. For example, Tham (2023) and Tham et al. (2024) show how interruptions to researchers’ funding expectations can disrupt their spending flows and their ability to retain skilled labor in their laboratories.

The structure of this experiment is as follows: first, we present the respondent with a hypothetical scenario where they have just been awarded an (unexpected) grant, the size and duration of which is randomized; then, we ask the respondent what the most important changes to their research strategies would be after they received this grant. For these hypothetical grants, the dollar amount and duration are independent random draws from $\{\$100,000, \$250,000, \$500,000, \$1,000,000, \$2,000,000\}$ and $\{2, 3, 4, 5, 6, 7, 8, 9, 10\}$ years, respectively, which aligns with the distribution of grant designs explored in the preference experiment.

Appendix A.4 contains a screenshot of how the scenario was posed to researchers. As with the experiment where we solicited researchers’ preferences, the framing and verbiage shown to researchers was informed by pilot interviews with a variety of researchers and is again structured as an unexpected award from an anonymous donor. We again frame the grant as one where the funding amount is provided immediately as a lump-sum award, and no extensions are not permitted (i.e., any unused funding is forfeited after the stated duration).²⁷

Notably, this sort of unexpected windfall of funding is rather rare in practice. While there are instances where researchers receive unexpected prizes that include research funding, most funding is obtained by researchers self-selecting into funding competitions. However, our goal with this exercise is to estimate the treatment effect of grant design on researchers’ strategies

²⁷The exclusion of renewals or extensions does eliminate a potential incentive whereby the researchers’ performance during the period of the grant may influence their ability to obtain another grant from the funder. However, pilot tests indicated a wide range of understandings about grant extensions and made us concerned about introducing heterogeneity in researchers’ responses due to their erroneous assumptions about the nature of any extension process. Furthermore, the general incentive, whereby a researchers’ performance influences both their short-term output and longer-term ability to obtain inputs, is still present in this experiment.

without conditioning on the selection process. That way, if grant design is an instrumental determinant of researchers’ strategies, this experiment (in combination with the previous experiment soliciting researchers’ preferences) can help us understand the degree to which the effect of grant designs might be through channels of selection versus treatment.

If grant design can affect researchers’ strategies, we should see researchers systematically making different choices about their strategic response to grants with different designs. To capture this, we present researchers with five possibilities of how they might change their strategies: (i) pursue riskier projects; (ii) increase speed; (iii) pursue projects less related to your current work; (iv) increase the size of ongoing projects; (v) increase accuracy or reliability.

These five options were motivated by prevailing theories and worded based on pilot interviews with researchers in order to arrive at simple, discipline-agnostic language. We don’t take a stance on the social value of any of these strategies. Rather, our goal is to understand the extent to which policymakers or managers might incentivize particular strategies with particular grant designs. And, as in the case of the preference experiment, there is no clear a priori reason researchers would view any of these strategies as more or less socially valuable. So, we are relatively unconcerned with the possibility of experimenter demand effects.

When presented with the hypothetical grant and these five options, respondents must choose the two “most important” changes they would make to their science if they received such a grant. Underlying our approach is an assumption that these strategies are perfect substitutes — researchers always have some fixed “quantity of strategy” and the only question is how their strategic focus is allocated across the five categories we offer.

We structured the question in this way based off of our pilot interviews. These pilots suggested that any quantification of strategic choices beyond binary indicators would be difficult to interpret and generate significant survey burden and, furthermore, unless respondents were constrained to select a subset of the responses, inferring the relative priority of their choices would be difficult. In what follows, we summarize the hypothesized effects of funding amounts and duration on researchers’ choice among the five strategic options.

Risk and speed

Regarding the effect of duration on risk-taking and speed, principal-agent and multi-tasking models predict that long-term reward structures can induce risk-taking at the potential cost of shirking or other forms of negative selection (Holmstrom 1989; Manso 2011; Hellmann and Thiele 2011; Nanda and Rhodes-Kropf 2017). This suggests that longer grants should

increase researchers' risk-taking and decrease their speed. In the same vein, models of investment with financing frictions tend to predict that positive shocks to net worth (e.g., cash flows) lower producers' risk aversion (Froot and Stein 1998; Krieger et al. 2022). This suggests that larger grants should increase researchers' risk-taking. To the extent that it is risky for researchers to slow their pace of work, these theories would also predict a reduced focus on speed in response to larger grants. The results of Azoulay et al. (2011) are consistent with these predictions focused on risk-taking, where HHMI-funded scientists (who have longer and larger guaranteed funding arrangements) exhibit a higher variance in their publication outcomes compared to matched controls.

New and ongoing projects

Conditional on changes in risk tolerance, researchers' decisions about how they invest grants in new and ongoing projects will reveal details about the net expected returns to grant funding over a project's life-cycle. These net returns will be determined by researchers' input costs, output production, and risk-adjusted payoff expectations as more grant resources are invested in a project. On the cost side, studies of R&D at firms tend to suggest there are large adjustment costs (Hall 2002), with academic researchers behaving as if they face large adjustment costs as well (Myers 2020). This suggests the possibilities of convex returns to investment in new projects. On the production side, the complexities of modern science suggest there are many opportunities for scale economies and positive externalities that could give rise to unconventional relationships between inputs and outputs. Still, many inputs to science are scarce, which suggests that many projects will face decreasing returns to scale. In Appendix D.5, we use a simple model to predict how the relative magnitude of our coefficients related to these two strategies will imply different functional forms of the within-project returns to grant resources. In short, our results will allow us to infer whether the within-project returns to scale are one of three forms: (i) constant or increasing; (ii) decreasing, or (iii) non-monotonic.

Accuracy

Models of racing in science suggest that the priority reward structure has the potential to incentivize low-quality pursuits of high-value research questions (Bryan and Lemus 2017). This sort of competitive pressure appears to be relevant in practice, especially when researchers are financially- or time-constrained, leading to replication concerns, biased statistical reporting, and low quality experimental designs (Pashler and Harris 2012; Smaldino and McElreath 2016; Andrews and Kasy 2019; Hill and Stein 2021; Bryan et al. 2022). There continue to

be numerous calls for new institutions and incentives to address these issues,²⁸ but whether grants with certain designs may be able to counteract this pressure remain unclear, and this motivates the “increase accuracy or reliability” option.

Validated, ex-ante measures of strategy in the context of science do not yet exist, but each of these options are clearly motivated by prior studies of researchers. An obvious limitation here is that these are subjective descriptions of a subset of all possible strategies researchers could undertake. Furthermore, while formal models can draw sharp distinctions between these concepts, they clearly have some potential overlap and, as subjective concepts, are not necessarily exhaustive or mutually exclusive.²⁹ Still, the traditional approach of making inferences from researchers’ output (e.g., publications) has its own set of drawbacks,³⁰ and so we view this approach as a new, complementary one.

Table 4 reports the summary statistics for the randomized and response variables in this survey experiment.

5.2 Empirical approach

To explore the effect of grant design on researchers’ probability of choosing a particular strategy, we estimate five regressions of the form:

$$Y_i^k = h^k(A_i, D_i, \mathbf{X}_i) , \quad (4)$$

where the Y is an indicator that equals one if strategy k is chosen by the researcher in response to the randomized grant, the funding amount and duration are A and D , and \mathbf{X} is a vector of covariates solicited elsewhere in the survey (see Appendix A for a full listing of these covariates and their summary statistics). We explore a range of functional forms of h . In the simplest scenario, we test only the main effects of the randomized grant amount and duration and estimate five separate OLS regressions using each of the five indicators as the dependent variable. In the appendix, we also report results from OLS regressions after using Lasso to select controls from the rest of the survey data to possibly improve our precision as well as estimates from a single discrete choice model that jointly estimates how researchers’

²⁸For example, see [this](#), [this](#), or [this](#) commentary and coverage.

²⁹To this point of mutual exclusivity, we explored some alternative specifications where we grouped the questions and, in general, found no evidence suggesting that researchers treated answers as being highly similar.

³⁰Those measures are ex-post realizations and may suffer from a truncation bias when failed projects do not produce observable output. For example, see [Franzoni and Stephan \(2022\)](#) for a discussion of the difficulties of quantifying risk-taking in science.

propensity to choose a particular strategy depends on grant attributes.³¹

For clarity, consider our OLS regression specification:

$$Y_i^k = \alpha^k + \beta_A^k A_i + \beta_D^k D_i + \epsilon_i^k, \quad (5)$$

which we estimate for each of the five strategies indexed by k . Written this way, our coefficient estimates (β_A^k, β_D^k) will sum to zero across the five models (i.e., $\sum_k \beta_A^k = 0$); again, we are only estimating how grant design leads to substitution across these options. Positive values of our coefficients indicate that, as grants get larger or longer, researchers increasingly prioritize some strategy because, with those additional resources, that strategy has become marginally more useful to the researcher. Conversely, negative values indicate which strategies researchers substitute away from as grants get larger or longer. Our statistical tests of the null are therefore tests of whether or not researchers systematically substitute to or away from particular strategies.

We are particularly interested in the possibility of heterogeneous responses given the idiosyncrasies of each researcher’s situation. To do this, we use the causal forests algorithm developed by [Wager and Athey \(2018\)](#) to estimate conditional average partial effects (CAPEs) of the grant attributes. This approach yields informative distributions of effects, but do not directly reveal the features of researchers associated with larger or smaller CAPEs. To understand the heterogeneity in a more low-dimensional manner, we then use the Best Linear Projection methodology to test how the CAPEs vary with a select number of covariates ([Chernozhukov et al. 2018b](#)).³²

5.3 Results

Table 5 reports our simplest version of the analyses where we conduct five separate OLS regressions of strategic choice indicators on the grant attributes.³³ We report conventional robust standard errors as well as the family-wise p -values, which adjust for the fact that we are testing ten hypotheses (five outcome variables and two independent variables).³⁴ Appendix D reports the results from a logit choice model that jointly estimates researchers’

³¹Due to software limitations and the format of the question, we could not randomize the order in which the strategy options were presented to respondents and so we make no causal inferences from the estimated values of the intercepts in the regressions.

³²All of these analyses are performed in R using the `grf` package ([Tibshirani et al. 2022](#)).

³³Since the independent variables are identical in each regression, estimating each OLS regression separately is identical to joint estimation via , for example, seemingly-unrelated-regression approach.

³⁴We make use of [Jones et al.’s \(2019\)](#) implementation of the free step-down procedure of [Westfall and Young \(1993\)](#) as codified in their Stata package `wyoung`.

propensities to choose each strategy. It more closely reflects the structure of the choice facing the respondent, but it yields very similar results. Likewise, the post-Lasso OLS regressions reported there, where covariates are included to potentially improve precision, also show very similar results. Appendix Figure D1 reports the results from using causal forests to estimate the Conditional Average Partial Effect (CAPE) distributions.³⁵

Three patterns emerge, which we investigate and discuss further in the sub-sections below. First, we find that larger and longer grants lead researchers to prioritize speed less, but that such grants can also increase researchers’ willingness to take risks. Second, as grant size increases, researchers shift their strategic focus *towards* increasing the size of ongoing projects and *away* from starting new projects. Third, grant designs appear to have no effect on researchers’ focus on the accuracy of their science.

One might also hypothesize the existence of an interaction effect whereby money and time influence researchers in a non-separable way (e.g., the effect of duration is increasing in the amount of money). Notably, our exploration of heterogeneous treatment effects of each parameter includes the other parameter as a potential determinant of heterogeneity—we let the causal forests tell us if there is a significant interaction term. We find no evidence for this sort of interaction effect.

Risk and speed

The first column of Table 5, clearly shows that larger and longer grants lead to a decreased emphasis on speed (equivalently, smaller and shorter grants lead to an increased emphasis on speed). This result is intuitive, but by no means guaranteed given the wide array of time horizons over which researchers are likely optimizing (e.g., due to the heterogeneity in tenure status, contract lengths, ages, etc.). The socially optimal pace of science is far from clear given that there are good reasons both that science moves “too fast” (e.g., as in the racing models of Loury (1979), which has some empirical support from Bryan et al. (2022)), and “too slow” (e.g., as is emphasized by Nanda and Rhodes-Kropf (2017) who study how the provision of long-term incentives affects the extensive margin of innovation). Still, it does suggest that grant design is a tool for manipulating researchers’ speed.

The large body of work showing how long-term incentives can induce risk-taking in firms (Lerner and Wulf 2007; Aghion et al. 2013; Tian and Wang 2014) and the analogous results of Azoulay et al. (2011) suggest that we should find some increased willingness to take risks when receiving longer grants. But we do not see an average effect of this sort (see Column 2

³⁵The means of each CAPE reported in Appendix Figure D1 are approximately equal to the mean effects reported in Table D.

of Table 5). This result aligns with recent work by [Carson et al. \(2022\)](#), who study business managers’ hypothetical choices over innovation projects and find that, on average, explicit incentives for risk-taking are relatively ineffective and most people are simply too risk-averse for the incentives to have an effect.

However, the CAPE distribution in Appendix Figure D1 Panel (b) clearly shows a large degree of heterogeneity in researchers’ responsiveness to grant duration with respect to risk-taking. This particular relationship is one of only two CAPE distributions that represent statistically significant degrees of heterogeneity. To better understand this heterogeneity, Appendix D.3 reports the Best Linear Predictors of the CAPEs. The best predictor of researchers’ responsiveness to grant duration with respect to risk-taking is their tenure status — the effect of a log-point increase in grant duration on researchers’ willingness to take risks is roughly 10 percentage points larger for tenured professors compared to nontenured professors (compared to statistically insignificant mean effect of 0.01). This suggests that grant duration may be a useful incentive for risk-taking as the aforementioned theories would suggest, but only amongst researchers who have the complementary job security, reputation, or resources associated with tenure. Conversely, nontenured researchers may be facing performance evaluations on timescales that render marginal changes in grant duration irrelevant.

This result is consistent with [Azoulay et al. \(2011\)](#), in that their sample consists entirely of elite researchers who may already have, or are clearly on a path to, a tenured or de-facto tenured position, but are still on the younger end of the age spectrum. Our findings help guide the generalizability of those results and identify the conditions under which researchers might be incentivized to take risks with longer grants.

New and ongoing projects

The third and fourth columns of Table 5 indicate the grant duration has no effect on researchers’ decision to start new projects or expand the size of ongoing projects. However, we do find that larger grants lead researchers to shift their focus away from starting new projects and towards the expansion of ongoing projects. Equivalently, as grants get smaller, researchers are more likely to use those smaller amounts of funds to start new projects. The motivating model described in Appendix D.5 suggests that this result is indicative of researchers exhibiting decreasing returns to scale within projects.

Taken together with our results on risk, we find that scientists are willing to take more risks when they have more funding, but those risks are still based on their ongoing work, rather

than risks from venturing into wholly new territory. This is consistent with the idea that it is relatively difficult to induce scientists to change the direction of their research (Myers 2020), while also suggesting that scientists’ reluctance may not primarily be about risk.³⁶ It also points to the importance of distinguishing between risk and other aspects of innovation such as novelty that intuitively appear to be related to risk, but are conceptually distinct and in practice may operate differently (Azoulay and Greenblatt 2025).

Our search for heterogeneous responses did find significant differences in researchers’ willingness to start new projects as a function of grant amount (see Appendix Figure D1 Panel c). This suggests there is significant heterogeneity in the costs and/or benefits of researchers’ initial work on a project. Loosely speaking, it appears that there are meaningful differences in how easily researchers can learn about the quality of their new ideas — some researchers exhibit much high returns to initial investments in a project than others. Still, researchers are indicating that, overall, they perceive there to be decreasing returns to scale within their projects. This finding is relevant for policies that seek to limit the amount of grant funding that researchers can obtain, and suggests future work to more directly estimate the degree of these decreasing returns to scale would be worthwhile.³⁷

Accuracy

The least popular strategic choice overall is the *increase accuracy or reliability* option, and there are no average or heterogeneous effects to suggest that grant design can shift researchers’ choices along this dimension. The importance of preventing and detecting false positives and negatives in science has come to be appreciated (e.g., Pashler and Harris 2012; Andrews and Kasy 2019). Our results suggest the reason researchers may be underinvesting in efforts related to accuracy or reliability is *not* because they lack the resources to do so, but is because those efforts are underincentivized in the market for science.

5.4 Grants and input allocations

In the preceding analyses, we find that grant design can affect researchers’ strategies (i.e., grant size influencing the choice of new directions versus larger ongoing projects). How much of this responsiveness is due to the grants allowing researchers to obtain new inputs for their work? Or, more generally, how much might grant design influence researchers’ input choices?

³⁶However, other notions of uncertainty such as Knightian uncertainty may still be at play here.

³⁷We cannot speak to researcher-level returns to scale, so we cannot speak to policies that limit the total amount of funding a researcher obtains.

A full investigation into such questions is beyond the scope of this paper, but we did solicit a second set of outcomes in this survey experiment, which allows us to estimate how grants with different designs are used. In the survey, after respondents were asked to report how their strategies would change in response to receiving the hypothetical grant, we asked them to report how they would spend those funds across five different input categories: (1) their own salary; (2) their own training; (3) travel; (4) equipment, data, or supplies; and (5) salaries or wages for other new or current researchers, students, or staff.

Pilot testing with researchers indicated that soliciting precise allocations across these categories (e.g., percentage of dollars spent) was an excessive burden and would likely involve a tremendous amount of uncertainty since most researchers do not have specific knowledge about input costs. As a lower-burden alternative, we simply asked respondents to indicate if they would spend “None”, “Some”, or “A lot” of the hypothetical grant funds on each category; researchers were much more comfortable with this format.

To conduct empirical analyses of the responses, we code the three possible responses to values of 0,1, and 2. Then, we convert these values into shares by dividing each response by the sum of the responses across all categories (e.g., if a researcher reports “Some” for all categories, then all categories will receive a 0.2 share of the funding from the hypothetical grant). As with our analyses of researchers’ strategic responses, we are less concerned with the precise magnitudes of the effects and instead our focus is on testing whether we can reject the null in particular directions.

Table 6 shows the summary statistics for the share of funding that researchers allocate to each of the five inputs. For simplicity, we report the results from five separate OLS regressions using the input shares for each category as the dependent variables in Table 7.³⁸ Overall, grant duration has no significant effect on input allocations. But we do find grant size to have some clear effects.

First, we see that larger grants lead researchers to allocate a greater share of funding toward both their own salary and their own training. By construction, this increase must come with a decrease elsewhere, and it appears that almost all of the decrease is occurring in the “Others’ salaries” category. To be clear, this does not mean that researchers are claiming to reduce their total spending on this category. Instead, they are indicating that, as grants get larger in size, a smaller share of the additional funding will be spent on others’ salaries or wages.

³⁸Unreported results from joint estimation of all choices via a fractional response regressions with a logit model for the conditional mean yield very similar results.

We cannot test whether this shift in allocations is due to supply constraints (e.g., researchers cannot attract additional labor to work with them), or whether this is due to researchers' demand for labor shrinking as grant size increases (e.g., their own time is the main constraint that the increased funding allows them to address).

The fact that we find larger grants increase researchers' spending shares on themselves (i.e., salary and training) and decrease spending shares on others (e.g., students, staff scientists) while simultaneously decreasing their likelihood of pursuing new research directions suggests a connection between strategies and inputs. This relationship is consistent with researchers' teams, their students and staff, being a key part of helping them explore new research directions.

We lack the variation or data to dig further into this question to disentangle how much these results are driven by the supply- or demand-side, and we cannot formally connect specific inputs to specific strategies given the variation in our data.³⁹ Still, recent work has begun to highlight how the wages and employment outcomes of researchers' teams can be influenced by funding shocks (Tham et al. 2024), and continued work on how inputs and strategies are connected within the scientific production function seems especially promising.

6 Discussion

Understanding how grant design can be used to manage science requires understanding the incentives and institutions that transform inputs and outputs into objects that researchers value (e.g., job security, salary, prestige). Practical examples of such factors include researchers' taste for science (Stern 2004; Roach and Sauermann 2010), the tenure process and output measurement schemes (MacLeod and Urquiola 2020), intellectual property regimes (Hvide and Jones 2018), the nature of competition within fields (Hill and Stein 2019), gate-keeping (Azoulay et al. 2019a), and other social factors more generally (Shapin 1995). Our analyses provide new insights about grant designs given the incentives and institutions surrounding researchers. We investigate both the selection and treatment effects that can be induced by grants with different designs.

Since the vast majority of grants require researchers to self-select their grant pursuits, the usefulness of grant designs depends both on these treatment effects as well as the presence of

³⁹It is tempting to devise a two-stage analyses where we test how grant design influences input choices, and in turn, how these input choices effect strategic choices. However, our randomized grant designs are not valid instrumental variables because grant design may influence other features of the researchers' production functions besides these specific input choices (i.e., we cannot guarantee that the exclusion restriction holds).

any selection effects — grant designs may change the number and composition of researchers who pursue them. To this point, our first survey experiment sheds new light on researchers’ preferences over grant design. Our results indicate that while there is some willingness by researchers to trade off grant size (total grant \$) for duration (years grant \$ are available), they value relative changes in size nearly four times as much as changes in duration. The γ parameter we estimate subsumes several structural parameters, and our heterogeneity analyses indicate there is some significant variation in preferences across researchers. Disentangling this heterogeneity further to better understand selection effects would require a much richer model and more complicated experiments. For example, we use age as a proxy for researchers discount rates, but it would be important to disentangle researchers’ actual discount rates versus dynamic incentives they face (e.g., due to career concerns) over their life-cycle.

Our results do support the existence of treatment effects; grants appear to affect researchers’ strategies. As predicted by prior theories, longer grants increase researchers’ plans to take risks in their work, but only if the researcher has the job security of tenure. This finding aligns with the work of [Azoulay et al. \(2011\)](#), but provides new evidence as to some important boundary conditions. Furthermore, these longer grants reduce *all* researchers’ focus on speed. More work remains to be done on the connection between these two strategies and its implications for optimal grant design.⁴⁰

When scaled to reflect the degree of grant design variation we see in practice, these treatment effects are small. In the example of the NIH’s use of MIRA grants versus traditional R01 grants (where the MIRA grant is roughly 15% smaller and 30% longer than the R01 grant), our estimates imply that receiving one or the other would only change researchers’ probabilities of choosing certain strategies by about 1 percentage point. Overall, our results from our two experiments indicate that the selection effects of grant design and how they might influence the composition of researchers in an area are likely more important than the treatment effect of how those grant designs might influence *how* researchers conduct their science.

More generally, our results are informative for ongoing debates about the optimal distribution of science. It is frequently claimed that researchers do not take enough risks, do not explore enough new ideas, and are not careful enough in their work, and that a key source of these distortions is the suboptimal design of R&D grants ([Thompson 2022](#); [Ford 2024](#)). But our

⁴⁰See [Nanda and Rhodes-Kropf \(2017\)](#) for a discussion of how alternative innovation policies can influence the types of projects funders are willing to support, and the importance of understanding how this selection process influences the extensive margin.

results suggest that, to the extent the direction of science is distorted, it may be that the key frictions are located in other parts of the market for science, such as the herding incentives that arise in the context of cumulative innovation (Bryan and Lemus 2017; Hopenhayn and Squintani 2021) or the misaligned incentives that arise due to our limited ability to measure output (Wang et al. 2017; Bhattacharya and Packalen 2020; Hager et al. 2024). Of course, we cannot speak to how broader, general equilibrium changes in the funding landscape (e.g., where all grants in a field have more or less money or time) might influence the direction of science.

A key limitation of this paper is that we rely on non-incentivized stated preferences from a small sample of researchers. As noted by Stantcheva (2022), this allows us to “create our own variation” and measure variables that are difficult or impossible to observe in existing datasets (e.g. bibliometric data). Nonetheless, this comes at the cost of having to assume that these stated preferences are informative about actual behavior and that our sample is representative of the population. Our investigations into respondents’ attention and accuracy (using salary comparisons) and their representativeness (using data comparisons and sample selection correction techniques) suggest that these issues may not loom large in this specific survey. However, our survey experiments ask researchers to consider consequential decisions on a much faster timescale (e.g., seconds, minutes) than they do in practice (e.g., days, weeks). Thus, to the extent that it is costly to mentally engage with the longer-term implications of choices, our results may be biased and reflect larger discount rates than researchers exhibit in practice (e.g., Deck and Jahedi 2015).

Still, our results indicate that grants can be instrumental, and that the large costs of field experiments with stronger guarantees of representativeness may be worth the investment to better understand how grant design can be used to manage science. We also hope that our focus on measuring researchers’ ex ante strategies motivates future work to better understand how researchers choose their pursuits and allocate their resources. The vast majority of empirical studies of science make ex-post inferences about researchers’ strategies (e.g., using publication outcomes), but our approach illustrates an alternative. Clearly, obtaining data on researchers’ ex-ante strategies is challenging, but it may prove essential for understanding the ways in which researchers’ intentions ultimately lead to the outcomes of interest.

The findings also motivate some potentially interesting questions related to the plausibility and usefulness of allowing researchers to borrow or save grant funding, in the financial sense of those terms. To our knowledge, such practices are extremely rare. However, the heterogeneity we identify in preferences for money versus time suggests that there may be individuals willing to use such a mechanism that would allow them to, for example, borrow

against future grant funding. Of course, this is highly hypothetical and one immediately encounters a number of challenges given the structure of grants in practice. In that vein, our results on researchers having strong preferences for money over time may be connected to the broader phenomenon whereby people seem to save less (e.g., via annuities) than standard discounting models would suggest ([Benartzi et al. 2011](#)). More inquiry into researchers' time horizons and new financial vehicles for science funding is surely warranted.

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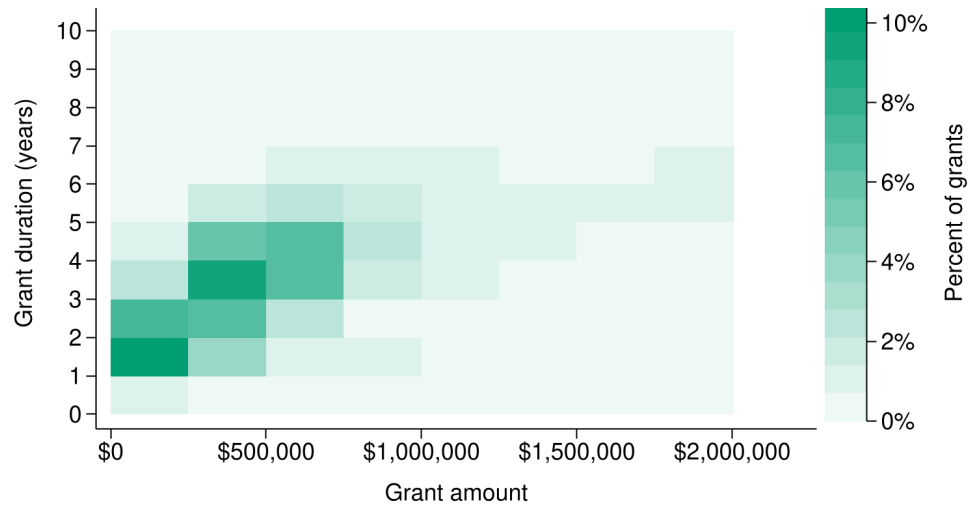
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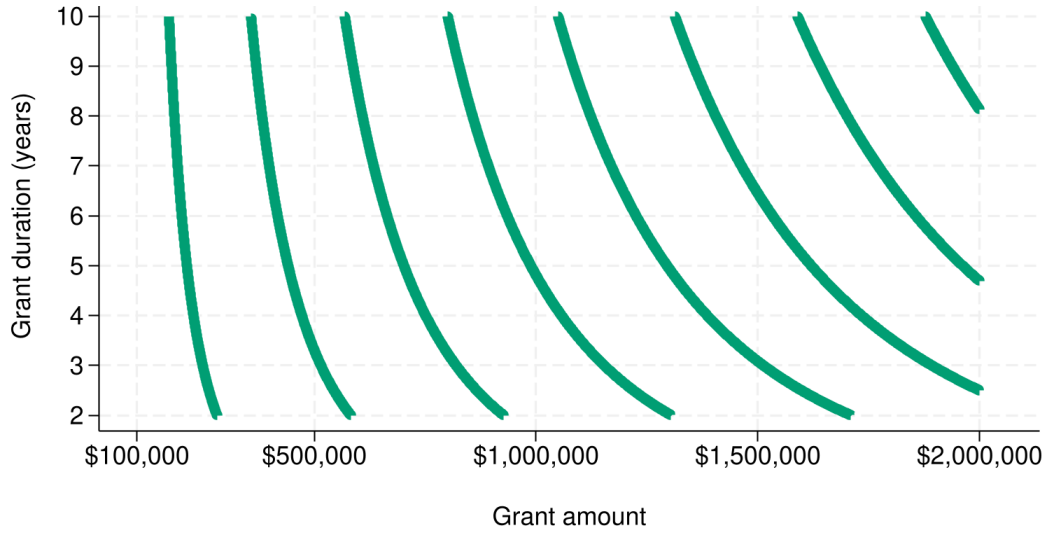
Figures & Tables

Figure 1: Distribution of observed grant designs



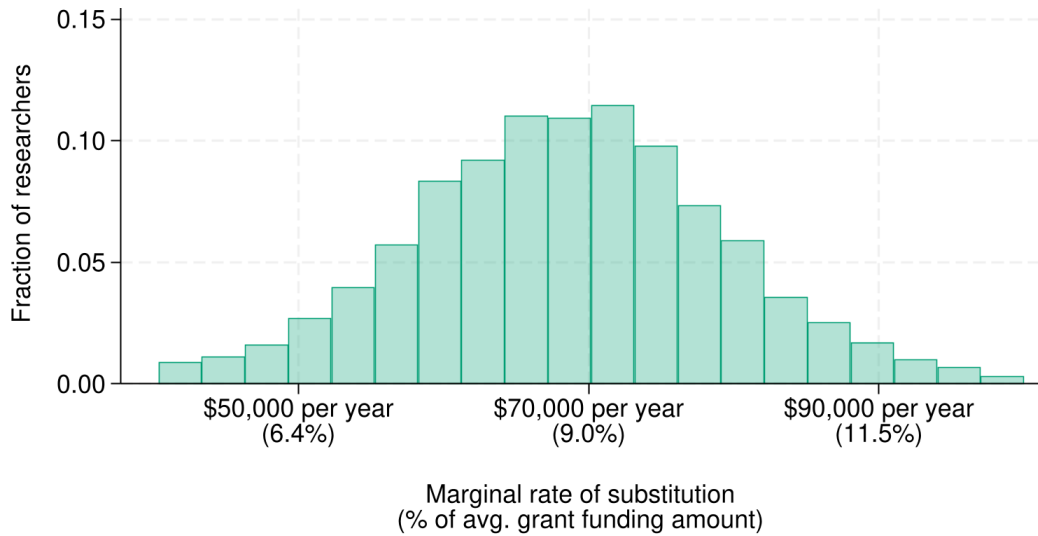
Note: Displays the share of grant designs as a two-dimensional histogram, where the intensity of the color reflects the percentage of all grants in the cell and cells span 1 year and \$250,000 in amount. Based on 222,041 grants at the focal institutions from 2000–2012. Data sourced from the Dimensions database ([Digital Science 2018](#)).

Figure 2: Grant design indifference curves



Note: Plots indifference curves over the support of the variation in the duration and size of the hypothetical research grants considered by respondents; based on the preferred γ estimate (Table 3, Col. 1).

Figure 3: Heterogeneous preference estimates



Note: Plots the distribution of researcher-specific marginal rates of substitution based on the predictions for $\gamma_i = Constant + \sum_X \beta_X X$ using the coefficient estimates reported in Table 3 Col. 7.

Table 1: Summary statistics — Survey sample

	count	mean	sd
<i>Broad field {0,1}</i>			
Engineering, math, & related	4,175	0.17	0.37
Humanities & related	4,175	0.19	0.39
Medical & health sciences	4,175	0.28	0.45
Natural sciences	4,175	0.16	0.36
Social sciences	4,175	0.21	0.41
<i>Research</i>			
Guaranteed funding	4,175	430,699.40	1,085,486.21
Fundraising expectations	4,175	549,238.32	1,055,926.36
<i>Professional</i>			
Has tenure {0,1}	4,175	0.59	0.49
Total salary	4,175	156,820.36	93,741.46
Soft-money salary	4,175	25,336.53	45,407.18
Total work hrs./week	4,175	49.88	13.06
Share of time spent on research [0,1]	4,175	0.39	0.20
Share of time spent on fundraising [0,1]	4,175	0.09	0.11
<i>Socio-demographic</i>			
Age	4,084	49.31	12.52
Female {0,1}	3,992	0.41	0.49
Non-white race/ethnicity {0,1}	4,110	0.24	0.43
Num. dependents in household	4,104	0.98	1.13
U.S. immigrant {0,1}	4,088	0.27	0.44

Note: “Guaranteed” research funding includes funds from prior awards and any other guaranteed sources over the next 5 years; “Fundraising expectations” are researchers’ beliefs about how much funds they will obtain from non-guaranteed sources over the next 5 years. All variables are continuous and bounded below at zero unless otherwise specified. Responses to socio-demographic questions were not mandatory, hence the differences in the count of observations.

Table 2: Summary statistics – Grant indifference experiment

	count	mean	sd	min	max
<i>Randommized elements</i>					
Grant #1: Duration (years)	4,175	3.5	1.1	2	5
Grant #1: Amount (\$)	4,175	781,904.2	690,313.1	100,000	2,000,000
Grant #2: Duration (years)	4,175	8.0	1.4	6	10
<i>Researcher’s response at indifference</i>					
Grant #2: Amount (\$)	4,175	607,129.1	584,626.5	10,000	2,000,000

Note: Summary statistics of the randomized elements and response for the grant preferences survey experiment.

Table 3: Researchers' willingness to trade off grant funding amount and duration

	$\gamma_i = Constant + \beta X_i$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
γ	0.767*** (0.0070)						
Constant		0.767*** (0.0039)	0.767*** (0.0039)	0.768*** (0.0039)	0.767*** (0.0039)	0.767*** (0.0039)	0.768*** (0.0070)
Age		0.0155*** (0.0039)					0.0150** (0.0072)
Funding intensity			0.0196*** (0.0041)				0.0166*** (0.0063)
Risk appetite				0.0181*** (0.0038)			0.0150** (0.0073)
Soft money salary share					0.0124*** (0.0039)		0.00581 (0.0072)
Fundraising productivity						0.0127*** (0.0037)	0.00798 (0.0058)
Marginal rate of substitution	\$68,000/year						
<i>N</i> obs.	4,175	4,175	4,175	4,175	4,175	4,175	4,175

Note: Reports results from estimating Eq. 3 to identify a single γ parameter (Col. 1) or allowing γ to depend on a constant term and some standardized observables (Cols. 2–7); robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Summary statistics – strategy survey experiment

	count	mean	sd	min	max
<i>Randomized elements</i>					
Grant duration (years)	4,175	6.06	2.56	2	10
Grant amount (\$)	4,175	778,239.52	684,075.23	100,000	2,000,000
<i>Strategic changes {0,1}</i>					
Increase speed	4,175	0.36	0.48	0	1
Pursue riskier projects	4,175	0.53	0.50	0	1
Pursue new directions	4,175	0.33	0.47	0	1
Increase size of ongoing projects	4,175	0.61	0.49	0	1
Increase accuracy	4,175	0.17	0.38	0	1

Note: Summary statistics of the two randomized elements of the research strategy survey experiment, and the respondent's answers to the question of "What are the two most important changes this grant would enable you to make in your research?".

Table 5: Effect of grant designs on researchers’ strategies

	Faster (1)	More risk (2)	New directions (3)	Larger ongoing projects (4)	More accurate (5)
log(Duration)	-0.056*** (0.015)	0.012 (0.016)	0.008 (0.015)	0.018 (0.015)	0.018 (0.011)
log(Amount)	-0.025*** (0.007)	0.017** (0.007)	-0.016** (0.007)	0.026*** (0.007)	-0.003 (0.006)
<u>Family-wise p-values</u>					
Duration	<0.01	0.83	0.85	0.61	0.49
Amount	<0.01	0.12	0.12	<0.01	0.85
dep. var. mean	0.36	0.53	0.33	0.61	0.17
N obs.	4,175	4,175	4,175	4,175	4,175

Note: Shows results from OLS regressions where the dependent variables are indicators that equal one if the strategy listed at the top of the column was chosen as one of the “two most important changes” that researchers would make in response to receiving a grant of a given (randomized) size and duration. Robust standard errors shown in parentheses; stars indicate significance levels using the unadjusted p -values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The family-wise p -values adjust for the ten hypotheses tested and are based on 10,000 bootstraps of the free step-down procedure of [Westfall and Young \(1993\)](#).

Table 6: Summary statistics — input allocations

	count	mean	sd
<u>Randomized elements</u>			
Grant duration (years)	4,175	6.06	2.56
Grant amount (\$)	4,175	778,239.52	684,075.23
<u>Input allocation share $[0,1]$</u>			
Own salary	4,175	0.18	0.13
Own training	4,175	0.08	0.10
Travel	4,175	0.18	0.10
Equipment or supplies	4,175	0.21	0.13
Others’ salaries	4,175	0.34	0.14

Note: Summary statistics of the two randomized elements of the research strategy survey experiment (duplicating the first two rows of [Table 4](#)) and respondents’ allocation of funding amounts across input categories. Input allocation responses are standardized such that input shares sum to one.

Table 7: Effect of grant designs on researchers' input allocations

	Own salary (1)	Own training (2)	Travel (3)	Equipment or supplies (4)	Others' salaries (5)
log(Duration)	-0.007* (0.004)	0.000 (0.003)	0.002 (0.003)	-0.001 (0.004)	0.005 (0.004)
log(Amount)	0.008*** (0.002)	0.008*** (0.001)	-0.001 (0.002)	-0.003 (0.002)	-0.012*** (0.002)
<u>Family-wise p-values</u>					
Duration	0.37	0.97	0.93	0.97	0.68
Amount	<0.01	<0.01	0.97	0.51	<0.01
dep. var. mean	0.18	0.08	0.18	0.21	0.34
N obs.	4,175	4,175	4,175	4,175	4,175

Note: Shows results from OLS regressions where the dependent variables are the share of the (randomized) grant that would be allocated to each input category. Robust standard errors shown in parentheses; stars indicate significance levels using the unadjusted p -values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The family-wise p -values adjust for the ten hypotheses tested and are based on 10,000 bootstraps of the free step-down procedure of [Westfall and Young \(1993\)](#).

A Additional summary statistics

A.1 Full summary statistics for covariates

Tables A1-A2 report the mean and standard deviation for all variables ever used in the analyses. If the count of observations is less than the full sample (4,175), it is because respondents were not required to answer the question and some chose not to — this only is relevant for the socio-demographic questions. Figure A1 plots the distribution of expected research funding across the five broad fields to illustrate the pervasiveness of demand for research grants.

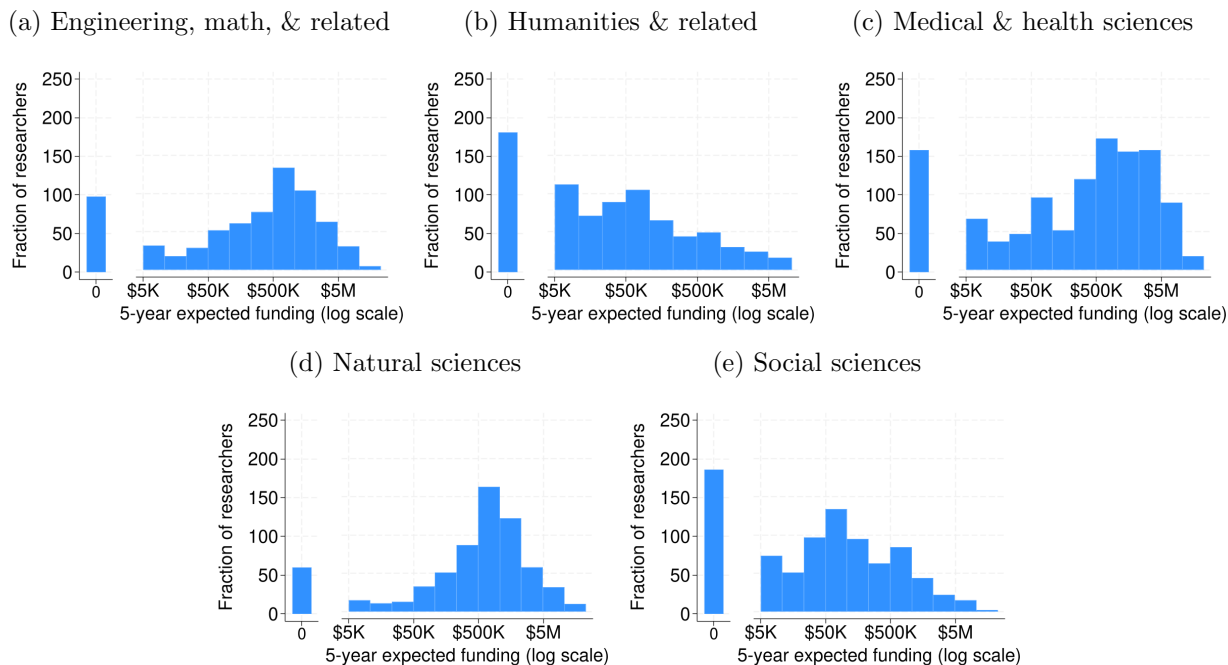
Table A1: Full summary statistics for covariates

	count	mean	sd
Professional			
<i>Rank {0,1}</i>			
Adjunct, clinical, teaching, other	4,175	0.06	0.23
Assistant	4,175	0.25	0.44
Associate	4,175	0.25	0.43
Emeritus or retired	4,175	0.03	0.17
Full	4,175	0.41	0.49
<i>Tenure {0,1}</i>			
Not on tenure track, not applicable	4,175	0.20	0.40
On tenure track, pre-tenure	4,175	0.22	0.41
Tenured	4,175	0.59	0.49
<i>Salary</i>			
Base	4,175	120,649.10	79,692.66
Grant-sponsored or	4,175	25,336.53	45,407.18
Supplementary	4,175	5,142.51	16,566.07
Clinical	4,175	5,692.22	37,375.73
Outside activities	4,175	7,798.80	29,703.94
Guaranteed research funding	4,175	430,699.40	1,085,486.21
Research fundraising expectations	4,175	549,238.32	1,055,926.36
Total work hrs./week	4,175	49.88	13.06
<i>Share of work time [0,1]</i>			
Research	4,175	0.39	0.20
Fundraising	4,175	0.09	0.11
Teaching	4,175	0.27	0.17
Administration	4,175	0.15	0.14
Clinical	4,175	0.04	0.14
Other	4,175	0.06	0.09
<i>Aggregate field [0,1]</i>			
Humanities	4,175	0.17	0.37
Medical & health	4,175	0.19	0.39
Natural	4,175	0.28	0.45
Social & mathematical	4,175	0.16	0.36
Social sciences	4,175	0.21	0.41

Table A2: Full summary statistics for covariates, continued

	count	mean	sd
Socio-demographic			
Age	4,084	49.31	12.52
Female {0,1}	3,992	0.41	0.49
Married or in partnership {0,1}	4,044	0.84	0.36
U.S. immigrant {0,1}	4,088	0.27	0.44
3rd generation in U.S. or less {0,1}	2,848	0.44	0.50
Household annual income	4,045	260,819.53	215,128.41
Num. household dependents	4,104	0.98	1.13
<i>Race/ethnicity</i> {0,1}			
Asian	4,175	0.12	0.33
Black	4,175	0.03	0.17
Hispanic	4,175	0.06	0.23
White	4,175	0.78	0.42
Other	4,175	0.05	0.22
Risk-taking			
Research, own belief	4,175	4.63	2.35
Research, others' belief	4,175	4.56	2.40
Personal life	4,175	5.25	2.12
Research			
Objective: ask or answer questions [0,10]	4,175	4.85	2.57
<i>Output</i> {0,1,2}			
Journal articles	4,175	1.87	0.37
Books	4,175	0.52	0.69
Materials, methods, or tools	4,175	0.70	0.68
Products	4,175	0.47	0.64
<i>Audience</i> {0,1,2}			
Academics	4,175	1.88	0.36
Policymakers	4,175	0.84	0.69
Businesses and orgs.	4,175	0.54	0.62
General public	4,175	0.83	0.62
Other			
Inverse Mills Ratio	4,175	2.40	0.10

Figure A1: Expected research funding by broad field



Note: Shows the within-field distributions of researchers’ total expected research funding over the next five years, which is the sum of existing funding and expected fundraising amounts over the same period.

A.2 Population and sample comparisons

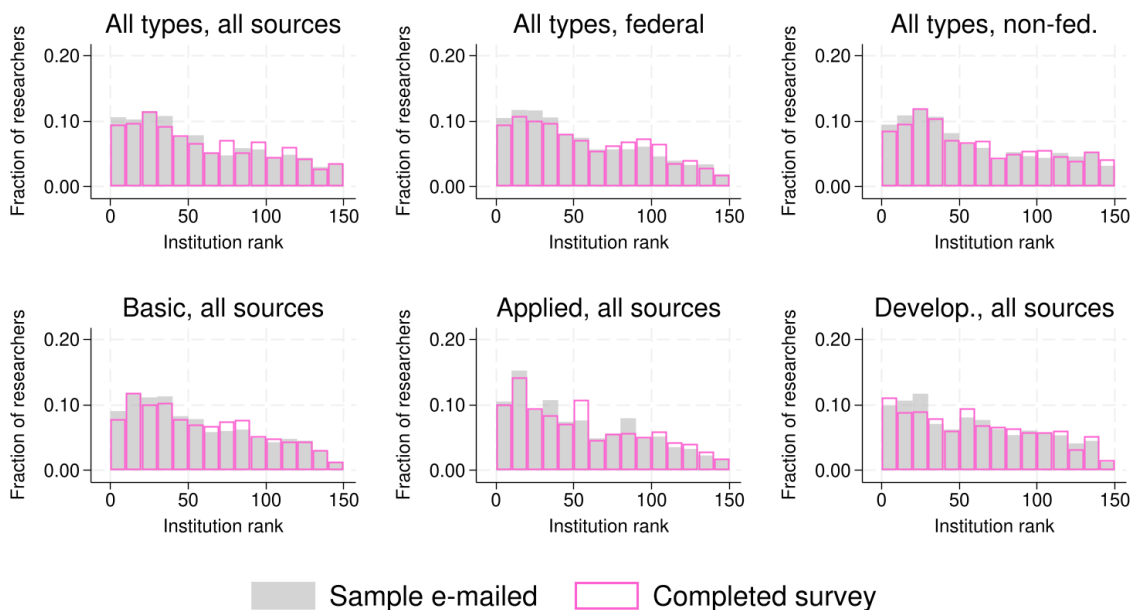
Here we replicate the two tests of representativeness for the survey as also done in Myers et al. (2023). First, A2 shows this comparison based on data from the National Science Foundation’s HERD survey (US National Center for Science and Engineering Statistics 2021), which reports institutional-level data on the total amount of funding flows into all of the institutions in our population (recall, our population was constructed using the HERD, which is why this data is available for the full population).

Figure A2 reports the distributions and regression tests for mean differences in six metrics of institution-level research funding that compare respondents to the full set of researchers invited to participate (“Sample e-mailed”). The distributions overlap to a large degree (see Panel a). We do estimate statistically significant differences in means (see Panel b), but the magnitudes of these differences are all in the range of approximately 4–6%.

Figure A3 reports the results of a similar exercise, instead using individual-level data on researchers’ publication output and grant receipts. This data was obtained by performing a fuzzy match of our population (i.e., using names and institution data) to the Dimensions database (Digital Science 2018), which includes disambiguated researcher-level records. We

Figure A2: Recruitment versus completion sample comparison per HERD metrics

(a) Distributions per HERD R&D measures



(b) Regression estimates of mean differences

	All sources, by type			All types, by source		
	All (1)	Federal (2)	Non-fed. (3)	Basic (4)	Applied (5)	Develop. (6)
Completed survey	-35.01*** (6.953)	-21.30*** (4.511)	-13.71*** (3.062)	-24.31*** (4.867)	-7.170*** (2.492)	-3.812*** (1.328)
Constant	649.1*** (1.308)	351.7*** (0.863)	297.4*** (0.578)	413.2*** (0.917)	176.7*** (0.459)	60.46*** (0.254)
% diff.	-5.4%	-6.1%	-4.6%	-5.9%	-4.1%	-6.3%
<i>N</i> obs.	130,785	130,785	130,785	130,785	130,735	128,169

Note: Panel (a) compares the distributions of e-mailed professors and respondents per the rank of their institution along each dimension of R&D funding. Panel (b) reports estimates from a regression of each sampled professor’s institutional R&D funding (in 2019 \$-M) on a dummy for whether the sampled individual completed the survey; the “% diff.” row reports the mean difference in the measure as a percentage of the non-respondent average (i.e., it is the ratio of the two coefficients); robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All institutional data is from the 2019 NSF HERD ([US National Center for Science and Engineering Statistics 2021](#)).

focus on researchers’ publications and grants during the twenty years prior (2003-2022) and see very little differences between our respondents and the full set of individuals invited to the survey. The distributions have strong overlap over the full support (see Panel a), and the mean differences are all insignificant and/or smaller than 7.5%.

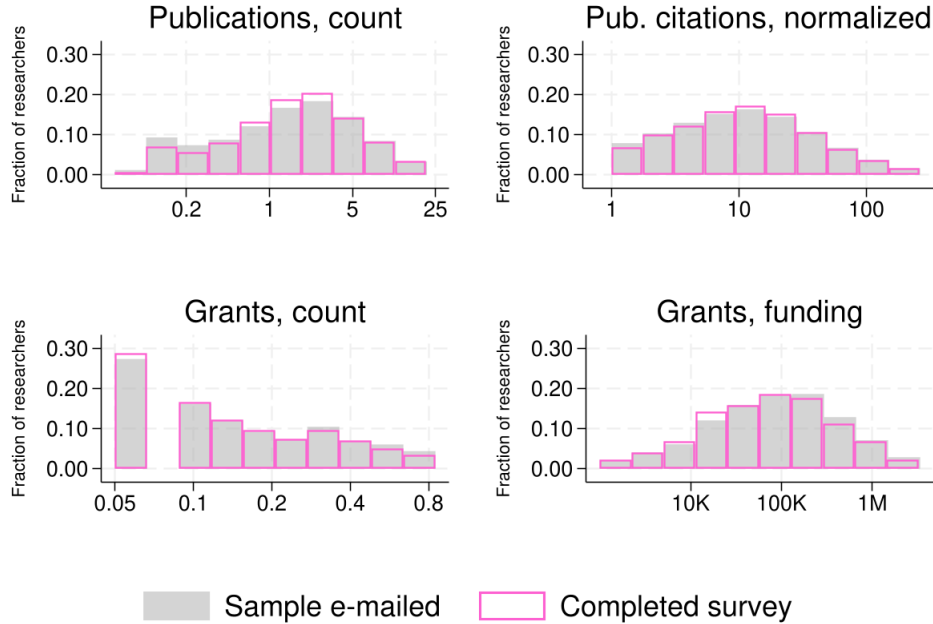
A.3 Randomized incentives

In order to implement the sample selection correction methods of Heckman (1979), we must have a source of variation in survey participation that is uncorrelated with any of the focal parameters. With such variation in hand, a control function can be constructed and included in the focal regressions to condition on any variation in respondents’ answers that otherwise would have been unobservable, endogenous, and led to biased estimates. Furthermore, the statistical significance of this control function can shed light on the degree to which selection into the survey on (relevant endogenous) unobservables may have occurred.

To generate the necessary variation in participation, we implemented randomized incentives and reminders in our e-mail strategy. Each e-mail was randomly assigned to (1) one of four incentive arms comprising a gift card lottery or a donation on the respondents’ behalf to a charity of their choice, and (2) one of three reminder schedules including zero, one, or two reminders. Figure A4a shows the raw data and statistical tests of the effects of these arms (aggregating the incentive arms in Panel a, and assuming linear separability of the treatments in the regressions shown in Panel b). There are clear effects, with the “most treated” invites (i.e., both incentives and two reminders) increasing the probability of survey participation by roughly 2.7 percentage points to roughly 4.5%, yielding an average response rate roughly more than twice as large as the “least treated” invites (i.e., no reminders and no incentives) whose response rate was 1.9%.

Figure A3: Recruitment versus completion sample comparison per Dimensions metrics

(a) Distributions per Dimensions publications and grants

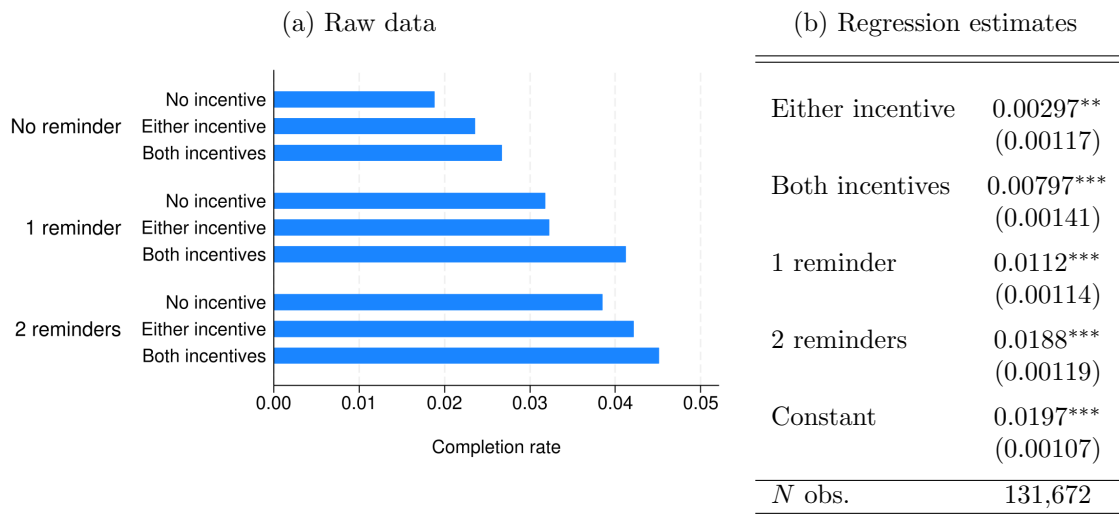


(b) Regression estimates of mean differences

	Pub., count (1)	Pub. cites, normalized (2)	Grant, count (3)	Grant, total \$ (4)
Completed survey	-0.111 (0.0688)	-0.575 (0.807)	0.00878** (0.00343)	-5568.8 (8519.4)
Constant	2.906*** (0.0158)	20.59*** (0.185)	0.117*** (0.000715)	121272.4*** (2093.0)
% diff.	-3.8%	-2.8%	7.5%	-4.6%
<i>N</i> obs.	87,000	87,000	87,000	87,000

Note: Panel (a) compares the distributions of e-mailed professors and respondents per each dimension of individual-level publication output and grant funding per year (2003–2022). Panel (b) reports estimates from a regression of each sampled professor’s publication or grant metric on a dummy for whether the sampled individual completed the survey; the “% diff.” row reports the mean difference in the measure as a percentage of the non-respondent average (i.e., it is the ratio of the two coefficients); robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All publication and grant data is from the Dimensions database ([Digital Science 2018](#)).

Figure A4: Completion rate by incentive and reminder arms

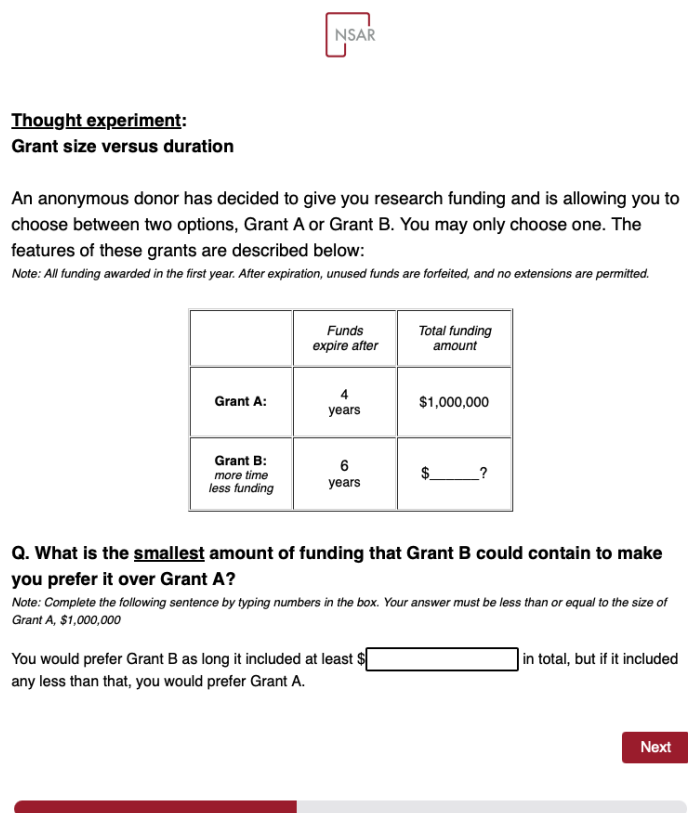


Note: Panel (a) shows the average completion rate by each combination of incentive and reminder arm; Panel (b) shows estimates from a linear probability model of completion as a function of incentives and reminders; robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Additional survey experiment details

Figure A5 shows the thought experiment shown to respondents when soliciting their preferences over grant designs. The framing and text were informed by pilot interviews with researchers across a range of disciplines and were designed to leave as little uncertainty about the grant design as possible. The respondent is asked to report the smallest amount of funding in “Grant B”, which is always set to a (randomly) longer duration than that of the (randomized) duration in “Grant A”, that would make them prefer it over “Grant A”. We do not solicit the exact indifference point because pilot testing indicated that the concept of indifference was costly for many researchers to engage with. However, they were much more comfortable engaging with the question as worded here. Thus, we must assume that the value of “Grant B” that achieves indifference for the respondent is infinitesimally smaller (e.g., \$1) than what they report to the question in the survey.

Figure A5: Respondent view of selection experiment



NSAR

Thought experiment:
Grant size versus duration

An anonymous donor has decided to give you research funding and is allowing you to choose between two options, Grant A or Grant B. You may only choose one. The features of these grants are described below:

Note: All funding awarded in the first year. After expiration, unused funds are forfeited, and no extensions are permitted.

	<i>Funds expire after</i>	<i>Total funding amount</i>
Grant A:	4 years	\$1,000,000
Grant B: <i>more time less funding</i>	6 years	\$_____?

Q. What is the smallest amount of funding that Grant B could contain to make you prefer it over Grant A?

Note: Complete the following sentence by typing numbers in the box. Your answer must be less than or equal to the size of Grant A, \$1,000,000

You would prefer Grant B as long it included at least \$ in total, but if it included any less than that, you would prefer Grant A.

Next

Note: Shows a respondent’s view of an example thought experiment soliciting the respondent’s approximate indifference point between two grants.

Figure A6 shows the thought experiment shown to respondents when soliciting their strategic responses to a hypothetical grant. First, they are shown a screen that only describes the

details of the thought experiment (left side of Figure A6). The red, bold text highlights the randomized components of the amount and duration of the grant. As in the previous thought experiment, we communicate the fact that all of the funding is awarded immediately, any unused funds cannot be retained after the duration of the grant, and there are no “renewals” or “extensions” of the grant possible.

After viewing the setup of this thought experiment, the respondents are shown the right side of Figure A6, which solicits respondents’ plans for how they’d use the grant regarding their top two strategic changes as well as their qualitative allocation of the grant across the five input categories. The strategic choice options are designed such that the same strategy cannot be chosen from both drop-down menus. The phrasing of the strategic options and the input categories was also informed by pilot interviews. Only a 3-point Likert scale is used in the input allocation exercise because pilot tests indicated a significant time cost and uncertainty on behalf of the respondent if we attempted to solicit anything more detailed.

Figure A6: Respondent view of treatment experiment

The screenshot displays a survey interface with two main sections. The left section, titled 'Thought experiment: How would you use a hypothetical grant?', provides context: 'Consider the following: An anonymous donor has decided to give you an unrestricted research grant of \$2,000,000, all of which would be available to you immediately in the first year, but after 7 years you would lose any unused funds and no grant renewals or extensions could occur. On the next page, we'll ask how you would use this hypothetical grant.' The right section contains two questions. The first question asks for the two most important changes, with dropdown menus for 'Most important change' (selected: 'Pursue projects less related to your current work') and 'Second most important change' (selected: 'Pursue riskier projects'). The second question asks for funding allocation across five categories using a 3-point Likert scale (None, Some, A lot).

	None	Some	A lot
Salary or wages for other researchers, students, or staff (i.e., new hires or current employees)	<input type="radio"/>	<input type="radio"/>	<input checked="" type="radio"/>
Your own salary (e.g., teaching or clinical buyouts)	<input type="radio"/>	<input checked="" type="radio"/>	<input type="radio"/>
Travel	<input type="radio"/>	<input checked="" type="radio"/>	<input type="radio"/>
Your own training or education	<input checked="" type="radio"/>	<input type="radio"/>	<input type="radio"/>
Equipment, instruments, data, or any materials	<input type="radio"/>	<input checked="" type="radio"/>	<input type="radio"/>

Note: Shows a respondent’s view of an example thought experiment about their use of a hypothetical research grant, including the preface (left side) and the response fields (right side).

B Additional results on researchers' preferences

B.1 A model of researchers' production and consumption

The main empirical model of researchers' preferences over grant amount and duration (Eq. 1: $\alpha A^\gamma D^{1-\gamma}$) is, effectively, a reduced form representation of a more complex model of researchers' production and consumption patterns. The following subsections motivate tests of heterogeneity across researchers in terms of their preferences (i.e., γ).

First, we specify a more expansive model of researchers' utility from the receipt of a grant that has some amount A and duration D :

$$\begin{aligned}
 V(\vec{a}_i; \theta_i, A, D) = \max_{\vec{a}_i} & \sum_t^T \delta_i^t u_i(f_i(\beta_i + a_{it}), a_{it}) \\
 \text{s.t.} & \\
 & \sum_t a_{it} \leq A \\
 & a_{it} = 0 \text{ if } t > D,
 \end{aligned}
 \tag{B1}$$

where the key choice for the researcher is how to allocate the grant over periods $t = (1, 2, \dots, T)$. The notation of the model is as follows: $\vec{a} = (a_1, a_2, \dots, a_T)$ represents the vector of choices about how to allocate the funding over time; θ is the set of parameters that govern the researcher's discount rate δ , their utility function u , their production function f , and their guaranteed funding β , which is available every year; u and f are assumed to be concave; and all parameters and variables are implicitly unique to each researcher. The two constraints dictate how much grant funding is at the researcher's disposal and how long they have access to those funds. In the context of this model, the focal empirical parameter of γ represents the marginal utility of an increase in grant amount ($\partial V/\partial A$) relative to the marginal utility of an increase in grant duration ($\partial V/\partial D$) after the researcher has re-optimized their allocations over time.

Researchers value grants with more funding (larger A) because they enable more production within a given year, and they value grants with a longer duration (larger D) because they enable more years of production on the steeper portion of the production function.⁴¹

⁴¹Anecdotal conversations with researchers indicate that they also value longer grants because of the reduced variance in their future funding streams, which is not formalized in our motivating model. Practically, this enables researchers to better plan their work over longer horizons. For example, a common remark by researchers when considering a longer grant is that it would allow them to offer their staff and students longer (formal or informal) contracts. Another (un-modeled) potential benefit of longer grants is that it provides

In this model, researchers derive positive utility not just from the outputs of their work, but also from their inputs directly. This reflects two realities; first, grant funds can often be used for researchers’ salaries; second, as sociological studies and popular critiques of science have long pointed out, researchers are not immune to “empire-building” tendencies, where funding levels can play a prominent role in delineating social one’s status (Merton 1968; Shapin 1995; Madsen and Aagaard 2020).⁴²

The model abstracts away from any of the researchers’ choices about their effort or their research strategies, with the production function f summarizing each researchers’ ability to convert funding into output they value.

Overall, the value derived from outputs and inputs depends on the incentives and institutions that transform these things into objects that researchers value (e.g., job security, salary, prestige) and their preferences for those objects. All of these forces and preferences are summarized by u . Practical examples of factors shaping u include researchers taste for science (Stern 2004; Roach and Sauermann 2010), the tenure process and output measurement schemes (MacLeod and Urquiola 2020), intellectual property regimes (Hvide and Jones 2018), the nature of competition within fields (Hill and Stein 2019), gate-keeping (Azoulay et al. 2019a), and social factors more generally (Shapin 1995).

In the Appendix Section B.5 below, we used highly simplified versions of this model to make some predictions about heterogeneity.

B.2 Alternative CES functional forms

Our preferred specification of a Cobb-Douglas utility function is a special case of the Constant Elasticity of Substitution (CES) function with an assumed unit elasticity. In our case, the more general CES function can be written as:

$$v(A_i, D_i) = \alpha_i (\gamma A_i^\rho + (1 - \gamma) D_i^\rho)^{1/\rho}, \tag{B2}$$

insurance for future supply or demand shocks — e.g., if a researcher believes there is a new technology on the horizon that would increase their own productivity, they could reserve funding for future periods.

⁴²Since most grants are obtained through competitive evaluations, they also serve as signals about researchers’ abilities. There is logic to this given the unobservability of researchers’ effort and the fact that most grants are awarded via peer review processes — they are signals that peers value a researchers’ ideas. Thus, they can have more direct and indirect value to each researcher depending on their position. Furthermore, grants often include overhead funding that flows to the researchers’ institution, with these “indirect costs” accounting for a significant fraction of a grants total value (Johnston et al. 2015). However, our thought experiment abstracts away from both of these points since they are non-competitive and devoid of indirect costs.

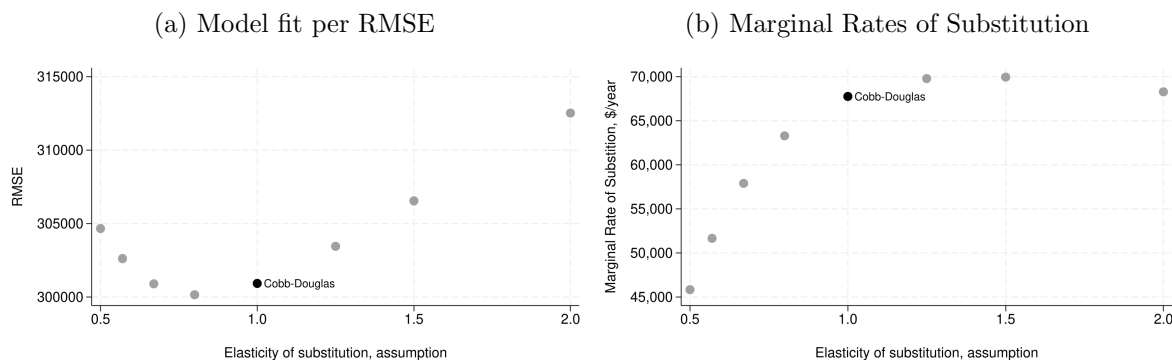
where the elasticity of substitution is $\sigma = 1/(1 - \rho)$. With a CES function, our focal estimating equation based on the thought experiment data is generalized from Equation 3 to:

$$\tilde{A}_{i2} = \left(A_{i1}^\rho + ((1 - \gamma)/\gamma)(D_{i1}^\rho - D_{i2}^\rho) \right)^{1/\rho} + \epsilon_i, \quad (\text{B3})$$

where ρ is determined by the elasticity of substitution (σ) that we assume. This still leaves the single focal parameter, γ to be estimated. To explore the sensitivity of our results to our assumption about σ , we estimate a series of models with an assumed elasticity of substitution spanning from 0.5 to 2.0.

First, we explore which assumed value of σ fits the data best by plotting the model’s RMSE across our range of assumptions in Figure B1 Panel (a). Our preferred assumption of the Cobb-Douglas unit elasticity appears quite close to the (local) minimum in model errors that we explore. This gives us some confidence that unit elasticity is a reasonable assumption. Figure B1 Panel (b) plots the marginal rates of substitution (in funding dollars per year). Unsurprisingly, larger assumed elasticities yield larger marginal rates of substitution. But across the full range of assumptions, we consistently estimate magnitudes between \$45,000 and \$70,000 dollars per year. Overall, we find that researchers are only willing to sacrifice 5–8% of their grant funding to extend their access to that funding.

Figure B1: Preference estimates under alternative elasticity of substitution assumptions



Note: Panel (a) plots the RMSE when estimating the γ parameter under alternative assumptions for the elasticity of substitution in the CES utility function. Panel (b) plots the marginal rate of substitution implied by the γ estimate at the sample means under the same range of assumptions. In both panels, the Cobb-Douglas case (unit elasticity) is highlighted, as it is the basis of all other results on preferences.

B.3 Main results on preferences, reporting sample selection correction estimates

Table B1 reports results from estimating Eq. 3 with the Inverse Mills Ratio (IMR) included as a control variable to account for the possibility of sample selection. The IMR variable is

constructed following Heckman (1979) using the randomized survey participation incentives and reminders as instrumental variables.

Table B1: Researchers’ willingness to trade off grant funding amount and duration, reporting sample selection coefficient

	(1)	(2)	(3)	(4)
γ	0.767*** (0.00698)	0.779*** (0.00676)	0.817*** (0.00591)	0.848*** (0.00533)
IMR	-9645.0* (5286.4)	-6260.8 (4472.4)	-5905.1* (3391.8)	-5089.1* (3035.3)
Marginal rate of substitution	67,708 [8.7%]	63,429 [8.1%]	50,091 [6.4%]	40,004 [5.1%]
Excl. above ptile.		99 th	95 th	90 th
N obs.	4,175	4,134	3,978	3,777

Note: Reports results from estimating Eq. 3 using alternative samples; larger γ values reflect a stronger taste for funding amount compared to duration; marginal rate of substitution (\$ per year) is reported at the means and is shown in brackets as a percentage of average funding amount; the “Excl. above ptile.” samples drop responses where $(A_{i1} - \tilde{A}_{i2})/A_{i1}$ is at or above the reported percentile; robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The IMR row reports the coefficients on the standardized Inverse Mills Ratios included in the regressions.

B.4 Alternative estimation of grant preferences

The empirical model of grant preferences in the main paper assumes constant returns to scale (CRS; $\gamma + (1 - \gamma) = 1$). We can relax this assumption by making use of a third thought experiment in the data. Although this third experiment was designed for another research question, it provides the variation necessary to estimate a more flexible model that has a separate utility parameter for both grant size and duration.

In this other experiment, we asked respondents to report their willingness to trade off their annual salary in exchange for increases in their guaranteed research budget, effectively, a grant. This was done twice, by asking respondents how low of a salary they would be willing to take in exchange for having access to an additional \$250,000 or \$1,000,000 for 5 years.

To begin, first consider a new indirect utility function that depends on researchers’ annual salary S , their current guaranteed research budget over the coming 5 years B , and a new

grant of amount A and duration D :

$$v(S_{ig}, B_i, A_{ig}, D_{ig}) = \alpha S_{ig}^\sigma (B_i + A_{ig})^\rho D_{ig}^\delta . \quad (\text{B4})$$

In order to use the variation from the thought experiments, we assume we can decompose Equation B4 into two separate functions that describe researchers' preferences over the new grant (evaluating just A and D) and their preferences over an increase in their 5-year guaranteed budget and a corresponding change in their salary:

$$\begin{aligned} v_1(A_{ig}, D_{ig}) &= \alpha_{i1} A_{ig}^\rho D_{ig}^\delta \\ v_2(S_{ig}, B_i, A_{ig}) &= \alpha_{i2} S_{ig}^\sigma (B_i + A_{ig})^\rho . \end{aligned} \quad (\text{B5})$$

To summarize, we have researchers' responses to three different scenarios: (1) being indifferent between two grants that vary in their size and duration, call these grants $g = \{1, 2\}$; (2) being indifferent between some decrease in their salary and an increase in their research budget by \$250,000, call this increase grant $g = 3$; and (3) being indifferent between some decrease in their salary and an increase in their research budget by \$1,000,000, call this increase grant $g = 4$. Thus, we have three equations:

$$\begin{aligned} v_1(A_{i1}, D_{i1}) &= v_1(A_{i2}, D_{i2}) \\ v_2(S_{i0}, B_i) &= v_2(S_{i3}, B_i, A_{i3}) \\ v_2(S_{i0}, B_i) &= v_2(S_{i4}, B_i, A_{i4}) , \end{aligned} \quad (\text{B6})$$

where S_{i0} indicates their current salary and B_i is their current 5-year research budget guarantee. The thought experiments involve providing $(A_{i1}, D_{i1}, D_{i2}, A_{i3}, A_{i4})$ to the respondents, who in turn report the values (A_{i2}, S_{i3}, S_{i4}) such that indifference holds. We use these three equations to generate the three moment conditions necessary to identify the key parameters σ , ρ , and δ .

Table B2 reports the results from GMM estimation of the key parameters. The ratio of these alternative estimates of the elasticities of grant size (σ) and duration (δ), $\frac{0.75}{0.22}=3.41$, are very close to our estimate of this same ratio under the CRS assumption we use in the main results, $\frac{0.77}{(1-0.77)}=3.26$. We take this as evidence that the CRS assumption is not distorting our results on the relative preferences researchers have for grant designs.

Table B2: Non-CRS based grant preference estimates

	(1)
σ (salary)	1.016*** (0.001)
ρ (grant amount)	0.075*** (0.004)
δ (grant duration)	0.022*** (0.001)
N obs.	4,003

Note: GMM estimates of parameters in Eq. B5 based on the three moments generated by the indifference points shown in Eq. B6; bootstrapped standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.5 Predicting heterogeneity in grant design preferences

Deriving comparative statics from Equation B1 without making further assumptions is particularly challenging. But, our goal is only to motivate tests of heterogeneity in researchers' relative preferences for grant funding amount versus duration (γ). So below, we take a much simpler approach of eliminating all but one of the four sources of heterogeneity in the model (δ, u, f, β) and then testing whether, in each stylized model, researchers' relative preferences for amount versus design is increasing or decreasing in a given parameter.⁴³

Throughout, we define $V_{A,D} \equiv V(a; \theta, A, D)$, $a^*(V_{A,D}) \equiv \arg \max_a V_{A,D}(a)$, and $\tilde{\gamma} \equiv \frac{\frac{\partial V_{A,D}}{\partial A}}{\frac{\partial V_{A,D}}{\partial D}}$, noting that $\tilde{\gamma}$ is proportional to γ as defined in the main empirical model shown in Equation 1.

In all cases below, we focus only on the scenarios where $A = \{1, 2\}$ and $D = \{1, 2\}$ and evaluate preferences based on changes from initial values of $A = 1$ and $D = 1$. This simplifies the construction of the partial derivatives that define researchers' preferences. The relative value of an increase amount is: $\frac{\partial V_{1,1}}{\partial A} = V_{2,1} - V_{1,1}$, and the relative value of an increase in duration is: $\frac{\partial V_{1,1}}{\partial D} = V_{1,2} - V_{1,1}$.

Discount rate

To study the effect of researchers' discount rates on their grant preferences, we assume that the production and consumption functions, where grant funding a is the input, can

⁴³Each stylized model retains concavity in the production (or consumption) function since linear formulations eliminate any concerns for the temporal dimension and in turn eliminate the value of grant duration by construction.

be jointly given by $a_t^{1/2}$. In this case, there is no heterogeneity in researchers' production or consumption functions, nor in their guaranteed funding flows ($\beta = 0$). Focusing first on changes in grant amount for a $D = 1$ grant, researchers' utility, optimal input allocations, and marginal valuations are given by:

$$\begin{aligned} V_{1,1} &= \max_a a^{1/2} \\ a^*(V_{1,1}) &= a^*(V_{2,1}) = 1 \\ \frac{\partial V_{1,1}}{\partial A} &= 2^{1/2} - 1. \end{aligned}$$

There are no costs to spending the grant funding, so all funds are used (as will be the case in all 1-year grant scenarios). Next, focusing on changes in grant duration for a $A = 1$ grant, researchers' utility, optimal input allocations, and marginal valuations are given by:

$$\begin{aligned} V_{1,2} &= \max_a a^{1/2} + \delta(1 - a)^{1/2} \\ a^*(V_{1,2}) &= \frac{1}{\delta^2 + 1} \\ \frac{\partial V_{1,1}}{\partial D} &= \left(\frac{1}{\delta^2 + 1}\right)^{1/2} + \delta\left(1 - \left(\frac{1}{\delta^2 + 1}\right)\right)^{1/2} - 1. \end{aligned}$$

Thus, the ratio of researchers' marginal value from increases in grant amount versus duration is given by:

$$\tilde{\gamma}(\delta) = \frac{(2^{1/2} - 1^{1/2})}{\left(\left(\frac{1}{\delta^2 + 1}\right)^{1/2} + \delta\left(1 - \left(\frac{1}{\delta^2 + 1}\right)\right)^{1/2} - 1^{1/2}\right)},$$

where $\partial\tilde{\gamma}/\partial\delta < 0$. Thus, we should expect researchers that discount future periods at a larger rate (smaller δ) to have a stronger preference for grant amount (larger γ).

Capital-intensity, risk-aversion, and direct utility

Here, we focus on the role of capital-intensity (i.e., convexity of f w.r.t. a), risk-aversion (i.e., convexity of u w.r.t. $f(a)$), and direct utility of grant funding (i.e., convexity of u w.r.t. a). We assume that the production and consumption functions, where grant funding a is the input, can be jointly given by a_t^κ . All three of these features are captured by κ : a larger value indicates a more convex production function w.r.t. funding, larger risk tolerance, and larger direct utility of grant funding. In this case, there is no discounting of the second period ($\delta = 0$) and no heterogeneity in researchers' guaranteed funding flow ($\beta = 0$). Focusing first on changes in grant amount for a $D = 1$ grant, researchers' utility, optimal input allocations,

and marginal valuations are given by:

$$\begin{aligned} V_{1,1} &= \max_a a^\kappa \\ a^*(V_{1,1}) &= a^*(V_{2,1}) = 1 \\ \frac{\partial V_{1,1}}{\partial A} &= 2^\kappa - 1 \end{aligned}$$

There are no costs to spending the grant funding, so all funds are used. Next, focusing on changes in grant duration for a $A = 1$ grant, researchers' utility, optimal input allocations, and marginal valuations are given by:

$$\begin{aligned} V_{1,2} &= \max_a a^\kappa + (1 - a)^\kappa \\ a^*(V_{1,2}) &= \frac{1}{2} \\ \frac{\partial V_{1,1}}{\partial D} &= \left(\frac{1}{2}\right)^\kappa + \left(\frac{1}{2}\right)^\kappa - 1. \end{aligned}$$

Thus, the ratio of researchers' marginal value from increases in grant amount versus duration is given by:

$$\tilde{\gamma}(\kappa) = \frac{2^\kappa - 1}{\left(\frac{1}{2}\right)^\kappa + \left(\frac{1}{2}\right)^\kappa - 1},$$

where $\partial\tilde{\gamma}/\partial\kappa > 0$. Thus, we should expect researchers that are more capital-intensive, more risk-loving, and receive more direct utility from funding to have a stronger preference for grant amount (larger γ).

Fundraising productivity

Here, we focus on the role of researchers' ability to obtain research funding (beyond the focal grant), which we capture with β . We assume that the production and consumption functions, where grant funding a is the input, can be jointly given by $(\beta + a_t)^{1/2}$. In this case, there is no discounting of the second period ($\delta = 0$) and no heterogeneity in researchers' production or consumption functions. Focusing first on changes in grant amount for a $D = 1$ grant, researchers' utility, optimal input allocations, and marginal valuations are given by:

$$\begin{aligned} V_{1,1} &= \max_a (\beta + a)^{1/2} \\ a^*(V_{1,1}) &= a^*(V_{2,1}) = 1 \\ \frac{\partial V_{1,1}}{\partial A} &= (\beta + 2)^{1/2} - (\beta + 1)^{1/2} \end{aligned}$$

There are no costs to spending the grant funding, so all funds are used. Next, focusing on changes in grant duration for a $A = 1$ grant, researchers' utility, optimal input allocations, and marginal valuations are given by:

$$\begin{aligned}
 V_{1,2} &= \max_a (\beta + a)^{1/2} + (1 - (\beta + a))^{1/2} \\
 a^*(V_{1,2}) &= \frac{1}{2} - \beta \\
 \frac{\partial V_{1,1}}{\partial D} &= (\beta + (\frac{1}{2} - \beta))^{1/2} + (1 - (\beta + (\frac{1}{2} - \beta)))^{1/2} - (\beta + 1)^{1/2}.
 \end{aligned}$$

Thus, the ratio of researchers' marginal value from increases in grant amount versus duration is given by:

$$\tilde{\gamma}(\beta) = \frac{(\beta + 2)^{1/2} - (\beta + 1)^{1/2}}{(\beta + (\frac{1}{2} - \beta))^{1/2} + (1 - (\beta + (\frac{1}{2} - \beta)))^{1/2} - (\beta + 1)^{1/2}},$$

where $\partial \tilde{\gamma} / \partial \beta > 0$. Thus, we should expect researchers who are more productive at securing funding for their research every period to have a stronger preferences for grant amount (larger γ). Intuitively, researchers who are good at fundraising have a larger inter-temporal budget constraint, which moves them "further up" their production function in the future and increases the relative value of being further up their production function in the present.

B.6 Additional heterogeneity in grant design preferences

Table B3 reports group-specific estimates of γ based on a researchers' major field of study (Col. 1), tenure status (Col. 2), and self-reported gender (Col. 3). The bottom of the table reports p -values from tests of equality in the estimates within each model. We find statistically significant differences in γ estimates across major fields; however, the magnitude of the differences is smaller than what we observe when we estimate γ to be a function of theory-motivated features. We estimate smaller and statistically insignificant differences between researchers split based on their tenure status or gender.

Table B3: Heterogeneity in the amount-duration tradeoff, per other features

	(1)	(2)	(3)
Engineering and math	0.760*** (0.0089)		
Humanities	0.740*** (0.0090)		
Medicine	0.779*** (0.0073)		
Natural sciences	0.782*** (0.0097)		
Social sciences	0.769*** (0.0089)		
Not tenured		0.759*** (0.0060)	
Tenured		0.773*** (0.0051)	
Male			0.769*** (0.0050)
Female			0.764*** (0.0062)
<i>p</i> -val of diff.	0.01	0.07	0.46
<i>N</i> obs.	4,175	4,175	4,175

Note: Reports results from estimating Eq. 3 allowing γ to be group-specific; robust standard errors in parentheses (below the parameter estimates) and *p*-values from tests of the null hypothesis that group-specific estimates are equal (at the bottom of the table); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Funders’ revealed preferences over grant designs

C.1 Funders’ preferences

For simplicity, assume the grant market is served by a single supplier, one “aggregate funder” that reflects the aggregated behavior of all individual funders. Researchers are the consumers. The aggregate funder’s budget is exogenously determined and the aggregate demand for grants of all types is inelastic. No matter which set of grants the funder chooses to offer, researchers will pursue, and be awarded, all of them. This assumption seems relatively plausible given the fact that published success rates across funders are on the scale of 10-20% (see [here](#) for more). This implies that the funder is ignoring researchers’ preferences, which will lead us to over-estimate their preferences (i.e., our estimate of γ_{fund} will be larger than the true value to the extent funders are in fact incorporating researchers’ preferences into their portfolio decisions).

We assume the funder’s maximization problem is given by:

$$\max_{\mathbf{N}} \sum_{a=\underline{A}}^{\bar{A}} \sum_{d=\underline{D}}^{\bar{D}} \pi(N_{ad}, a^{\gamma_{\text{fund}}} d^{1-\gamma_{\text{fund}}}), \quad (\text{C1})$$

where $\mathbf{N} = (N_{\underline{A},\underline{D}}, \dots, N_{\bar{A},\bar{D}})$ and π is a value function that describes the funder’s net costs and benefits of awarding N_{ad} grants of amount a and duration d . Furthermore, assume that π is well-behaved and increasing and concave in its arguments: $\pi' > 0, \pi'' < 0$. π would be concave if funders prefer strategies that involve diversification across different grant designs, for example, to learn about the value of projects with small investments before making larger investments. Implicitly, π incorporates the funder’s budget constraint.

Since the funder’s valuation of their grant portfolio is separable in the groupings of amount and duration per a and d , then, in equilibrium, they will choose an allocation, \mathbf{N}^* , that equalizes the marginal returns across all groups (i.e., $\frac{\partial \pi}{\partial N_{ad}}$ is equal for all ad groups).

Here, just as changes in prices set by a monopolist firm are informative about changes in the firm’s marginal costs, the observed distribution of grant types (\mathbf{N}^* ; as shown in Figure 1) will be informative about the funder’s preferences. Ideally, we would have some theory or data to be more specific about the shape of π in order to estimate $\frac{\partial \pi}{\partial a}$ and $\frac{\partial \pi}{\partial d}$, but we do not. We can still speak to the *relative* value the funder appears to place on grant size and duration per γ_{fund} .

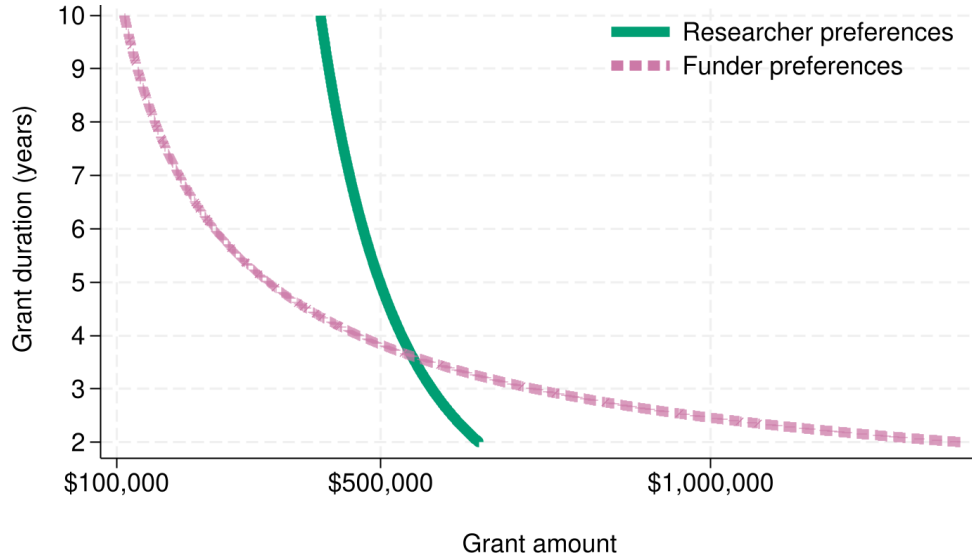
Note that the funder’s choice of N_{ad}^* can be written as $\phi(a^{\gamma_{\text{fund}}} d^{1-\gamma_{\text{fund}}})$, where the shape of ϕ

is governed by π . Then, the relationship between N_{ad}^* and (a, d) will be informative of γ_{fund} since:

$$\frac{\partial N_{ad}^*/\partial a}{\partial N_{ad}^*/\partial d} = \frac{\partial \phi(a^{\gamma_{\text{fund}}} d^{1-\gamma_{\text{fund}}})/\partial a}{\partial \phi(a^{\gamma_{\text{fund}}} d^{1-\gamma_{\text{fund}}})/\partial d} = \frac{\gamma_{\text{fund}} d}{(1 - \gamma_{\text{fund}}) a}. \quad (\text{C2})$$

Thus, by regressing N_{ad}^* on a and d , the ratio of those coefficients (and the sample means) can be used to infer γ_{fund} .

Figure C1: Grant design preferences, researchers versus funders



Note: Shows the preferences of researchers per the survey-based thought experiment (γ), and of funders per observed distribution of research grants in the Dimensions data (γ^{fund}); the intersection of the curves is chosen to be the average grant size and duration.

Using \$100,000 bins for a and 1-year bins for d , a regression of the number of grants in each bin on the grant size and duration indicates that there are -150.3 (s.e.=34.7) fewer grants for every \$100,000 increase and -355.8 (s.e.=72.4) fewer grants for every 1-year increase. These coefficients imply a γ_{fund} of 0.39. Figure C1 plots the indifference curve implied by this estimate, contrasted with researchers’ preferences.

This approach is likely identifying an upper bound of γ^{fund} because of our assumption that funders do not respond to researchers’ demand. Thus, if they are in fact responding to demand already, and we know from the prior analyses researchers’ γ is approaching 0.8, then our estimate of funders’ preferences is biased upward. Still, funders are clearly much more willing to trade off size for duration. Intuitively, the time constraints facing funders must be less important than their cash constraints. But, it is notable how different funders’ and researchers’ preferences appear to be. In a sense, it appears funders are already pulling researchers towards a “longer-smaller” portfolio of grants than they would prefer.

D Additional results on strategic responses

D.1 Main results on strategic responses, reporting sample selection correction estimates

Table D1 replicates the results from the same analyses as reported in Table 5, but here also reporting the coefficient estimates associated with the Inverse Mills Ratio (IMR) covariate and the constant term. In all cases, we do not reject the null hypothesis of no sample selection effects with none of the IMR coefficients being significantly different from zero (Heckman 1979). The strong effects of the participation incentives and reminders reported in Section A.3 suggests that the IMR control is valid in the sense that there is non-trivial variation in the IMR driven by those randomized incentives and reminders. We still cannot rule out that there are no other unobservable dimensions of heterogeneity that are both correlated with researchers’ responsiveness to either the incentives or the reminders as well as our focal parameters (i.e., researchers’ strategic responsiveness to grant designs). However, we have no qualitative, theoretical, or other empirical reason to suggest any specific sources of such lingering selection that we could investigate further.

Table D1: Effect of grant designs on researchers’ strategies, reporting sample selection coefficient

	Faster (1)	More risk (2)	New directions (3)	Larger ongoing projects (4)	More accurate (5)
log(Duration)	-0.056*** (0.015)	0.012 (0.016)	0.008 (0.015)	0.018 (0.015)	0.018 (0.011)
log(Amount)	-0.025*** (0.007)	0.017** (0.007)	-0.016** (0.007)	0.026*** (0.007)	-0.003 (0.006)
IMR	-0.008 (0.007)	0.003 (0.008)	0.010 (0.007)	-0.005 (0.008)	0.000 (0.006)
Constant	0.776*** (0.097)	0.285*** (0.101)	0.533*** (0.095)	0.228** (0.099)	0.178** (0.077)
dep. var. mean	0.36	0.53	0.33	0.61	0.17
<i>N</i> obs.	4,175	4,175	4,175	4,175	4,175

Note: Shows results from OLS regressions where the dependent variables are indicators that equal one if the strategy listed at the top of the column was chosen as one of the “two most important changes” that researchers would make in response to receiving a grant of a given (randomized) size and duration. The IMR row reports the coefficient on the standardized Inverse Mills Ratio. Robust standard errors shown in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Joint estimation of strategic responses

In the main text, we estimate five separate OLS models where we regress an indicator variable for whether the researcher chose each strategy on the (randomized) grant attributes they were provided with. This approach abstracts away from the fact that the researcher is effectively making a single, discrete choice decision about which pair of strategies to select.⁴⁴ To more closely model the structure of this problem, we can convert our data into researcher (i) by strategy (j) level observations and estimate a logit model of the form:

$$\Pr(\text{Strategy change}_{ij} = 1 \mid D_i, A_i, X_i) = \frac{\exp(\log(D_i)\beta_j^D + \log(A_i)\beta_j^A + \mathbf{X}\boldsymbol{\delta})}{1 + \exp(\log(D_i)\beta_j^D + \log(A_i)\beta_j^A + \mathbf{X}\boldsymbol{\delta})}, \quad (\text{D1})$$

which allows the effect of the grant attributes to depend on the strategy being considered. Now, instead of five OLS regressions, we have a single logit regression. Table D2 reports the results of estimating Equation D1 either without any covariates (Col. 1) or by using cross-fit partialing-out Lasso to select and include some covariates (Col. 2) per Chernozhukov et al. (2018a). In both cases, the results mirror those of our simpler, OLS-based approach shown in the main text.

D.3 Heterogeneous effects

Figure D1 plots the individual-level Conditional Average Partial Effects (CAPEs) based on the high-dimensional vector of covariates included in the causal forests (see Tables A1-A2 for the full list of those covariates).

To better understand the sources of that heterogeneity in a more low-dimensional way, Figure D2 reports some of the Best Linear Predictors of the CAPEs (Semenova and Chernozhukov 2021). Loosely speaking, the Best Linear Predictor approach regresses an individual’s CAPE on some covariate in order to test how the CAPE covaries with that observable, effectively testing whether that covariate mediates the focal causal effect. To do this, we make use of the `best_linear_projection` function within the `grf` package (Tibshirani et al. (2022)).

There is no systematic way for choosing which observables to test here, so Figure D2 reports only any observable which yields a p -value in a univariate regression of less than 0.01. The “Univariate coefficient” reported is in standard deviation units and relative to the mean CAPE (i.e., the mean treatment effect). Thus, a univariate coefficient of 0.1 for Humanities would indicate that the effect of the grant attribute on the focal strategy is 0.1 larger for

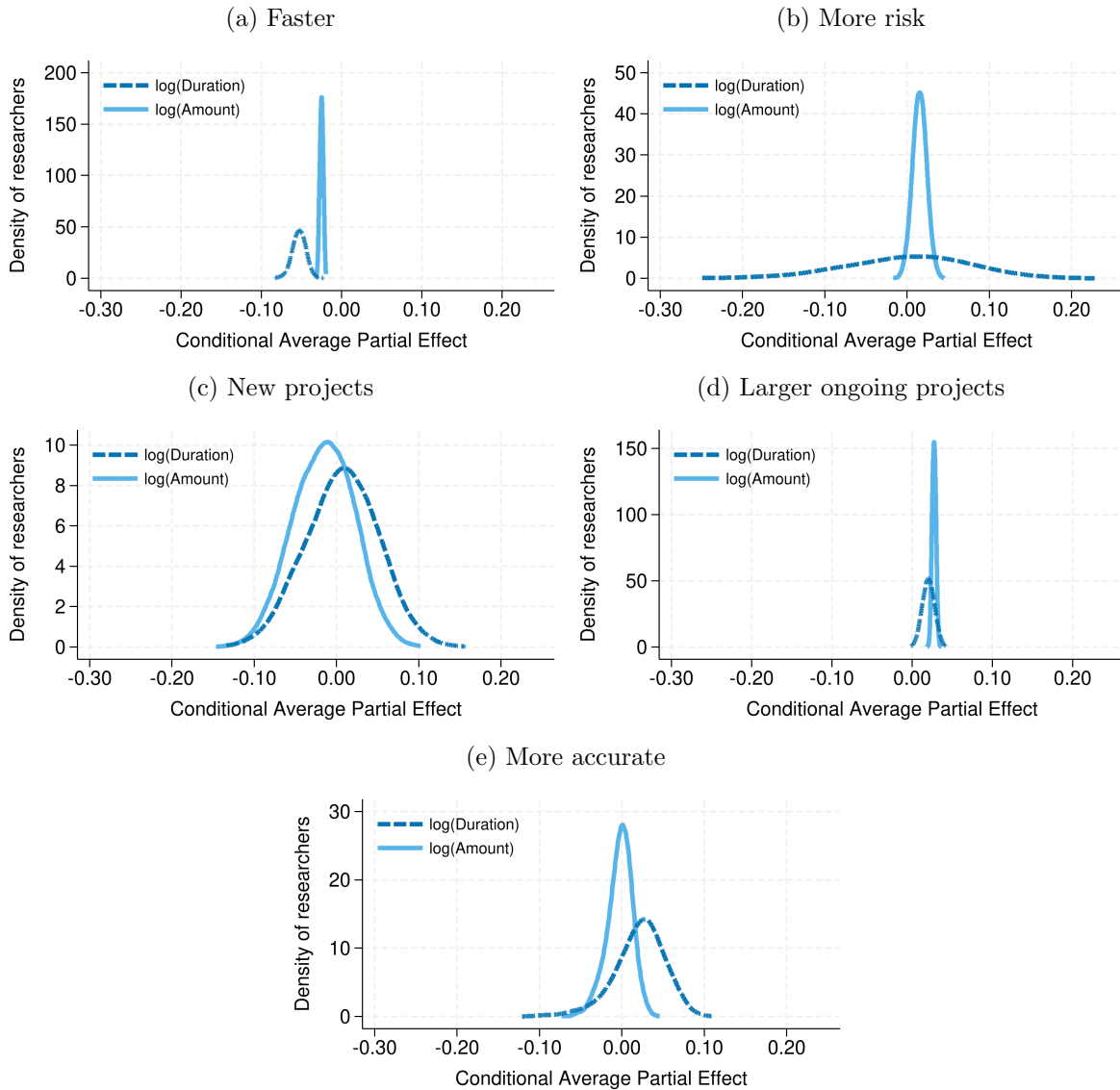
⁴⁴Recall, the researcher is told they have received some grant and are asked to choose the “top 2” most important changes to their research the grant would enable.

Table D2: Logit model of strategy changes in response to grant design

	(1)	(2)
More risk		
× log(Duration)	0.048 (0.062)	0.050 (0.063)
× log(Amount)	0.069** (0.030)	0.069** (0.030)
Faster		
× log(Duration)	-0.239*** (0.065)	-0.243*** (0.065)
× log(Amount)	-0.107*** (0.031)	-0.104*** (0.031)
New direction		
× log(Duration)	0.034 (0.066)	0.043 (0.067)
× log(Amount)	-0.073** (0.031)	-0.079** (0.032)
Ongoing bigger		
× log(Duration)	0.078 (0.063)	0.083 (0.064)
× log(Amount)	0.111*** (0.030)	0.117*** (0.031)
More accurate		
× log(Duration)	0.125 (0.082)	0.122 (0.083)
× log(Amount)	-0.019 (0.040)	-0.019 (0.040)
$L(\mathbf{X})$		Y
Ni obs.	4,175	4,175
Nij obs.	20,875	20,875

Note: Reports the estimates of β_j^D and β_j^S for each of the strategic options (indexed by j) per Eq. D1. Col. 1 includes no covariates and Col. 2 includes any Lasso selected covariates. Robust standard errors shown in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure D1: Heterogeneous effects of grant designs on researchers' strategies



Note: Each panel shows the distribution of the Conditional Average Partial Effects (CAPEs), which are estimated separately for the two randomized grant attributes using the causal forest method of [Wager and Athey \(2018\)](#); the CAPE is defined as $E[cov[W, Y|X]/var[W|X]]$, where $W = \{\log(duration), \log(size)\}$, Y is the dependent variable, and X are the covariates. The only two distributions that show statistically significant heterogeneity are (1) the effect of grant duration on risk-taking ($p < 0.001$) in Panel (b), and (2) the effect of grant funding amount on undertaking new projects ($p < 0.001$) in Panel (c).

researchers in that field compared to the rest of the sample.

Figure D2: Univariate Best Linear Predictors of heterogeneous treatment effects

(a) Faster		(b) More risk	
	Univar. coef.		Univar. coef.
<u>Size</u>		<u>Size</u>	
Outputs: Books	-0.021***	<i>n/a</i>	
<u>Duration</u>		<u>Duration</u>	
<i>n/a</i>		Tenured	0.084***
		Not on tenure-track	-0.097***

(c) New projects		(d) Larger ongoing projects		(e) More accurate	
	Univar. coef.		Univar. coef.		Univar. coef.
<u>Size</u>		<u>Size</u>		<u>Size</u>	
Outputs: Books	0.021***	<i>n/a</i>		<i>n/a</i>	
<u>Duration</u>		<u>Duration</u>		<u>Duration</u>	
<i>n/a</i>		Outputs: Products	0.039***	<i>n/a</i>	

Note: Reports the results from univariate Best Linear Predictors (BLP) regressions of the Conditional Average Partial Effects (CAPEs) on aggregate field indicators as well as other select covariates; only variables with univariate coefficient p -values of 0.01 or less are shown; “n/a” indicates that no covariates had p -values less than 0.01 in the BLP regressions. The mean effects are reported in Table 5. Robust standard errors, which account for the fact that the CAPEs are estimated objects, shown in parentheses; *** $p < 0.01$

D.4 Alternative estimation of heterogeneity

In the main analyses, we estimate researchers’ heterogeneous strategic responses to grant design using the causal forest method of [Wager and Athey \(2018\)](#). That approach is appealing because it allows for the estimation of unit-specific partial effects. However, in order to obtain a lower-dimensional understanding of what covariates are influencing the heterogeneity captured by the random forest, we must ex-post select variables of interest.

An alternative approach is to directly focus on identifying treatment-covariate interaction terms that are “important” with the use of machine learning tools such as the lasso. Here, we follow an algorithm inspired by the approaches of [Imai and Ratkovic \(2013\)](#) and [Blackwell and Olson \(2022\)](#) that uses two stages of lassos. The algorithm is as follows:

1. Generate interaction terms between each randomized grant attributes (amount A and

duration D) and all covariates \mathbf{X}

2. Perform a lasso regression of a given dependent variable on $(A, D, A \times \mathbf{X}, D \times \mathbf{X}, \mathbf{X})$; let $L_{A,D,\mathbf{X}}^1$ denote the subset of attribute-covariate interaction terms selected in this step, i.e., from the possible set $(A \times \mathbf{X}, D \times \mathbf{X})$
3. Perform a cross-fit partialing-out lasso linear regression of a given dependent variable on $(A, D, L_{A,D,\mathbf{X}}^1)$ using \mathbf{X} as the set of high-dimensional controls (Chernozhukov et al. 2015)

This approach relies on a separate penalty parameter to be used in the selection of interaction terms (Step 2.) and in the selection of covariates (Step 3.) as suggested by Imai and Ratkovic (2013). And by using a post-selection inference method (Chernozhukov et al. 2015), we are attempting to avoid regularization biases as noted by Blackwell and Olson (2022).

Table D3 reports the results from this alternative approach. Overall, the results are consistent with the random forests approach. Notably, tenure status is again observed to be a clear moderator of the effect of grant duration on risk-taking. The main effect estimates also align with our other estimation approach.

D.5 Returns to scale and new versus old projects

Here, we provide a stylized model of a producer’s decision to allocate a windfall of inputs into “new” or “ongoing” projects and how this decision depends on the returns to scale of the producers’ production function. The purpose of this model is to provide guidance on how to interpret the effect of grant designs on researchers’ decisions to invest grant resources in new or ongoing projects (as reflected by the β^{new} and β^{ongoing} coefficients that we estimate in Section 5.1 (see Eq. 5).

The producer has a production function $f()$ that is common to all projects.⁴⁵ The production function converts inputs X into some output with a constant marginal payoff normalized to 1. For now, assume only that output is increasing in inputs and non-negative: $f' > 0$ and $f(0) = 0$. The producer has two projects that they can invest in: an ongoing project where \bar{X}_{ongoing} has already been invested, and a new project where \bar{X}_{new} has already been invested. As suggested by the names, more inputs have been invested in the ongoing project: $\bar{X}_{\text{ongoing}} > \bar{X}_{\text{new}}$.⁴⁶

⁴⁵Our goal here is to provide a rationalization of results that does not rely on uncertainty, since we capture that dimension separately in the survey. Hence, we abstract away from the reality that new projects may be more uncertain than ongoing projects.

⁴⁶We do not formally model the source of this differential investment, but it could be due to the unmodeled

Table D3: Heterogeneous strategic responses to grant attributes

	Faster	More risk	New directions	Larger ongoing projects	More accurate
log(Duration)	-0.030 (0.023)	-0.052** (0.023)	0.015 (0.017)	0.019 (0.015)	0.019* (0.011)
× Male	-0.044 (0.030)				
× Tenured		0.094*** (0.030)			
× On t.t., not tenured			-0.045 (0.035)		
× Audience: Policymakers					0.015 (0.011)
log(Amount)	-0.018 (0.013)	0.012 (0.010)	-0.016** (0.007)	0.026*** (0.008)	-0.003 (0.006)
× Age	0.002 (0.008)				
× Assistant professor	0.027 (0.028)				
× Full professor	-0.014 (0.017)	0.011 (0.016)			
× On t.t., not tenured	-0.015 (0.029)				
× Engineering & related	-0.036** (0.018)				
× Total salary		0.001 (0.008)			
× Fundraising expect.		0.005 (0.006)			
× Research risk, own belief			-0.012 (0.007)		
× Humanities & related				0.009 (0.018)	
Incl. main effect	Y	Y	Y	Y	Y
Lasso method	plug-in	plug-in	plug-in	plug-in	plug-in
NX var. sel.	11/62	19/62	7/62	4/62	7/62
dep. var. mean	0.36	0.53	0.33	0.61	0.17
N obs.	4,175	4,175	4,175	4,175	4,175

Note: Robust standard errors shown in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To connect this model to our experiment, consider a scenario where the producer randomly receives a windfall of additional inputs; let the size of this windfall be given by Δ . Now, they must choose some fraction s of Δ to allocate to the ongoing project, with the remainder $(1-s)$ being allocated to the new project. Thus, their optimization problem is as follows:

$$\max_{s \in [0,1]} f(\bar{X}_{\text{ongoing}} + s \times \Delta) + f(\bar{X}_{\text{new}} + (1-s) \times \Delta) . \quad (\text{D2})$$

The optimal allocation choice (s^*) is either a corner solution (i.e., $s^* = \{0, 1\}$) or an interior solution (i.e., $s^* \in (0, 1)$). If an interior solution exists, it equates the marginal product across the two projects:

$$f'(\bar{X}_{\text{ongoing}} + s^* \times \Delta) = f'(\bar{X}_{\text{new}} + (1-s^*) \times \Delta) . \quad (\text{D3})$$

The producer's choice of how to allocate this windfall of inputs is analogous to researchers' choice of how to respond to the hypothetical grant in our experiments by either starting new projects or scaling up ongoing projects. Here in this model, the key comparative static is $\partial s^* / \partial \Delta$, which describes how the producer's allocation choice depends on the size of the input windfall. In our regressions using the experimental data (per Eq. 5), the coefficients β^{new} and β^{ongoing} capture the same relationship: how researchers' decisions to focus on new or ongoing projects depends on the amount of grant resources they've (randomly) received.

In the model, the sign of $\partial s^* / \partial \Delta$ will depend on the shape of f , which yields three possible outcomes when we map this to our empirical setting. If there are constant returns to scale ($f'' = 0$), then there is no relationship between s and Δ , since marginal returns are equal at all initial investment levels. If this is true among the researchers in our experiment, then we should find $\beta^{\text{new}} = \beta^{\text{ongoing}}$, since the amount or duration of a grant will have no bearing on researchers' decision to focus on either project type. Similarly, if f is convex ($f'' > 0$), then the optimal choice in the model is the corner solution of always investing all of Δ into the ongoing project, regardless of the size of Δ . Analogously, if researchers' within-project returns to scale are convex, then their choice to focus on new versus ongoing projects would be independent of the amount or duration of a grant and we'd estimate $\beta^{\text{new}} = \beta^{\text{ongoing}}$.

If the returns to scale are concave ($f'' < 0$), then, in the model, the producer will allocate a larger portion of larger windfalls to the ongoing project ($\partial s^* / \partial \Delta > 0$).⁴⁷ If this is true

differences in risk or differences in input costs (e.g., it is initially harder to raise money for new directions).

⁴⁷To see this, note that global concavity in f would imply that the interior solution to Eq. D3 simplifies to $\bar{X}_{\text{ongoing}} + s^* \times \Delta = \bar{X}_{\text{new}} + (1-s^*) \times \Delta$, which yields: $s^* = \frac{\bar{X}_{\text{new}} + \Delta - \bar{X}_{\text{ongoing}}}{2\Delta}$. Thus, $\frac{\partial s^*}{\partial \Delta} = \frac{\bar{X}_{\text{ongoing}} - \bar{X}_{\text{new}}}{2\Delta^2}$, which is greater than zero because $\bar{X}_{\text{ongoing}} > \bar{X}_{\text{new}}$.

among the researchers in our experiment, then we should find $\beta^{\text{new}} < \beta^{\text{ongoing}}$, since larger (or longer) grants would increasingly lead researchers to allocate resources towards ongoing projects.

The final scenario possible in the model is $\partial s^*/\partial \Delta < 0$, which is only possible if there are both increasing and decreasing returns to scale (i.e., f'' is non-monotonic). This could be possible if there were both convex and concave portions of the production function (e.g., sigmoid-shaped functions with convexity at low input levels and concavity at high input levels). Practically, this could correspond to fixed costs unique to new projects. If this scenario holds among the researchers in our experiment, then we should find $\beta^{\text{new}} > \beta^{\text{ongoing}}$, since larger (or longer) grants would increasingly lead researchers to allocate resources towards new projects to take advantages of the increasing returns to scale at lower levels of investments.

To summarize, the model delivers three scenarios that we can separately identify in our data based on the relative magnitudes of the regression coefficients as follows:

$$\partial s^*/\partial \Delta \begin{cases} = 0, \text{ if } f'' \geq 0 \text{ (convex or linear)} & \implies \beta^{\text{new}} = \beta^{\text{ongoing}} \\ > 0, \text{ if } f'' < 0 \text{ (concave)} & \implies \beta^{\text{new}} < \beta^{\text{ongoing}} \\ < 0, \text{ if } f'' \text{ is non-monotonic} & \implies \beta^{\text{new}} > \beta^{\text{ongoing}} \end{cases} .$$

D.6 Effects at the limits of grant design

To get a sense of the limits grant designs, Figure D3 plots the predicted choice probabilities for each strategy given designs at the limits of the dollar amount and duration explored in the thought experiment. These predicted probabilities are based on the point estimates from the logistic regression model. Thus, they are “generous” in that we do not force any point estimates that do not reject the null (per their standard errors) to zero.

We find that a “MIRA-sized change in grant design” (i.e., an additional 30% in duration and a 15% decline in amount) leads to no more than a 1 p.p. change in the probability of any strategy being chosen. Using the full support of grant designs from the thought experiment, the point estimates and confidence intervals indicate that it takes a tremendous amount of difference in grant design (i.e., plus or minus approximately \$2 million and/or 8 years) to change choice probabilities by an amount that could reasonably be considered meaningful (i.e, approximately 10 p.p.). For comparison, \$2 million is an order of magnitude larger than the median funding expectations of professors in the sample, and 8 years is roughly 3 times the average duration of non-tenure-track professors contracts (approximately 2.5 years) and the average amount of time before a pre-tenure professor’s tenure evaluation (approximately

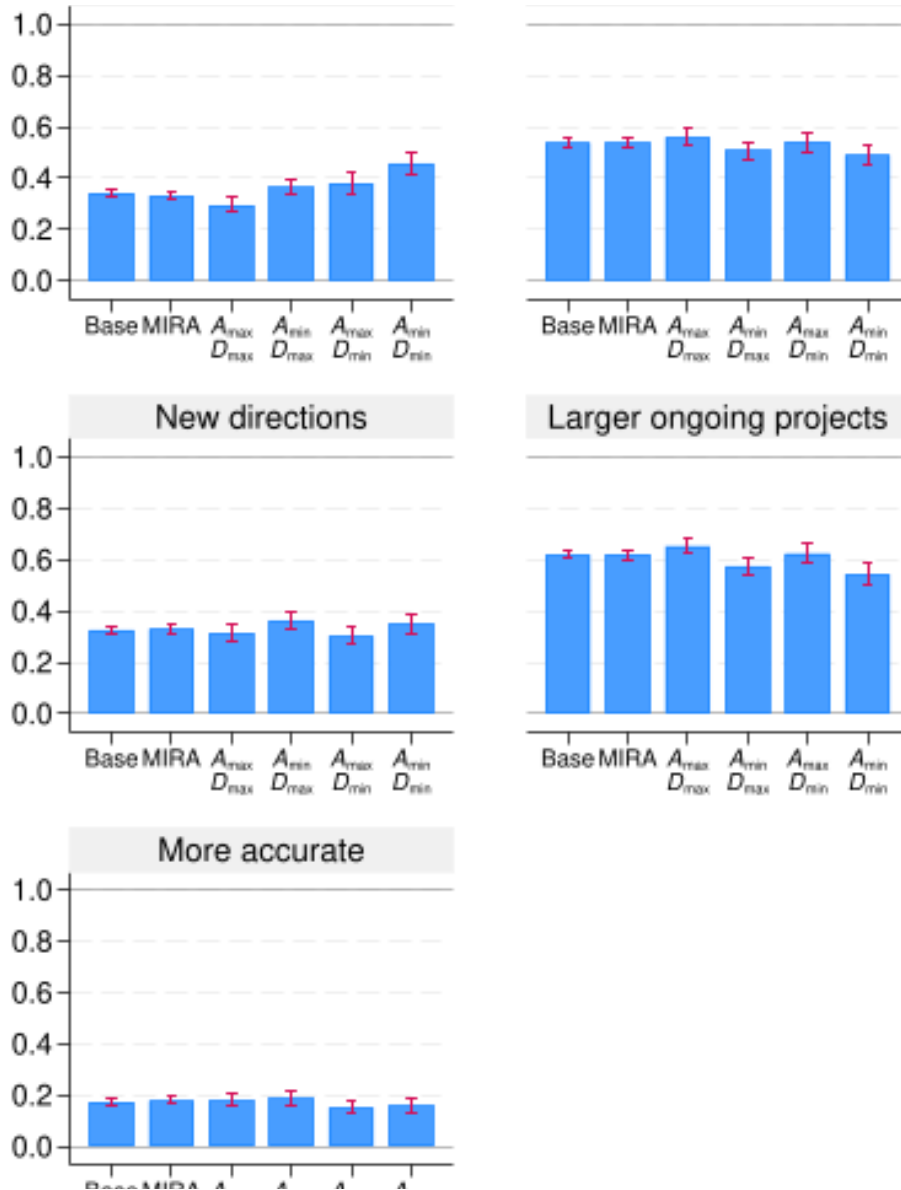
2.5 years). Table D4 summarizes these ranges based on estimates from the full sample, as well as estimates from just the non-tenured and tenured subsamples.

Table D4: Span of probabilities at observed and limits of grant designs

	mean	MIRA	Point Estimate		95% C.I.	
			min	max	min	max
<u>Panel (a): Full sample</u>						
Faster	0.34	0.33	0.29	0.46	0.27	0.50
More risk	0.54	0.54	0.49	0.56	0.45	0.59
New directions	0.33	0.33	0.30	0.36	0.27	0.40
Larger ongoing projects	0.62	0.62	0.54	0.65	0.50	0.68
More accurate	0.17	0.18	0.15	0.19	0.13	0.22
<u>Panel (b): Non-tenured</u>						
Faster	0.40	0.39	0.36	0.48	0.32	0.54
More risk	0.47	0.45	0.42	0.52	0.37	0.58
New directions	0.31	0.32	0.27	0.35	0.22	0.41
Larger ongoing projects	0.63	0.63	0.55	0.66	0.48	0.70
More accurate	0.20	0.21	0.17	0.22	0.13	0.27
<u>Panel (c): Tenured</u>						
Faster	0.30	0.29	0.25	0.44	0.22	0.50
More risk	0.59	0.60	0.50	0.63	0.44	0.67
New directions	0.34	0.34	0.32	0.37	0.28	0.43
Larger ongoing projects	0.62	0.61	0.54	0.65	0.49	0.69
More accurate	0.16	0.16	0.14	0.17	0.11	0.20

Note: Reports the predicted probabilities of choosing a strategy when receiving (Column 1) an average grant in the thought experiment (“Base”); (Column 2) a grant that is 30% longer and 15% smaller than average, reflecting the same relative change as the MIRA grants compared to traditional R01 grants at the NIH (“MIRA”); (Columns 3-6) report the minimum and maximum choice probabilities obtained under any grant design in the thought experiment using either the point estimates or the 95% confidence intervals.

Figure D3: Choice probabilities at observed and limits of grant designs



Note: Reports the predicted probabilities of choosing a strategy when receiving (1) an average grant in the thought experiment (“Base”), (2) a grant that is 30% longer and 15% smaller than average, reflecting the same relative change as the MIRA grants compared to traditional R01 grants at the NIH (“MIRA”); (3-6) grants with the largest or smallest dollar amount in the thought experiment ($A_{max} = \$2,000,000$; $A_{min} = \$100,000$) and the longest or shortest duration in the thought experiment ($D_{max} = 2$ years; $D_{min} = 10$ years). All predicted probabilities are based on the logit models.