

The Causal Effects of R&D Grants: Evidence from a Regression Discontinuity

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Abstract. We leverage the discontinuity in the assignment mechanism of the Small and Medium Enterprise Instrument - the first European R&D subsidy targeting small firms - to provide the broadest quasi-experimental evidence on R&D grants over both geographical and sectoral scopes. Grants trigger sizable impacts on a wide range of firm-level outcomes. Heterogeneous effects are consistent with grants reducing financial frictions. This reduction is due to funding rather than certification. We also provide direct causal evidence on pure certification - signaling not attached to funding - and show that firms that only receive ‘quality stamps’ do not improve their performance. Finally, our estimates suggest that the scheme produces private returns that are positive and comparable to those of the US Small Business Innovation Research program, while also generating geographical and sectoral spillovers in the form of increased rates of entrepreneurial entry.

Keywords: Regression discontinuity design · Research and development · Innovation Policy · SMEs

JEL: D22 · G24 · G32 · L53 · O31 · O38

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1 Introduction

The use of government funding to stimulate business research and development (R&D) is a broadly accepted remedy to private under-investment in R&D due to the presence of knowledge spillovers (Nelson, 1959; Arrow, 1972) and financial constraints (Hall and Lerner, 2010). These market failures affect above all young and small innovative firms.¹ Among the most common policy instruments designed to overcome these frictions, R&D grants represent the most direct form of support to private innovation efforts. Differently from other policy measures (e.g. R&D tax credits), R&D grants are in principle better equipped to affect both the rate and the direction of technological change and may be deployed to prioritize areas plagued by heavier market failures or to address specific societal challenges (Azoulay and Li, 2022; Van Reenen, 2020). Despite the tendency to report positive results, the available empirical evidence does not provide a definitive answer on the effectiveness of R&D subsidies (Dimos and Pugh, 2016; Bloom et al., 2019).² Recent studies, while adopting more rigorous identification strategies, also report mixed results (Bronzini and Iachini, 2014; Howell, 2017; Wang et al., 2017). The need for further robust evidence on the effects of R&D grants (Bloom et al., 2019; Hünermund and Czarnitzki, 2019b) is even more critical as it is not clear which is the prevailing causal mechanism through which such effects materialize. Grants might benefit firms by “signaling” their quality to private investors (i.e. certification) or by allowing them to secure the resources to successfully develop a technology (i.e. funding). However, disentangling certification from funding is problematic and direct causal evidence on certification is not available.

¹ Such barriers to innovation might be particularly detrimental to aggregate economic outcomes given the prominent contribution of young-small firms to net job creation (e.g. Haltiwanger et al. 2013) and their higher propensity to introduce radical innovations (e.g. Baumol 2005).

² Studies reporting a positive impact of R&D subsidies on firm outcomes include Lerner (2000), González et al. (2005), Einiö (2014), Howell (2017), Azoulay et al. (2019) and Widmann (2020). Conversely, Wallsten (2000), Klette et al. (2000), Lach (2002), De Blasio et al. (2014), Wang et al. (2017) find no effect. Others, such as Bronzini and Iachini (2014) and Hünermund and Czarnitzki (2019a), find no average impact, but detect large heterogeneous treatment effects. David et al. (2000) and Zúñiga-Vicente et al. (2014) review the literature on R&D subsidies while Dimos and Pugh (2016) provide a meta-regression analysis of the literature. More recent reviews are presented in Hünermund and Czarnitzki (2019b), Vanino et al. (2019) and Bloom et al. (2019).

Against this backdrop, the paper provides the broadest quasi-experimental evidence over sectoral and geographical dimensions on the impact of R&D grants available to date. More specifically, it studies the effects of the Small and Medium Enterprise Instrument (henceforth, SME Instrument), the first European R&D grant program directly targeting innovative small and medium-sized businesses. Firms compete to secure grants of up to €2.5 million to finance R&D activities. In each competition firms are ranked by independent external experts, and winners are selected based ultimately on budget availability. We leverage this aspect of the policy assignment mechanism and adopt a sharp regression discontinuity (RD) design (Lee and Lemieux, 2010) to identify the causal effect of R&D subsidies.

We estimate the effects of R&D grants on a wide number of firm-level outcomes encompassing several steps of the innovation-to-market process. Results indicate that grants trigger an increase in subsequent firm investment, especially in intangibles. Grants also induce an increase between 15 and 31% in innovation output as measured by cite-weighted patents. This additional amount of patents is due to both intensive and extensive margins. In other words, the effects of R&D grants are not limited to firms already engaged in innovative activities, but extend to firms' selection into patenting. R&D grants also represent a catalyst for follow-on equity investments: firms experience a higher likelihood of receiving private equity (over 100% increase), and this is associated with larger funding rounds and a higher number of deals. Furthermore, grants are conducive to faster firm growth (28-56%) and lower likelihood of failure (over 100% decrease).

The cross-country and cross-industry setting also allows us to explore heterogeneous responses over several dimensions. We report larger effects for younger and smaller businesses and for firms that operate in sectors with higher financial vulnerability. We also observe larger benefits for firms located in countries and regions with lower economic development. These findings are consistent with grants reducing the financial frictions that characterize the innovation process. This reduction in financial frictions might accrue through i) certification effects (i.e. the grant signals firms' quality to the market) or ii) funding effects (i.e. the firm uses the grant to

successfully develop a technology). Our estimates suggest that funding is the primary mechanism behind the effects. For example, we show that the increase in the probability of receiving follow-on equity is mainly driven by firms patenting after the competition. This indicates that the grant money allows firms to invest in R&D and develop a technology that is ultimately patented. If anything, the certification effect at work is the quality signal conveyed to external investors through the patent. Additional tests confirm that funding effects are overall much more important than certification effects.

We also provide causal evidence on pure certification effects (i.e. signaling not attached to funding). To shed light on this, we exploit a unique feature of the program: firms that deserve funding according to experts' evaluation, but do not get it only due to lack of sufficient budget, receive a certificate called "Seal of Excellence" (SOE). This is intended to signal the high-quality of the innovation project proposed by the firm to other funding bodies and private investors, so as to increase its chance of securing alternative funding. We leverage the assignment of SOEs to test whether pure certification effects, not attached to funding, trigger any increase in firm performance. Our results show that SOE firms do not perform better than non-grantees, indicating that certification alone does not seem to generate any positive impact on firm performance. In sum, we do not find any evidence, either direct or indirect, in favor of certification, thus supporting the funding channel as the primary mechanism behind the results.

Back-of-the-envelope calculations suggest that the private returns to recipient firms are positive and comparable to those of the US Small Business Innovation Research (SBIR) program. In particular, we show that the SME Instrument spawns approximately 0.76 patents per million euro of R&D, which is similar to the 0.88-0.96 patents per million euro of R&D the US SBIR generates (Myers and Lanahan, 2022). Finally, apart from improving recipients' performance, we document that the program generates positive spillovers on entrepreneurship: grants awarded to firms in a given geographical-technological area foster the subsequent creation of similar firms in that location.

The paper contributes to the recent literature exploiting discontinuities to estimate the causal effects of R&D grants. Bronzini and Iachini (2014) study a regional R&D program in Italy and find no impact of grants on firm investment. Howell (2017) examines the Department of Energy’s (DoE) SBIR program and finds substantial impacts on several productive outcomes. Wang et al. (2017) examine an innovation subsidy program in China and find no effect in terms of survival, patenting, or venture funding. Zhao and Ziedonis (2020) focus on a Michigan-based program finding positive effects on survival and external financing but no impact on patenting. A common feature of these studies is that they focus on sector-specific or region-specific programs, thus limiting external validity and making the generalization of the results quite difficult. Our paper leverages a much broader policy intervention in terms of both sectoral and geographical scope, featuring applications from firms located in more than 40 different countries and operating in virtually any sector of activity. This unique cross-country and cross-industry setting also allows us to test for heterogeneous effects over more dimensions (i.e. sectors, countries and regions) than usually explored in the literature.

We also contribute to the long-standing debate on certification vs funding. Earlier studies reported that the impact of grants materialize due to grants acting as market signals about the quality of recipients (Lerner, 2000; Feldman and Kelley, 2006; Meuleman and De Maeseneire, 2012). Conversely, Howell (2017) attributes the effects to funding rather than certification. A key difference between our study and previous work is that the assignment of a ‘quality label’ (i.e. SOE) to firms that do not receive funding allows us to provide the first causal evidence on pure certification effects within the R&D program evaluation literature. Finally, we add to the literature addressing spillovers from innovation policies (e.g., Bloom et al. 2013; Azoulay et al. 2019; Myers and Lanahan 2022) by showing that the program indirectly promotes entrepreneurship. In contrast with previous studies documenting similar results (Audretsch et al., 2002; Qian and Haynes, 2014), our research design allows for a causal interpretation of this finding.

The results of the paper are important for policy. The SME Instrument is a case of cross-national policy transfer, since it was modeled after the successful US Small Business Innovation Research (SBIR) (Mazzucato, 2015). This study provides the first quasi-experimental evidence on the impact of SBIR-type policies implemented outside the US. Hence, the analysis is highly relevant for practitioners and policy-makers managing or considering this kind of scheme in other countries. Furthermore, assessing the effectiveness of R&D grants in European countries is of utmost importance given that Europe has traditionally lagged behind the US in terms of funding opportunities for start-ups and small firms with more radical projects (O’Sullivan, 2005; Hall et al., 2016; Cincera et al., 2016). This funding gap is arguably one of the factors behind the so-called “European paradox”, namely, the relative inefficiency of European countries in translating scientific advances into marketable innovations, growth, and jobs. To alleviate these frictions, the creation of a European SBIR ‘equivalent’ has been the object of long-standing debates among scholars and policy-makers (Encaoua, 2009; Connell, 2006; Mazzucato, 2015). The SME Instrument represents the EU’s attempt to bridge this gap and the evidence is that it is effective in helping start-ups and small firms to bring new ideas to market.

The remainder of this paper is organized as follows. In Section 2 we detail the key institutional features of the SME Instrument and provide an overview of the data. Section 3 describes the empirical strategy and presents tests of the validity of the RD design. Section 4 contains the estimation results. Section 5 explores the specific mechanisms behind the effects of the policy. Section 6 addresses the value-for-money of the program and provides evidence on spillovers. Robustness checks are contained in Section 7 while Section 8 brings the paper to a close.

2 Institutional setting and data

2.1 The SME Instrument

The SME Instrument was established in 2014 and was rolled out by the Executive Agency for Small and Medium-sized Enterprises (EASME) to provide innovation support to SMEs. With

a budget of around €3 billion over 2014-2020, its goal has been the selection and funding of companies with the most innovative ideas and highest growth potential. Until its introduction, at the pan-European level there was no dedicated policy tool designed to support directly the innovative efforts of individual SMEs (Di Minin et al., 2016). EU innovation policies had been traditionally much more focused on cooperative R&D projects bringing together science and businesses to promote cross-border technological innovation. In this framework, SMEs could indirectly benefit from policy support only as part of larger consortia.³ On the contrary, the SME Instrument allows individual SMEs to apply for support as sole beneficiaries.

Firms can submit their proposals in one of four yearly application cycles. They apply to competitions that are sector-specific and organized in 13 different topics.⁴ A proposal will be taken into consideration if all three of the following conditions are met: i) the applicant is a for-profit SME⁵, including newly created companies and start-ups; ii) the applicant is established in a EU Member State or a Horizon2020-associated country⁶; iii) the applicant is not found in a situation of concurrent submission or implementation with another SME Instrument proposal.

Firms compete to secure grants that can range between a minimum of €0.5mln and a maximum of €2.5mln.⁷ Fundable R&D activities encompass prototyping, testing, design, performance evaluation, monitoring, demonstration, piloting, validation for market duplication, scaling-up

³ Examples of these EU policies not targeting individual SMEs are the Fast-Track to Innovation (FTI) and the Eurostar II programs (Hünemund and Czarnitzki, 2019a).

⁴ For every cut-off date, there can be multiple competitions in each topic. Descriptive statistics for applicants across topics are reported in Table A2.

⁵ SMEs are defined by the European Commission as having less than 250 persons employed, an annual turnover of up to €50 mln, or a balance-sheet total of no more than €43 mln.

⁶ Appendix Table A4 contains further details on applicants' countries. For an overview of the policy including statistics concerning the distribution of applicants (and winners) by country see European Commission (2018)

⁷ These grants are officially called Phase II grants, and represent 90% of the overall budget of the program. Similar to the US SBIR, the SME Instrument also offers proof-of-concept grants (i.e. Phase I) to test the commercial and technological feasibility of a business idea. Yet, one major difference between the two programs is that the phases of the SME Instrument are non-sequential (i.e. firms can apply directly to Phase II) and can be considered as two separate programs. Additionally, Phase I grants are much smaller than those awarded by the SBIR (€50,000 vs \$150,000) and were recently discontinued by EASME. In this paper we focus on Phase II grants. In the Appendix we show that previous wins or participation in Phase I grants do not affect our results. In the working paper version of this document (Santoleri et al., 2020), we show that Phase I grants do not affect firm performance, arguably due to their small size as well as and their focus on early-stage proof-of-concept activities, which are far away from market applications.

and application development. Grants cover 70% of all eligible costs of the proposed project⁸ for a period between 12 and 24 months. The expected result is a product, process or service that is ready to compete on the market.

SMEs apply to a given competition by submitting a 30-page proposal that should include a business plan and a description of the proposed activities. The proposals are evaluated by four independent experts appointed by EASME.⁹ The evaluation procedure is conducted remotely. Evaluators work independently from one another and are not aware of the assessment of their peers (European Commission, 2018). Also, they do not know *ex ante* the effective number of grants that will eventually be awarded in the competition.

The experts score the projects on three counts : i) impact, ii) excellence, and iii) quality and efficiency of implementation, each on a scale from 0 to 5. The final score for each project is calculated by adding up the median scores on all three criteria, thus ranging from 0 to 15.¹⁰ The projects are then ranked based on these scores. Only those that are above a minimum quality threshold (i.e. 12 points) are eligible for the grant. However, not all of them will receive a grant since the funding allocated to each competition is limited and the effective number of grants is ultimately a function of EASME budgetary constraints.¹¹ The projects that are considered worthy of funding, but do not win the grant only because of insufficient budget, receive a certificate called “Seal of Excellence” (SOE). This represents a ‘quality label’ recognizing the high value of the proposal. The SOE was instituted by EASME to give companies more external visibility and to facilitate access to alternative, private or public, sources of funding.

⁸ This share goes up to 100% in health-related topics for which firms can receive grants of up to €5mln.

⁹ These are selected from a pool of around 1,500 experts. A yearly rotation of 20% of experts helps to ensure an impartial treatment of the projects submitted. Experts can apply to be evaluators through a call for expressions of interest. As a general rule, expert evaluators coming from the same country as the application will not be assigned to its assessment (European Commission, 2018).

¹⁰ Scores can take any value from 0 to 15 and are rounded to two decimal places.

¹¹ It is important to stress that the funding amount for each competition is decided *ex ante*. This does not vary depending on the number of applicants nor on the number of firms considered eligible for the grant by the experts. Grants will be assigned to firms above the minimum quality threshold until the funds allocated to the competition are exhausted.

In principle, the assignment mechanism described above could allow the use of a fuzzy RD design (Lee and Lemieux, 2010) since firms that are above the minimum quality threshold do not automatically win the grant. However, in this scenario we would need to use the scores as a running variable instead of the final ranks. Unfortunately, this is not possible as we do not observe the scores but only the ranks. Therefore, our identification strategy, as outlined in Section 3, will exploit the sharp discontinuity in terms of ranks between two groups of firms, namely, those that win the grant and those that do not.

2.2 Data and summary statistics

We have access to confidential data concerning all SME Instrument competitions organized by the EASME. While the list of winners for each competition is public, the information concerning competitions' applicants and rankings is not. These confidential data include information on the applicant's firm name, country, funded status, requested and approved funding amount, competition and final ranking.

Table 1 reports summary statistics concerning the 176 competitions that took place between 2014 and 2017 (Panel A and B). During this period, the number of applications submitted to the program was 14,904, whereas the number of grants awarded was 719. On average the number of applicants per competition is 85. Of these, around 7% are eventually awarded a grant, whereas roughly 36% are considered worthy of funding, but since they cannot be granted the award because of budgetary constraints, they receive the SOE (Table A1). The average grant size of the SME Instrument is €1.6mln, which is comparable to what the US SBIR program offers through its Phase II program (Lerner, 2000; Howell, 2017).

We employ the ORBIS Bureau van Dijk's (BvD) company database to link applicants data with firm-level outcomes. Absent the possibility to access harmonized country-specific business register data, ORBIS represents the best available source of comparable cross-national firm-level

data (Autor et al., 2020).¹² Based on probabilistic matching on firm name and exact matching on country, we retrieved longitudinal information about applicants to SME Instrument calls for the period 2014-2017.¹³ After the exclusion of 22 firms with revenues and/or employees not complying with the SME Instrument eligibility criteria, we are able to successfully match 74% of all firm-applications.¹⁴

In order to assess the impact of the policy on innovation outcomes, we use the ORBIS Intellectual Property database¹⁵ to retrieve information regarding all patent applications and their forward citations up to 2019.¹⁶ Instead of resorting to a simple patents count, which would neglect their heterogeneity, we weight each patent by its forward citations to better assess its impact and commercial potential. In doing so we follow a well-established approach: forward patent citations are a good indicator of the ‘quality’ of the innovation (Trajtenberg, 1990) a predictor of both patents and firms market value (Harhoff et al., 1999; Hall et al., 2005) and are correlated with product innovations (Argente et al., 2020). We retrieve from the ORBIS Zephyr database private financing data (time-span 1997-2019). The availability of balance-sheet data gives us longitudinal records of investment, total assets, employment and revenues. We also link firm-applications with data on the status of the firm at the beginning of 2019. This information allows us to assess whether each firm is still active or has exited due to failure or through initial

¹² While representativeness has been improving during recent years, ORBIS still does not provide an optimal coverage of smaller firms. This is especially so for balance-sheet variables as national business registers allow them to file simplified financial accounts, with requirements that vary from country to country and across variables.

¹³ We exclude 2019 competitions because we need at least one post-treatment year. Also, we exclude 2018 competitions because of changes introduced to the SME Instrument in the 2018-2020 work program: since 2018 the SME Instrument has no topics, all proposals are in competition with each other, and one last screening step has been added to the evaluation procedure in the form of interviews between experts and applicants (European Commission, 2018).

¹⁴ Appendix Table A3 shows that standardized differences between the population of applicants and the BvD-matched sample are negligible with most variables featuring values below the conservative threshold of 0.10.

¹⁵ The information is sourced from the PATSTAT database of the European Patent Office. The match between ORBIS and PATSTAT is carried out by Bureau van Dijk under a mutual agreement with the OECD. Squicciarini and Dernis (2013) show that the share of successful matches of patent records between PATSTAT and ORBIS is above 90% for selected OECD countries.

¹⁶ A patent is a DOCDB patent family comprising an application to the European Patent Office. The filing date of a patent is defined as the earliest filing date within each patent family. We resort to patent applications in line with most of the innovation literature and because of the short post-treatment time window that characterizes our sample. However, we also re-run the entire analysis using granted patents and find qualitatively similar results.

public offering (IPO) or merger and acquisition (M&A).¹⁷ To mitigate the influence of outliers, all balance-sheet variables are winsorized at the 2% on both tails of the distribution whereas patent variables are winsorized at the 98th percentile.

Descriptive statistics of R&D grant competitions and firm-level variables are reported in Table 1 (Panel C). Firms applying to SME Instrument competitions tend to be young, with a median age of 5 years old. They also tend to be small with a median of 8 employees. Roughly 60% operate in medium or high-tech manufacturing or high-tech knowledge-intensive services.¹⁸ The median firm is not patent-active and a very small share of applicants has received some external private financing. Finally, around 6% of all applicants have failed by 2019, whereas IPO events are extremely rare.

3 Empirical strategy

Our identification strategy leverages the policy’s assignment mechanism: firm proposals are ranked according to experts’ evaluation, but funding availability is the ultimate determinant of the number of grants awarded in each competition. We exploit this discontinuity and adopt a sharp RD design comparing firms around the threshold. The RD approach, first introduced by Thistlethwaite and Campbell (1960), is based on the idea that treatment assignment around the threshold is approximately random (Lee, 2008). In this context, firms that are close to the threshold on either side are assumed to be very similar, and potential differences in the post-treatment performance of beneficiaries and non-beneficiaries can be attributed to the grant.

¹⁷ Even though Zephyr’s coverage of private-firm acquisitions is superior to alternative databases (Erel et al., 2015), we cannot completely rule out that some IPOs or M&As were not recorded in Zephyr, which could also imply that some firms are wrongly recorded as failed.

¹⁸ These are identified at the 2-digit NACE Rev. 2 drawing on Eurostat definitions (https://ec.europa.eu/eurostat/cache/metadata/Annexes/htec_esms_an3.pdf).

In order to assess the causal effect of the SME Instrument, we estimate the following equation by means of ordinary least squares (OLS):

$$Y_{ic}^{Post} = \alpha + \beta Grant_{ic} + f(Rank_{ic}) + \gamma Y_{ic}^{Pre} + \delta_c + \varepsilon_{ic} \quad (1)$$

where $-r \leq Rank_{ic} \leq r$

Y_{ic}^{Post} is the post-treatment outcome for firm i in competition c , $Rank_{ic}$ is the centered rank assigned by experts to firm i in competition c , $Grant$ is an indicator for firm i being awarded a grant in competition c (i.e. $Rank_{ic} > 0$), and $f(Rank_{ic})$ is a polynomial control for centered ranks. All regressions feature competition fixed effects (δ_c). These fixed effects effectively restrict the comparison to applicants participating to the the same competition, thus controlling for time and sector specific factors. Additionally, r is the bandwidth, and ε_{ic} is the idiosyncratic error term. Standard errors are robust and clustered at the competition-level to account for correlation in time and competition topics.¹⁹ We use polynomials that are allowed to differ on either side of the threshold, as standard practice in RD designs (Lee and Lemieux, 2010). Drawing on Gelman and Imbens (2018), we model the running variable linearly or quadratically throughout the analysis, as higher-order polynomials may often lead to sensitive and unreliable estimates. As suggested by Lee and Lemieux (2010), we run regressions with a variety of bandwidths. We use the entire sample (i.e. infinite bandwidth) and two different bandwidths of ± 10 and ± 5 centered ranks around the threshold. Using the infinite bandwidth amounts to comparing all grant-winning firms with all firms participating in a given competition. When we use the narrower bandwidths we are limiting the comparison to firms that are closer to the threshold. It is worth noting that, given the lower number of firms above the threshold relative to those below it²⁰, the use of the ± 10 bandwidth means that we are mainly discarding non-grantees and only a few grant-winning firms that rank very high in competitions where a lot of grants were

¹⁹ Results are robust to different error adjustments, including Eicker-White heteroskedasticity-robust standard errors, clustering by rank, firm, or country-by-topic.

²⁰ See Appendix Figure A1 where we plot the distribution of applicants by ranks.

awarded. When using a ± 5 bandwidth we are further restricting the number of grantees and that of non-grantees. Using narrower bandwidths also means that we compare grant-winning firms that are closer to the threshold with those that receive the SOE, that is, those that were not awarded the grant only due to budgetary constraints.²¹

The use of centered ranks around zero is motivated by the heterogeneity across competitions in terms of number of applicants and grants. However, we might be losing information contained in the un-centered raw ranks: two firms with the same centered rank participating in two competitions that award a different number of R&D grants might differ quite substantially. This could induce heterogeneous effects across competitions based on the un-centered rank of the threshold (Barrows, 2018; Howell, 2017). To address this problem we draw upon Howell (2017), who proposes to control for dummies for the firm’s rank quintile within the competition.²²

Although RD designs do not need conditioning on baseline covariates, Lee and Lemieux (2010) suggest including pre-treatment dependent variables as they are usually correlated with post-treatment outcomes and because doing so can reduce sampling variability and improve precision. Therefore, in all models we include Y_{ic}^{Pre} , which controls for the respective pre-assignment dependent variable. Finally, it is important to note that the empirical strategy allows for the estimation of local average treatment effects (LATE). These apply to the subpopulation of firms with ranks near the threshold. Hence, while the RD enables the estimation of causal effects, it does not allow to draw conclusions about the average treatment effects (ATE) induced by the policy for the whole population of applicants (Imbens and Lemieux, 2008).

3.1 Validity of the RD design

In this section we provide a number of tests to assess the validity of the RD design. First, the grant (i.e. treatment) should not cause rank. In our case, this is not problematic given that

²¹ More precisely, the ± 10 bandwidths restrict the sample on the left side of the threshold almost only to SOE firms, while the ± 5 bandwidths only to SOE firms.

²² In unreported results, we also run specifications where centered ranks are interacted with competition fixed effects on both sides of the threshold. Point estimates are overall similar though less precise.

the decision to assign the grant takes place after the ranking has been compiled based on the aggregation of four independent expert evaluations. The presence of firms with multiple grants could induce the treatment to cause rank. However, although in our data we have firms with multiple applications, there are no multiple grant winners. A potential concern has to do with those firms that have previously participated or won a proof-of-concept grant (i.e. Phase I).²³ To check whether this might be happening, we tested whether winning a Phase I grant is associated with a higher probability of winning Phase II. We found no evidence of this. Furthermore, results hold when we exclude Phase I grant-winning firms or include a dummy variable identifying this group of firms in the regressions (see Section 7).

Given that the threshold must be exogenous to rank in a valid RD design, a second concern involves the possibility of manipulation (Lee and Lemieux, 2010). In our context this could happen if experts were able to manipulate the rank around the threshold. As described in Section 2.1, evaluation is conducted remotely and the individual expert does not know the scores given by the others. As the final score mapping into a rank is an aggregation (i.e. median) of four independent scores, even if one expert had any intention to manipulate the evaluation, it will be highly unlikely that this could automatically result into a winning rank. Manipulation is made even harder by the fact that experts do not know the effective number of grants that will be awarded in the competition *ex ante* since this is ultimately a function of the agency's budgetary constraints.²⁴

Manipulation might come from applicants trying to influence ranking by submitting high-quality proposals and requesting relatively small amounts of funding in order to have higher chances to secure a grant given the budgetary constraints. If this happened, we should observe

²³ Approximately 12% of all applications come from firms that have previously won a proof-of-concept grant.

²⁴ The program's rules explicitly forbid experts to contact one another. Note that experts sign a Code of Conduct that has serious consequences if violated. Among them, they risk being removed from the experts' list. Moreover, considering that experts are paid for their work (450 euros every 4 proposals evaluated), the incentive to manipulate scores is far from obvious.

grantees systematically requesting lower budgets relative to losing firms. We found no evidence of local discontinuity in funding request (see Appendix Figure A3).²⁵

To obtain evidence against differential sorting across the threshold, we test whether firms that eventually win a grant are different in terms of their pre-assignment observables and outcome variables.²⁶ We first provide evidence of continuity around the threshold from a graphical perspective in Figure 1 (Panel A).²⁷ Additionally, we estimate models where the pre-competition firm outcome (Y_{ic}^{Pre}) or baseline covariate is regressed against $Grant_{ic}$, linear or quadratic ranks on both sides of the threshold, and competition fixed effects. We run separate regressions for each dependent variable using different bandwidths, and report the results in Table 2. Point estimates tend to be small in magnitude and not statistically significant across both baseline covariates (e.g. age, high-tech) and pre-assignment outcomes (e.g. private equity, assets, revenues). Note that to achieve balancing we do not need to restrict the bandwidth around the threshold, since there are no statistically significant differences even when considering all participating firms in a given competition (i.e. infinite bandwidth). Overall, the evidence suggests the absence of any systematic difference across treated and untreated groups.

A last potential concern for manipulation does not regard the grant but the assignment of the SOE (i.e. the ‘quality stamp’ awarded to firms that do not win the grant due to insufficient budget). Indeed, experts know *ex-ante* the minimum quality threshold score (i.e. 12 points) that would allow a given proposal to receive the SOE, which might raise the possibility of manipulation. However, this is still unlikely in light of the aggregation of four independent scores. To provide evidence on this, we re-run the above tests by re-centering the threshold so that zero lies between the last SOE-recipient firm and the first SOE-losing firm. Both graphical and statistical evidence indicate the absence of any systematic pre-competition difference around

²⁵ Note that this also provides evidence against manipulation by experts. That is, to squeeze in a given proposal, experts should make sure that the funding amount requested by the proposal fitted within the pre-established budget for that competition.

²⁶ As in each competition there are at least one winning firm and one losing firm, the number of firms at the threshold is symmetric by design and we cannot resort to the canonical McCrary (2008) test.

²⁷ See Appendix Figure A2 and A3 for additional pre-competition observables.

the SOE threshold (see Appendix Figure A4 and Table A5). In sum, while it is impossible to fully test the assumption of no sorting on observables around the threshold, all the evidence (both institutional and statistical) provides clear support for the validity of the research design.

4 Results

4.1 The effects on firm-level outcomes

In this section we examine the effects of R&D grants on a wide number of firm-level outcomes encompassing several aspects of the innovation-to-market process. Before reporting the econometric results, we show graphical evidence of discontinuity in post-grant outcome variables. Plots are reported in Figure 1 (Panel B) using a bandwidth of -20 and 10 centered ranks using quadratic polynomial regressions estimated separately on both sides of the threshold. The graphs suggest a positive discontinuity for investment, cite-weighted patents, private equity, assets, employees and revenues. Finally, a negative discontinuity is present for firm failure (see Appendix Figure A2).

The R&D subsidy evaluation literature has traditionally focused on the effects on subsequent private R&D spending to test for the presence of ‘crowding-out’ or ‘crowding-in’ effects. Unfortunately, our data do not contain information on R&D expenditures, which prevents us from directly testing whether grants increase firm-financed R&D. Hence, as in Bronzini and Iachini (2014), we examine the effects of direct public R&D funding on firm investment. Firm investment is defined as the annual variation in fixed assets net of depreciation. We cumulate firm investment at time t (i.e. the competition year) and $t + 1$ and scale it by total assets. To prevent potential endogeneity concerns we use the pre-grant total assets.

Results are shown in Table 3. Columns 1 to 3 contain OLS specifications using infinite bandwidths (i.e. all firms) whereas columns 4 to 7 use bandwidths of ± 10 and ± 5 centered ranks (i.e. firms close to the threshold). We use both linear and quadratic interpolations of the running variable separately on both sides of the threshold. In order to select the most appropriate

polynomial order within a given bandwidth (e.g. columns 4-5 both using a ± 10 bandwidth), we report the Bayesian Information Criterion (BIC) and choose the model with the minimum value as the preferred specification. We include in all regressions the pre-assignment dependent variable and competition fixed effects.

Grants trigger a positive and statistically significant increase in firm investment. Considering an average investment of 0.25, the point estimates selected by the BIC²⁸ (ranging from 0.44 to 0.68) imply a sizable effect of the policy.²⁹ A further test concerns the impact of R&D grants on investment in tangible as opposed to intangible assets.³⁰ The opaqueness and information asymmetries characterizing intangibles make their financing more problematic leading to financial constraints (Hall and Lerner, 2010; Bronzini and Iachini, 2014). To examine whether R&D grants are especially beneficial to this kind of investment, we run separate regressions for tangible and intangible investment using Equation (1). Results show that the effects on intangible investment appear to be systematically larger if compared with the effects on tangible investment (see Appendix Tables B2 and B3).

We then report estimation results on the causal impact of the SME Instrument on subsequent innovation and external finance. To assess the effects on innovation outcomes, we employ patent data, which are one of the most common proxies to capture firms' innovative behavior. We use a quality-adjusted patenting measure that is obtained by weighting patents with their subsequent citations. We run Equation (1) using as dependent variable the log of cite-weighted patents plus one after the competition. To be conservative, the dependent variable considers all cite-weighted

²⁸ For certain outcomes (e.g. investment and asset growth) the use of a quadratic polynomial, combined with the smallest bandwidth (i.e. ± 5), shows signs of over-fitting (see column 7). This is not surprising since restricting the bandwidths around the threshold will make the relationship between the running variable and the outcomes closer to being linear (Lee and Lemieux, 2010). Indeed, in these cases, the BIC favors the specifications adopting a linear adjustment of the running variable (column 6).

²⁹ Note that these models might only partially capture the full effect of the grants given that these might last up until two years. Therefore, we also run the same regression using as dependent variable the cumulated investment including $t + 2$ scaled by pre-assignment total assets. Estimations are based only on firms applying during 2014, 2015 and 2016 since they feature enough post-treatment observations. Results indicate substantially larger treatment effects (Table B1).

³⁰ Tangibles assets have a physical value (e.g. machinery and equipment), whereas intangibles are assets lacking physical substance. Examples are research and development, software, licences, intellectual property and trademarks. Intangible assets are more difficult to measure compared to tangibles, and are likely to be under-reported on balance sheets.

patents starting from $t + 1$ (i.e. the year after the competition) and not t (i.e. the year of the competition) since this could lead to overestimate treatment effects by considering innovation outcomes that are unlikely to stem from the grant. Results are reported in Table 4 (Panel A) and show that grants increase log cite-weighted patents across all specifications. Point estimates indicate an increase within the range of 15 to 31% depending on the bandwidth employed.³¹ Models using the simple (log) number of patents yield similar results (see Appendix Table B6).

The reported increase in cite-weighted patents could be ascribed to firms that would not have filed any patent application without the grant (i.e. extensive margin) and/or to firms that would have filed patent applications but in smaller numbers absent the grant (i.e. intensive margin). To test for the presence of extensive margin effects, we estimate our baseline models using a dummy variable for patent applications. Estimates show that the policy increases by 8-15 percentage points the probability to apply for a patent. Relative to an 8% mean, this effect translates into an over 100% increase (Appendix Table B7). We estimate the same model by splitting the sample according to pre-competition patenting activity. While firms with patents before the grant experience larger treatment effects, these are not statistically different from those of non-patent active firms (Table B8). This indicates that the policy operates through both intensive and extensive margins. In other words, R&D grants benefit firms that have engaged in innovation activities in the past, but also increase the probability of first-time patenting. The latter effect is particularly important because it indicates behavioral change of great significance for the future growth prospect of the firm.

Next, we examine the effects of R&D grants on follow-on external finance. One of the intended outcomes of the SME Instrument is the reduction of information asymmetries between potential external investors and innovative firms. Receiving R&D grants should diminish the risk perceived by potential investors, who in turn may have greater propensity to invest. Testing whether R&D grants enhance the prospect of further external financing also indicates whether grant-

³¹ In the Appendix we report extensive robustness tests which corroborate these findings (see Appendix Table B4).

winning firms represent privately profitable opportunities and constitute a measure of early-stage entrepreneurial success (Howell, 2017). We start by estimating Equation (1) where the dependent variable is a dummy indicating whether or not a firm has received private equity investment following the competition (as of March 2019). Grant-winning firms are more likely to receive private equity (Panel B, Table 4). More precisely, estimates indicate that winning the grant increases the probability of receiving external equity by about 7 and 11.7 percentage points, relative to a 4% mean. Hence, the receipt of R&D grants triggers up to a threefold increase in the likelihood of receiving follow-on equity investments.

To assess whether R&D subsidies help companies to raise more funding amount and more founding rounds, we also estimate the models using the log of equity received plus one and the log of the number of equity deals plus one, respectively. We find that the SME Instrument triggers a sizable increase between 46 and 97% in the amount of private equity (Table B9) and around 8-17% increase in the number of deals (Table B10).³²

We then test whether grants increase firm growth in terms total assets, employees and revenues. The models feature the log transformed outcome at $t + 1$ (i.e. one year after the competition) as dependent variable, while controlling for the log transformed outcome at $t - 1$ (i.e. the year before the competition). Results reported in Table 5 indicate an increase in assets growth between 48 and 56% (Panel A) whereas the effect on employment growth is within the 24 to 33% range (Panel B). Positive (albeit noisy) effects are documented also in the case of firm revenues with an approximate 28-48% increase (Panel C).³³

The overall improvement in firm outcomes might also be accompanied by a reduction in failure chances, which tend to be particularly high for innovative new ventures (Hyytinen et

³² In principle, the positive effects on equity could be materializing via negative spillovers. That is, the grant increases the probability of receiving private finance by reducing that of losing firms. Following Howell (2017), we exploit the fact that equity funds tend to invest close to their location and test whether the effects of the grant change for firms in the same NUTS3 region. We do not detect any statistically significant difference.

³³ As for investment, the short post-treatment period arguably leads to underestimate the effects on firm growth. Treatment effects over time seems to be consistent with this observation as point estimates for revenues (and the other growth measures) tend to increase at $t + 2$ (see Appendix Figure B2).

al., 2015). We therefore examine whether grants decreases firm failure, namely, exit through bankruptcy, dissolution, liquidation or insolvency by 2019. Results show a decrease in the likelihood of failure that is around 4 to 7 percentage points (Appendix Table B11). This represents a substantial impact in economic terms since the mean of the dependent variable is 6%.³⁴

4.2 Heterogeneous effects

A large literature has documented that financial constraints are especially problematic for innovative firms (for a survey, see Hall and Lerner (2010)). This is one of the reasons why governments subsidize R&D, that is, to help cash-constrained firms engaging in innovation projects they would otherwise be unable to pursue. If the effect of R&D grants on firm performance takes place by reducing market failures, the additionality is socially more desirable. In this section we explore whether the policy alleviates financial frictions using proxies at different aggregation levels (i.e. firm, sector, country). The unusual variety in our data in terms of both sectors and countries of origin of applicants allows us to explore interesting heterogeneous effects which are new to this literature.

First, we investigate whether the effect of the SME Instrument varies according to the most commonly used proxies for financial vulnerability, namely, firm age and firm size (Hadlock and Pierce, 2010).³⁵ To that end, we estimate a variant of Equation (1) where we insert a dummy variable for above-median age or firm assets (as a proxy for firm size) and interact it with the treatment variable. The coefficient on the interaction between the treatment variable and the dummy variables captures the differential effect of R&D grants on firm-level outcomes for older

³⁴ The result is also desirable from a policy perspective because the positive impact of the scheme on other firm outcomes could in theory be counterbalanced by decreased or unchanged survival chances among awarded firms, which might indicate a dispersion of public resources. Conversely, we do not find that grants increase the likelihood of an IPO or M&A (see Appendix Tables B12 and B13). This is not surprising given the very low number of successful exits observed after 2014.

³⁵ Small firms suffer from information asymmetries, often lack sufficient collateral and feature more volatile revenues since they are less diversified. Young firms are considered to be even more financially vulnerable because of their lower cash-flow, weaker reputation and higher likelihood of bankruptcy. These aspects make them more dependent from external finance but less able to secure it relative to larger and older businesses, especially if they engage in innovation activities (Brown et al., 2009; Hall and Lerner, 2010).

(larger) firms, relative to younger (smaller) firms. Results in Appendix Table C1 indicate that older or larger firms systematically experience treatment effects of lower magnitude if compared with younger or smaller firms. This suggests that R&D subsidies trigger a stronger impact on firms that are much more likely to suffer from financial constraints. Results are consistent when using sectoral-level measures of financial frictions, i.e., asset tangibility and liquidity (see Appendix Table C2).

Moreover, we investigate potential heterogeneous effects across countries. In particular, we examine whether the impact of R&D grant varies according to the economic development of the recipients' country. We use GDP per-capita and divide countries in two groups using the corresponding median value.³⁶ Estimates reported in Appendix Table C3 indicate that the effects of R&D grants generally decline as economic development increases.³⁷ We also investigate heterogeneous responses depending on countries' financial development. We find that firms in countries with lower credit availability tend to reap larger benefits from R&D grants (see Appendix C for more details).

We further explore differential effects of R&D grants across levels of economic development from a more disaggregated perspective. In more detail, we test for potential heterogeneous effects across European regions (NUTS2) depending on their GDP per capita to understand whether grants spur larger effects in more disadvantaged regions. Results in Appendix Table C4 show that being located in a more advanced region does not lead to a statistically different effect in terms of patenting and equity. For the remaining outcomes, we observe that firms in relatively poorer regions enjoy larger effects. These findings suggest that the effects of R&D grants are generally more beneficial for firms operating in regions that lag behind economically.

³⁶ We use GDP per-capita in constant 2010 US dollars for 2013 from the World Bank Development Indicators.

³⁷ Note that firms, especially if they are not awarded a grant, may rely on alternative subsidies offered by their respective national governments. Therefore, applicants from more developed countries may be able to substitute the grant with other public funds. The interaction between the SME Instrument and other R&D (or related) funding schemes is potentially relevant. The lack of harmonized application data concerning supra-national, national and sub-national funding schemes supporting innovative SMEs prevents us from exploring this issue.

5 Certification versus funding

The positive impact of R&D grants could be materializing through different channels. In principle, one can think about two main mechanisms, that is, funding or certification (Lerner, 2000; Howell, 2017). Funding refers to the possibility that the grant’s money allows firms to successfully develop a technology, thus mitigating information asymmetry and investors’ uncertainty. Certification refers instead to the possibility that the grant provides a positive signal about firm (or project) quality to the market (Takalo and Tanayama, 2010) which decreases information asymmetries towards external investors. In order to shed light on the above mechanisms, we run a number of tests.

We start by providing direct evidence on pure certification effects. To this end we leverage a unique institutional feature of the SME Instrument. As described in Section 2.1, those firms that deserve funding according to experts’ evaluation, but do not obtain it only due to budget constraints, receive the so-called “Seal of Excellence” (SOE). This is a certificate designed to signal firm quality to other external investors (both public and private) that could provide alternative funding opportunities. For this purpose, the information on which firms receive the SOE is publicly announced by EASME after each competition. Therefore, we leverage the assignment of the SOE to test whether pure certification effects, not attached to funding, trigger any increase in firm performance.

First, we re-run our models using only firms that received the SOE and the rest of unsuccessful firms. This test is based on the idea that, if pure certification is at work, this would imply the presence of statistically significant differences in post-grant outcomes between the recipients of the SOE and all the other firms that win neither the grant nor the SOE. We run a variant of Equation (1) in which the treatment variable is the SOE itself and the re-centered threshold lies between the last SOE-certified firm and the first SOE-losing firm. Results reported in Table 6 document the absence of statistically significant differences for all firm-level outcomes (the only exception is revenues, although this is not confirmed when we vary the bandwidth).

Second, we re-run Equation (1) limiting the sample to grant-winning firms and SOE-certified firms. In this way, we are testing whether pure certification effects match the effects due to the grant (which plausibly embodies both funding and certification effects). In presence of strong (pure) certification effects, one may plausibly expect differences between grant-winning firms and SOE-certified firms to be smaller compared with baseline estimates. Although in some cases point estimates tend to be slightly smaller, all results tend to be strongly confirmed, thus indicating that pure certification effects are weaker than the grant (Appendix Table D1).³⁸

Overall the above tests document that pure certification effects, not attached to funding, do not have any impact. Yet, pure certification effects stemming from the SOE may be different from the certification effects embodied in the grant, which might convey a stronger signal to the market. Therefore, we provide evidence on which of the two channels embedded in the grant prevails. Note that, while the SOE allows us to test directly for pure certification effects, we can only provide indirect evidence on the certification effects that are intrinsically associated with funding as fully disentangling the two mechanisms is challenging. We report and discuss in detail these results in Appendix D. For instance, we show that the increase in patenting after the grant is mainly driven by those firms not receiving private equity, which is consistent with funding being the main channel. In line with this, we find that the positive effects of the grant on patenting emerge before the ones on private equity. We also exploit variation in R&D grant size and show that firms obtaining larger amounts systematically drive the overall results, thus providing additional support for funding as primary mechanism (Lerner, 2000). Finally, we find no indication that grants certify winning firms towards banks (Meuleman and De Maeseneire, 2012), as we do not detect an increase in the amount of debt nor a re-balancing towards long-

³⁸ One potential explanation for the absence of pure certification effect is the possibility that the effect is present only for first-time recipients of the SOE. The intuition is that certification is beneficial at first but repeated certifications are redundant and might even be detrimental to firm performance (Lanahan and Armanios, 2018). Results for the tests discussed above could also be influenced by a small share of firms that receive the SOE multiple times. In unreported results, we find no clear-cut evidence of pure certification when we include only first-time SOE firms. One additional possibility behind the null effects of the SOE is that it is just not salient in the market, and therefore private investors do not respond to the signal because they are unaware of it. We discuss this aspect in Appendix E suggesting that this is unlikely.

term debt. In sum, all of the above tests do not provide any evidence, direct or indirect, in favor of certification, and point instead quite clearly to the funding channel as the primary mechanism behind the results.

6 Value-for-money and spillovers

In this section we perform some back-of-the-envelope calculations to gauge the value-for-money of the program. While our main results indicate that grants have a positive and sizable causal effect on several firm-level outcomes, they are not necessarily indicative of whether grants are a high-value use of public funds. To assess the returns on grants, we begin by drawing upon Clancy (2021) who proposes a simple yet effective way to measure their value-for-money. The main problem when computing the returns on grants is that it requires assigning a monetary value to patents, which is far from easy (Azoulay et al., 2019).³⁹ As a useful alternative heuristic, Clancy suggests to compute the number of patents a program generates per R&D dollar. Based on our results, the SME Instrument spawns approximately 0.76 patents per million euro of R&D.⁴⁰ Note that this figure is rather close to the one attributable to the US DoE SBIR, which lies within the range of 0.88-0.96 based on the estimates of Howell (2017) and Myers and Lanahan (2022).⁴¹ In sum, while these simple calculations should be interpreted with caution, they do suggest that the SME Instrument features a value-for-money similar to that of the SBIR.⁴²

³⁹ In our setting, this is further complicated as our sample spans all sectors, and includes a pool of highly heterogeneous patents.

⁴⁰ To come up with this figure we take the point estimates on raw patent counts obtained with the narrowest bandwidth of ± 5 (see columns 6 and 7 in Table B6) and select the one for which the BIC is minimized (i.e. 0.307). Considering an average value of 4.03 patents and a mean grant amount of €1.6 million, the average marginal cost per patent would be between €1.3 million ($= 1/((4.03 \times 0.307)/€1,638,000)$) which translates into 0.76 patents per million euro of R&D.

⁴¹ Myers and Lanahan (2022) report that the average marginal cost of a patent is roughly \$1.3 million (€1.13 million) according to their estimates or \$1.2 million (€1.04 million) according to Howell (2017). These translate into 0.88-0.96 patents per million euro of R&D.

⁴² However, these might be not enough to conclude that allocating public resources through R&D grants is a high-value use of public funds. One way to benchmark the returns to both programs is to determine whether they outperform the private sector in turning R&D into patents (assuming that the private sector is able to achieve good returns from their investments in innovation). Appendix F provides an illustrative comparison using the private sector's patent-to-R&D ratio in both the US and EU-28. These suggest that returns from these programs are in line, if not better, with those characterizing the private sector.

As mentioned above, an alternative approach to provide evidence on the return to the grant involves assigning a monetary value to the benefits that stem from it. To do so, we consider the monetary value generated by the grant in terms of three firm-level outputs: patenting, private equity, and revenues.⁴³ To assign a value to patents, we rely on Bessen (2009), who estimates that, on average, a patent is valued by the stock market around \$798,000 in 1992 \$US (or 1.2 million in 2015 €).⁴⁴ This implies that, with a sample mean of 4.03 and assuming a 30.7% effect (Table B6, column 6), a grant produces around 1.2 patents or €1.5 million in firm market value. Combined with the returns in terms of private equity and revenues⁴⁵, the average grant of €1.6 million produces a private return of around €2 million. While this back-of-the-envelope calculation does suggest a positive rate of return on grants, we follow Azoulay et al. (2019) and abstain from reporting an exact figure given the illustrative nature of the exercise.

It is worth stressing that these returns do not comprise the social benefits stemming from R&D grants that recent literature found to be more than four times the private ones (Myers and Lanahan, 2022). Given the short post-treatment period characterizing our empirical setting, we refrain from providing evidence along the lines of prior studies (e.g. Bloom et al., 2013). While we leave a more comprehensive account of this matter to future research, we can nevertheless produce some evidence on spillovers by focusing on whether grants awarded to firms in a given geographical-technological area foster the creation of similar firms in that location. A shock to the local pool of R&D might create knowledge spillovers and strengthen agglomeration economies, thus increasing the incentives for similar new firms to locate in that same area. To test for this,

⁴³ Note that we do not consider other outcomes such as employment creation and survival which would increase the benefits of the grant. Also, it is worth stressing that the effects of the grant are estimated using a short post-treatment period and, as a result, they might not fully capture their impact leading to underestimation.

⁴⁴ To be conservative, we avoid using the recent estimates of the monetary value of patents reported by Kogan et al. (2017) (\$3.2 million in 1982 dollars); these would imply a considerably higher rate of return.

⁴⁵ We compute them using the point estimates obtained with the narrow bandwidth of ± 5 and selecting the one that minimizes the BIC for revenues (Table 5, Panel C, column 6) and private equity amount (Table B9, column 6). Considering these point estimates (i.e. 0.190 and 0.731) and a sample mean of €2.9 million revenues and €170,000 private equity, the estimated value is 559,360 and €124,270, respectively.

we use Eurostat data on firm entry rates at the NUTS3 region and 1-digit NACE rev.2 sector level.⁴⁶ We link these data to the firms in our sample based on their geographical location and sector of activity. We then estimate a variant of our baseline RD equation to test whether a grant awarded to a firm located in a specific region \times sector increases the rate of entry in that same geographical-technological area. Results reported in Table 7 indicate that, before the assignment of the grant, entry rates are similar across the threshold (columns 1 to 4). That is, firms that eventually win the grant are located in a region \times sector featuring similar entry dynamics. After the assignment, grants increase the growth in entry rates by approximately 4-6% (columns 5 and 6).⁴⁷ Conversely, no effect is found when considering SOE firms (see Appendix Table D7). Overall, these findings suggest that the program makes an important contribution not just by improving recipients’ performance but also by indirectly fostering entrepreneurship.

7 Robustness

To check the sensitivity of our results we conduct a number of robustness and falsification tests, described in greater detail in Appendices G, H, I, J, K, L, M. Appendix G shows that our findings are confirmed when adopting alternative fixed effects structures as well as different standard error adjustments. We show that our results hold when adopting alternative criteria to select the bandwidths around the threshold (Appendix H), local polynomial models with triangular kernel (Appendix I), a local randomization approach (Appendix J), and using RD difference-in-differences (Appendix K). In Appendix L we perform falsification tests with placebo

⁴⁶ Data are available up until 2018 and for 15 out of 40 countries in our sample as this includes 15 that are not part of the EU-28. Eurostat coverage is also limited by the fact that regional business demography statistics are voluntary and not all EU members report them.

⁴⁷ These results are in line with prior studies suggesting a positive association between SBIR grants and new firm formation in the US (Audretsch et al., 2002; Qian and Haynes, 2014). While we do not attempt to isolate the prevailing mechanism behind our results, empirical evidence suggests several possible channels, e.g.: i) Audretsch et al. (2002) show that SBIR grants have a “demonstration effect”, inducing potential entrepreneurs to start a business; ii) Wallsten (2001) argues that one of the reasons SBIR awardees are strongly spatially concentrated is that similar firms locate close together (e.g. firms may work on complementary technologies and thus choose to co-locate); iii) Babina and Howell (2018) show that corporate R&D investments lead to an increase in employee departures to entrepreneurship.

thresholds. Finally, Appendix [M](#) provides evidence in favor of the stability and external validity of the results.

8 Conclusions

In this study we exploit confidential data on the applicants to the SME Instrument, a large-scale European R&D grants program modeled after the US SBIR program. We leverage the discontinuity in the assignment mechanism to adopt a sharp RD design, thus providing the broadest quasi-experimental evidence on the effects of public R&D support across sectors and countries. Our results indicate that R&D grants to small and young innovative firms have large and positive effects on cite-weighted patents, investment, firm growth, the probability of receiving external equity and on firm survival. Heterogeneous effects indicate that R&D grants alleviate financial constraints that typically hamper innovation. The mechanism behind the positive results appears to be funding, rather than certification, because it makes possible for firms to pursue technology development, decrease technical and market uncertainty, and increase the likelihood of further external investments. By leveraging the assignment of a ‘quality stamp’ to firms that are not awarded the grant, we also provide evidence that pure certification effects, not attached to funding, are not conducive to any improvement in firm-level performance. Back-of-envelope calculations suggest that the rate of return on grants is positive and that the program is able to generate a number of patent per R&D euro that is rather similar to that of the US SBIR. Finally, we provide evidence on spillovers by showing that grants awarded to firms in a given geographical-technological area foster the subsequent creation of similar firms in that location.

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Table 1: Descriptive statistics on SME Instrument competitions and applicants

Panel A: competitions (raw data)				
	Mean	SD	p50	N
# competitions				176
# applicants per competition	84.68	74.35	68	14904
# winning applicants per competition	4.09	3.08	3	719
Panel B: competitions (cleaned data)				
	Mean	SD	p50	N
# competitions				176
# applicants per competition	63.04	56.97	50	11095
# winning applicants per competition	2.66	2.17	2	468
Panel C: applicants characteristics				
	Mean	SD	p50	N
Patents ^{Pre}	4.03	8.13	0	11095
Citw patents ^{Pre}	30.84	84.70	0	11095
Private Equity ^{Pre} (d)	0.04	0.18	0	8352
Private Equity ^{Pre} (1,000 €)	170	1940	0	8352
Revenues ^{Pre} (1,000 €)	2944	7832	554	6238
Employees ^{Pre}	19.40	29.96	8	6700
Assets ^{Pre} (1,000 €)	2932	5337	994	8411
Age ^{Pre}	8.83	11.62	5	11313
High-Tech (d)	0.57	0.50	1	11024
Failure (d)	0.06	0.24	0	11402
IPO (d)	0.00	0.05	0	8432

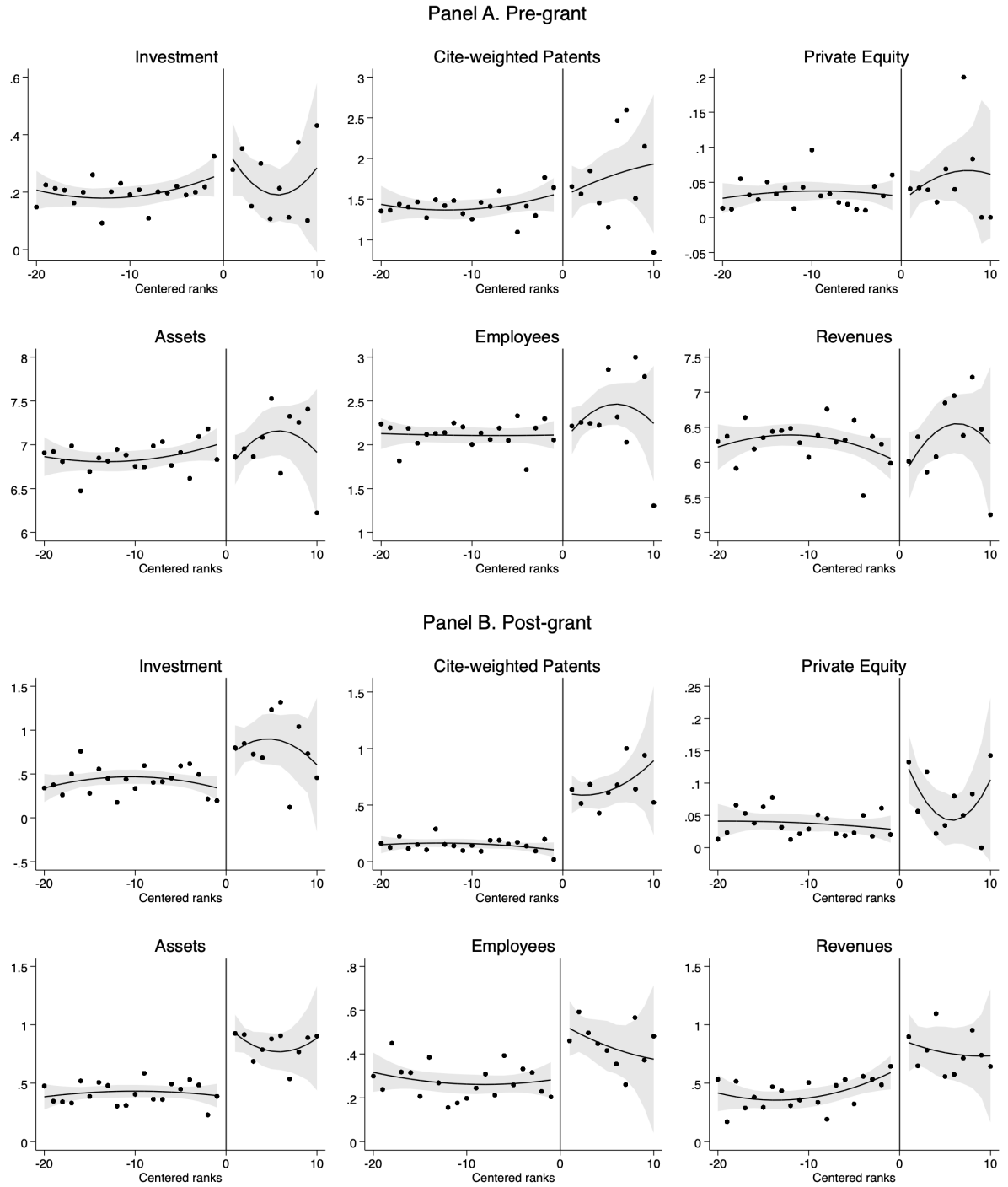
Notes: summary statistics for competitions and applicants participating to the SME Instrument during 2014-2017. Panel A (Panel B) reports summary statistics at the competition-level for the original sample (estimation sample). In Panel A and B, the last column (“Count”) reports the total number of competitions, applicants, and winning applicants contained in the two samples. The remaining columns in these panels report the mean, standard deviation and median of the number of (winning) applicants per competition. Panel C presents summary statistics at the firm-level across a number of observables. Balance-sheet variables are reported in thousand euros. These are winsorized at 2% level on both sides of the distribution while patent count and cite-weighted patents are winsorized at the 98% level.

Table 2: Balancing tests of baseline observables and pre-grant outcomes

	1 st order polynomial			2 nd order polynomial		
	All	±10	±5	All	±10	±5
Citw patents ^{Pre}	0.127 (0.149)	-0.121 (0.196)	-0.115 (0.278)	0.270 (0.220)	-0.178 (0.326)	-0.124 (0.603)
Private Equity ^{Pre}	-0.028 (0.023)	0.005 (0.024)	-0.028 (0.030)	0.017 (0.031)	-0.070 (0.037)	-0.005 (0.076)
Revenues ^{Pre}	-0.439 (0.254)	-0.077 (0.304)	-0.445 (0.429)	-0.655 (0.375)	-0.103 (0.498)	0.400 (0.896)
Assets ^{Pre}	-0.047 (0.156)	-0.189 (0.180)	-0.515** (0.251)	-0.096 (0.205)	-0.315 (0.282)	0.111 (0.563)
Employees ^{Pre}	0.007 (0.131)	0.014 (0.168)	-0.111 (0.223)	-0.000 (0.193)	-0.031 (0.253)	0.166 (0.472)
Age ^{Pre}	-0.067 (0.074)	-0.078 (0.097)	-0.159 (0.140)	-0.088 (0.107)	-0.108 (0.163)	0.294 (0.260)
Cash-flow ^{Pre}	0.017 (0.030)	0.069 (0.040)	0.037 (0.069)	0.001 (0.043)	0.044 (0.077)	0.105 (0.135)
Profit margin ^{Pre}	5.286 (3.460)	5.542 (4.648)	6.276 (7.933)	-1.472 (5.913)	1.397 (8.716)	10.164 (16.006)
High-tech	-0.056 (0.039)	-0.055 (0.050)	-0.025 (0.067)	-0.058 (0.057)	-0.031 (0.081)	-0.161 (0.153)
GDP per capita	-0.024 (0.038)	-0.011 (0.056)	0.075 (0.076)	0.004 (0.060)	0.059 (0.090)	0.107 (0.131)
VC Hub	-0.027 (0.038)	-0.015 (0.048)	0.036 (0.065)	-0.008 (0.054)	0.043 (0.076)	0.127 (0.136)

Notes: results obtained estimating our baseline RDD equation by means of OLS with pre-determined observables as dependent variables: $Y_{ic}^{Pre} = \alpha + \beta Grant_{ic} + f(Rank_{ic}) + \delta_c + \varepsilon_{ic}$. Estimates are obtained using different bandwidths around the threshold (i.e. an infinite one, ±10 or ±5 centered ranks). All regressions include linear or quadratic polynomials of the running variable on both sides of the threshold and competition fixed effects. VC Hub is a dummy variable taking 1 if the firm is located in one of the top 15 EU NUTS2 regions with the highest concentration of VC investors according to Colombo et al., 2019, and 0 otherwise. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Fig. 1: RDD plots before and after the grant



Notes: RD plots for firms with centered ranks between -20 and 10. The left plot refers to the pre-assignment period, whereas the right plot to the post-grant period. Circles represent rank-level means of the firm-level outcomes. Fitted lines from local polynomial regressions with a quadratic fit together with 95% confidence intervals.

Table 3: The effects on investment

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.437*** (0.129)	0.369* (0.211)	0.388*** (0.090)	0.481*** (0.169)	0.481* (0.274)	0.677*** (0.224)	1.595*** (0.524)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	6873	6873	6873	1241	1241	698	698
R-squared	0.05	0.05	0.05	0.20	0.20	0.26	0.27
BIC	20231.97	20241.51	20242.34	3760.74	3770.39	2116.04	2122.26

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the cumulated investments during time t and $t + 1$ scaled by total assets at $t - 1$ winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the threshold. In order to select the most appropriate polynomial order within a given bandwidth (e.g. columns 4-5 both using a ± 10 bandwidth), we report the BIC and choose the model with the minimum value as the preferred specification. All regressions include the pre-grant dependent variable (log of fixed assets at time $t - 1$) and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The effects on cite-weighted patents and external equity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Citw-patents	All	All	All	± 10	± 10	± 5	± 5
Grant	0.203*** (0.068)	0.282** (0.117)	0.148*** (0.051)	0.147* (0.085)	0.236* (0.138)	0.314*** (0.113)	0.390* (0.230)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	11095	11095	11095	1822	1822	1050	1050
R-squared	0.36	0.36	0.36	0.45	0.45	0.51	0.51
BIC	23502.73	23516.83	23509.32	4221.02	4234.39	2318.97	2332.66
Panel B: Private Equity	All	All	All	± 10	± 10	± 5	± 5
Grant	0.070** (0.028)	0.126*** (0.045)	0.036** (0.015)	0.080*** (0.027)	0.123*** (0.047)	0.117*** (0.039)	0.157* (0.085)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	8352	8352	8352	1358	1358	784	784
R-squared	0.07	0.07	0.07	0.17	0.17	0.27	0.27
BIC	-5077.46	-5071.33	-5058.36	-600.21	-588.55	-337.13	-324.29

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable in Panel A is the log of cite-weighted patents applications plus one filed starting from the year after the competition. In Panel B is a dummy variable indicating whether a firm has received private equity financing after the competition (as of March 2019). Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the threshold. In order to select the most appropriate polynomial order within a given bandwidth (e.g. columns 4-5 both using a ± 10 bandwidth), we report the BIC and choose the model with the minimum value as the preferred specification. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: The effects on firm growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Assets	All	All	All	± 10	± 10	± 5	± 5
Grant	0.561*** (0.065)	0.578*** (0.099)	0.437*** (0.050)	0.477*** (0.095)	0.570*** (0.150)	0.545*** (0.138)	1.037*** (0.321)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	7306	7306	7306	1311	1311	743	743
R-squared	0.77	0.77	0.77	0.74	0.74	0.74	0.74
BIC	17860.70	17875.35	17862.13	2990.32	3002.53	1682.91	1691.63
Panel B: Employees	All	All	All	± 10	± 10	± 5	± 5
Grant	0.330*** (0.062)	0.256*** (0.092)	0.219*** (0.038)	0.283*** (0.081)	0.318** (0.132)	0.242** (0.120)	0.234 (0.222)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	5493	5493	5493	962	962	548	548
R-squared	0.79	0.79	0.79	0.80	0.80	0.83	0.83
BIC	9093.99	9109.37	9108.84	1472.89	1485.45	730.64	743.24
Panel C: Revenues	All	All	All	± 10	± 10	± 5	± 5
Grant	0.489*** (0.136)	0.664*** (0.199)	0.370*** (0.083)	0.283* (0.146)	0.199 (0.230)	0.190 (0.206)	0.152 (0.439)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	5119	5119	5119	867	867	480	480
R-squared	0.77	0.77	0.77	0.78	0.78	0.80	0.80
BIC	13957.11	13968.98	13964.44	2262.67	2274.97	1198.20	1210.52

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the log of e.g. assets at time $t + 1$ (i.e. the year after the competition). Variables are winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the threshold. In order to select the most appropriate polynomial order within a given bandwidth (e.g. columns 4-5 both using a ± 10 bandwidth), we report the BIC and choose the model with the minimum value as the preferred specification. All regressions include the pre-grant dependent variable (log of e.g. assets at time $t - 1$) and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: SOE recipient firms vs rest of losing firms

	Citw Patents ^{Post}		Private Equity ^{Post}		Assets ^{Post}	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	± 10	All	± 10	All	± 10
Seal	0.025 (0.023)	0.037 (0.066)	-0.001 (0.007)	-0.006 (0.016)	0.030 (0.030)	-0.003 (0.094)
N	10528	2386	7768	1766	6892	1636
R-squared	0.35	0.39	0.07	0.11	0.76	0.80
	Employees ^{Post}		Revenues ^{Post}		Failure ^{Post}	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	± 10	All	± 10	All	± 10
Seal	0.018 (0.022)	0.072 (0.073)	0.076** (0.035)	0.052 (0.120)	-0.011* (0.007)	-0.030 (0.019)
N	5191	1255	4844	1138	10819	2460
R-squared	0.79	0.81	0.77	0.81	0.03	0.09

Notes: results obtained using different specifications of equation (1) by means of OLS. The treatment variable (Seal) is a dummy variable indicating whether a firm has received the Seal of Excellence. Ranks are re-centered so that 0 lies between the last SOE-winning firms and the first SOE-losing firm. All regressions include the pre-grant dependent variable, linear controls for ranks on both sides of the threshold, and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Spillovers on entrepreneurship

	Entry ^{Pre}		Δ Entry ^{Pre}		Δ Entry ^{Post}	
	(1) All	(2) ± 10	(3) All	(4) ± 10	(5) All	(6) ± 10
Grant	-0.013 (0.009)	-0.005 (0.012)	-0.004 (0.021)	-0.000 (0.032)	0.037** (0.017)	0.060** (0.024)
Rank \times Grant	Yes	Yes	Yes	Yes	Yes	Yes
N	4380	569	4380	569	4380	569
R-squared	0.98	0.98	0.51	0.48	0.50	0.59
BIC	-10696.00	-1457.82	-7418.47	-882.84	-6559.15	-949.64

Notes: results obtained using different specifications of equation (1) by means of OLS. Columns 1-4 report balancing tests on pre-assignment entry rates (i.e. number of newly-born enterprises as a proportion of the total number of active enterprises) at the region-sector level. The dependent variable in columns 1-2 (Entry^{Pre}) is the log of entry rates at $t - 1$; in columns 3-4 (Δ Entry^{Pre}) is the log difference in entry rates between $t - 3$ and $t - 1$. Columns 5-6 report the treatment effects of grants on subsequent growth of entry at the region-sector level. The dependent variable (Δ Entry^{Post}) is the log difference in entry rates between $t - 1$ and $t + 1$. All regressions include year and NUTS3 \times NACE fixed effects. Standard errors are robust and clustered at the NUTS3 and NACE level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendices

This Online Appendix accompanies the paper “*The Causal Effects of R&D grants: Evidence from a Regression Discontinuity*”, by Pietro Santoleri, Andrea Mina, Alberto Di Minin and Irene Martelli. It provides the following additional information:

1. Section [A](#) provides further descriptive statistics on applicants and competitions as well as balancing tests;
2. Section [B](#) contains additional results on firm-level outcomes;
3. Section [C](#) reports heterogeneous effects of the program;
4. Section [D](#) provides further results on the causal mechanism behind the effects (i.e. certification vs funding);
5. Section [E](#) discusses whether the Seal of Excellence can be considered as a salient signal;
6. Section [F](#), in order to benchmark the performance of the program, provides an illustrative comparison between the patents per R&D euro generated by the program with the patents per R&D euro generated by the private sector;
7. Section [G](#) contains estimation results using an alternative set of fixed effects and standard error adjustment;
8. Section [H](#) shows that results hold when using alternative bandwidths around the threshold;
9. Section [I](#) and Section [J](#) show that results hold when using a non-parametric local polynomial estimation;
10. Section [J](#) show that results hold when using a local randomization inference approach;
11. Section [K](#) presents results from a combination of RDD and DID;
12. Section [L](#) presents falsification tests;
13. Section [M](#) examines estimates stability and external validity.

A Additional descriptive statistics

In this section we report additional descriptive statistics. Table A1 presents summary statistics at the competition-level across the original sample and the final sample. Table A2 reports summary statistics for the different competition topics across the original sample and the final sample. Table A3 shows statistics for the original sample and the final sample and reports standardized differences across the two. Table A4 displays the distribution of applicants across countries. Figure A1 plots the distribution of applicants by centered ranks around the threshold. Figure A2 provides graphical evidence of continuity across the threshold in pre-competition firm age, and discontinuity in post-competition failure likelihood. Figure A3 provides graphical evidence of continuity across the threshold for a larger number of pre-competition observables. Figure A4 and Table A5 reports graphical and statistical evidence of continuity across the SOE threshold (i.e. firms that marginally win the SOE and those that marginally lose the SOE).

Table A1: Descriptive statistics on competitions

	Raw sample					
	Mean	SD	p25	Median	p75	N
mean # firms	84.68	74.35	28	68	112	176
mean # grant-winning firms	4.09	3.08	2	3	6	176
% grant-winning firms	0.07	0.07	0	0	0	176
% first applicants	0.59	0.20	0	1	1	176
% firms in consortium	0.18	0.09	0	0	0	176
% firms that won Phase I	0.12	0.08	0	0	0	176
% Seal of Excellence firms	0.36	0.16	0	0	0	176
mean requested grant amount	1591.51	477.07	1406	1478	1575	176
mean grant amount	1639.11	607.62	1284	1527	1847	176
	Matched sample					
	Mean	SD	p25	Median	p75	N
mean # firms	63.04	56.97	20	50	86	176
mean # grant-winning firms	2.66	2.17	1	2	4	176
% grant-winning firms	0.06	0.07	0	0	0	176
% first applicants	0.48	0.25	0	0	1	176
% firms in consortium	0.18	0.10	0	0	0	176
% firms that won Phase I	0.15	0.10	0	0	0	176
% Seal of Excellence firms	0.40	0.17	0	0	1	176
mean requested grant amount	1649.71	919.20	1405	1477	1577	176
mean grant amount	1656.84	669.63	1237	1534	1896	176

Notes: the table reports descriptive statistics at the competition-level during the time span 2014-2017. The top panel computes these figures using the entire population of applicants, whereas the bottom panel uses the sample of applicants that were linked to ORBIS. The figures concerning the grant amount are expressed in thousand euros.

Table A2: Competition topics

	Population		Sample	
	N	%	N	%
Open Disruptive Innovation Scheme	3605	24.2	2657	23.9
Nanotechnologies	1407	9.4	1025	9.2
Biotechnology	354	2.4	275	2.5
Space research and development	249	1.7	205	1.8
Healthcare biotechnology	870	5.8	674	6.1
ICT solutions for health, well-being and ageing well	1434	9.6	1058	9.5
Sustainable agriculture, forestry, agri-food and bio-based sectors	1060	7.1	787	7.1
Innovative solutions for blue growth	228	1.5	157	1.4
Low carbon and efficient energy system	1626	10.9	1242	11.2
Transport and Smart Cities Mobility	1480	9.9	1038	9.4
Climate action, environment, resource efficiency and raw materials	1246	8.4	962	8.7
New business models for inclusive, innovative and reflective societies	686	4.6	518	4.7
Security research and development	659	4.4	497	4.5
Total	14904	100.0	11095	100.0

Notes: the table reports the distribution of the sample across all competition topics during the time-span 2014-2017.

Table A3: Comparison between applicants population and sample

	(1)	(2)	Std. Diff.
Applicants	75.29	78.03	-0.152
Partners	1.23	1.22	0.043
Uncentered ranks	76.13	73.47	0.140
Grant (%)	0.05	0.04	0.105
Local10 (%)	0.16	0.16	0.035
Local5 (%)	0.10	0.09	0.005
Firms in consortium (%)	0.16	0.15	0.032
North (%)	0.17	0.17	0.055
South (%)	0.33	0.35	0.137
East (%)	0.10	0.09	0.114
West (%)	0.34	0.33	0.052
Other (%)	0.06	0.06	0.092
Application year	2016.03	2016.09	0.249
Cut-off date	2.62	2.65	0.101
Grant amount (th. euros)	1638.79	1632.74	0.023
N	14904	11095	

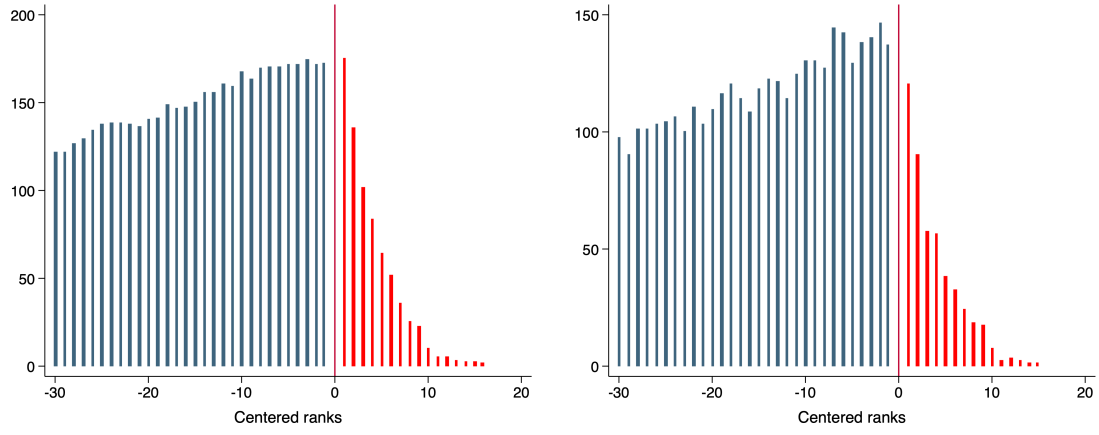
Notes: columns 1 contain means for the population of applicants. Columns 2 uses the sample of firms without missing variables for patents. Columns 3 reports mean standardized differences between column 1 and 2. Applicants refer to the number of participating firms; partners refer to the number of firms participating in the same application; uncentered ranks is the average raw rank. Grant indicates the percentage of winning firms; Seal the percentage of firms awarded with the “Seal of Excellence”; Losers refer to the remaining applicants; Grant amount is the amount received by winning firms.

Table A4: Applicants' countries

	ISO	Type	GDP	Population		Sample	
				N	%	N	%
Anguilla (UK)	AI	OCT	.	4	0.0		
Albania	AL	H2020	Q1	2	0.0	1	0.0
Austria	AT	EU-28	Q2	218	1.5	155	1.4
Bosnia and Herzegovina	BA	H2020	Q1	3	0.0	2	0.0
Belgium	BE	EU-28	Q2	210	1.4	179	1.6
Bulgaria	BG	EU-28	Q1	151	1.0	87	0.8
Switzerland	CH	H2020	Q2	53	0.4	37	0.3
Cyprus	CY	EU-28	Q2	44	0.3	38	0.3
Czech Republic	CZ	EU-28	Q1	45	0.3	33	0.3
Germany	DE	EU-28	Q2	1013	6.8	771	6.9
Denmark	DK	EU-28	Q2	525	3.5	426	3.8
Estonia	EE	EU-28	Q1	197	1.3	128	1.2
Greece	EL	EU-28	Q1	212	1.4	59	0.5
Spain	ES	EU-28	Q2	2225	14.9	1859	16.8
Finland	FI	EU-28	Q2	682	4.6	549	4.9
Faroer Islands (Denmark)	FO	H2020	Q2	2	0.0	1	0.0
France	FR	EU-28	Q2	1150	7.7	717	6.5
Georgia	GE	H2020	Q1	1	0.0		
Croatia	HR	EU-28	Q1	50	0.3	21	0.2
Hungary	HU	EU-28	Q1	483	3.2	329	3.0
Ireland	IE	EU-28	Q2	263	1.8	202	1.8
Israel	IL	H2020	Q2	799	5.4	590	5.3
Iceland	IS	H2020	Q2	55	0.4	39	0.4
Italy	IT	EU-28	Q2	2161	14.5	1644	14.8
Lithuania	LT	EU-28	Q1	38	0.3	27	0.2
Luxembourg	LU	EU-28	Q2	16	0.1	14	0.1
Latvia	LV	EU-28	Q1	90	0.6	78	0.7
Moldova	MD	H2020	Q1	13	0.1	5	0.0
Montenegro	ME	H2020	Q1	1	0.0	1	0.0
Macedonia	MK	H2020	Q1	5	0.0		
Malta	MT	EU-28	Q2	26	0.2	19	0.2
Netherlands	NL	EU-28	Q2	627	4.2	501	4.5
Norway	NO	H2020	Q2	244	1.6	190	1.7
Poland	PL	EU-28	Q1	259	1.7	178	1.6
Portugal	PT	EU-28	Q1	285	1.9	248	2.2
Romania	RO	EU-28	Q1	52	0.3	45	0.4
Serbia	RS	H2020	Q1	28	0.2	16	0.1
Sweden	SE	EU-28	Q2	664	4.5	478	4.3
Slovenia	SI	EU-28	Q2	219	1.5	177	1.6
Slovakia	SK	EU-28	Q1	123	0.8	91	0.8
Turkey	TR	H2020	Q1	143	1.0	45	0.4
Ukraine	UA	H2020	Q1	30	0.2	5	0.0
United Kingdom	UK	EU-28	Q2	1493	10.0	1110	10.0
Total				14904	100.0	11095	100.0

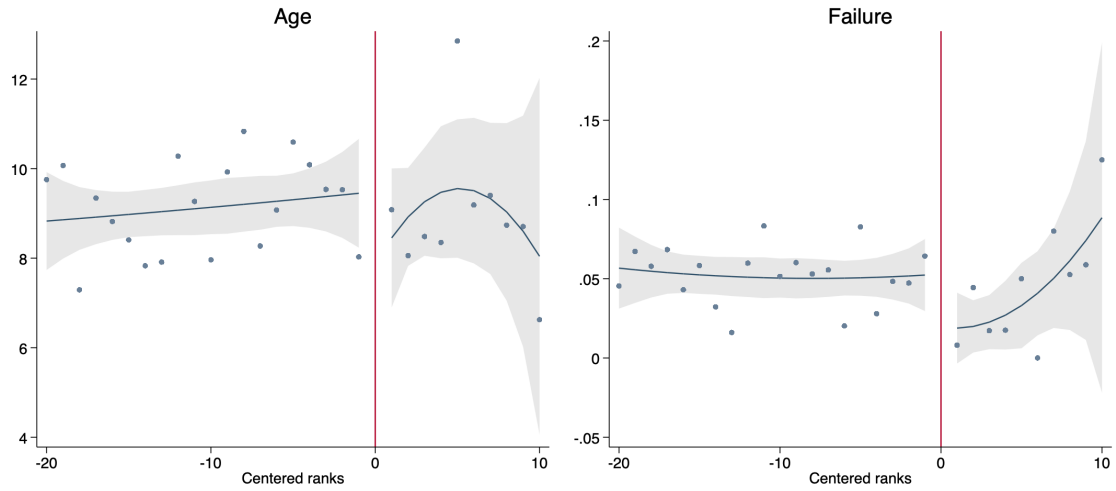
Notes: elaboration based on EASME data for 2014-2017 SME Instrument competitions. Type indicates whether the country is part of the European Union or an Horizon2020 associated country. Anguilla, Greenland and Virgin Islands are Overseas Countries and Territories (OCT) linked to the EU Member States. Area indicates the geographic area of the country. GDP indicates the quantile of GDP-per capital level (i.e. Q1 for low economic development, Q2 for high economic development). Data for GDP are drawn from the World Bank Development Indicators. Empty cells indicate missing observations.

Fig. A1: Applicants by centered ranks



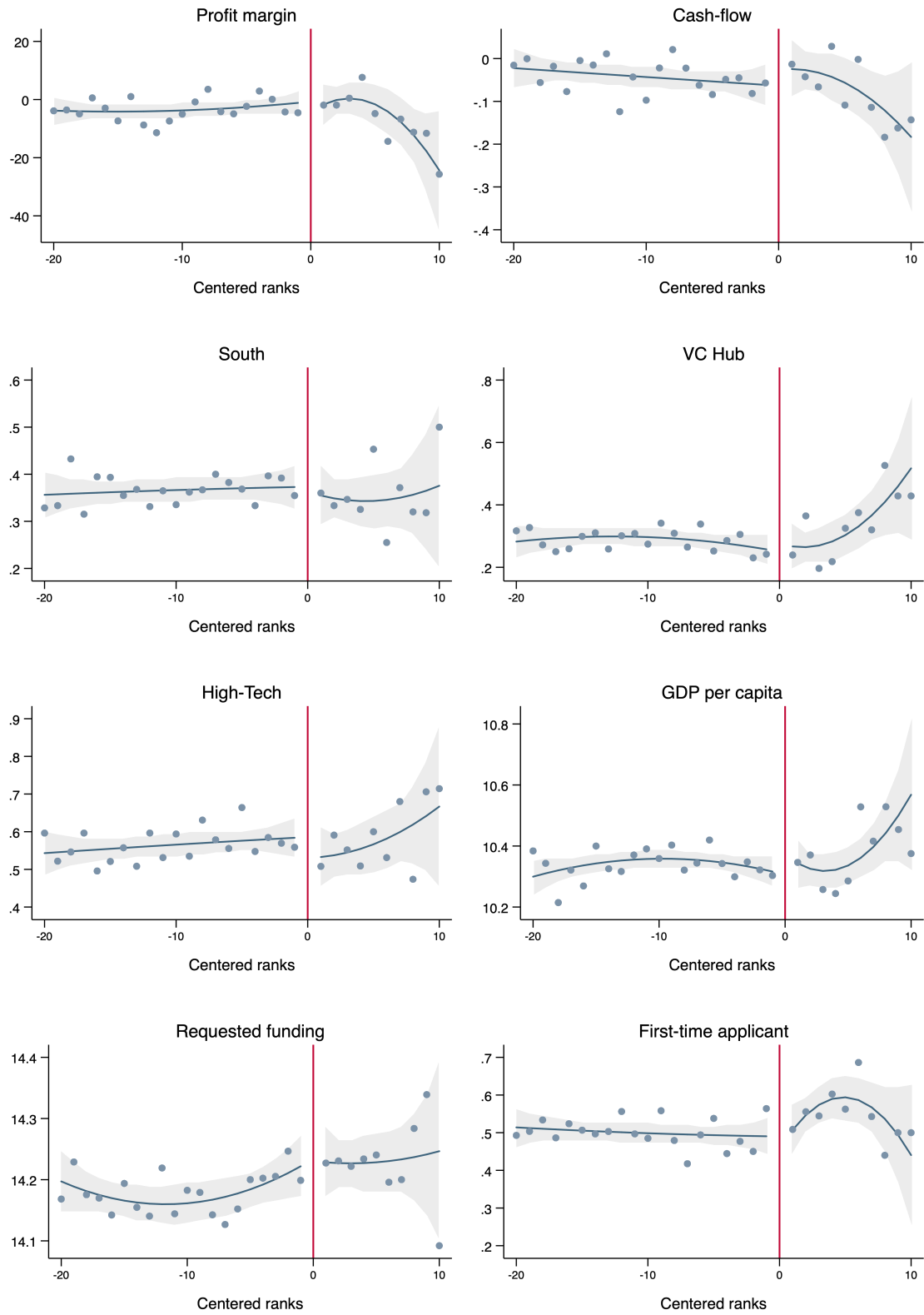
Notes: the figure reports the number of applicants by centered ranks for firms between centered ranks of -30 and +20. The left panel uses raw data while the right panel uses the estimation sample.

Fig. A2: RD plots for age (pre-grant) and failure likelihood (post-grant)



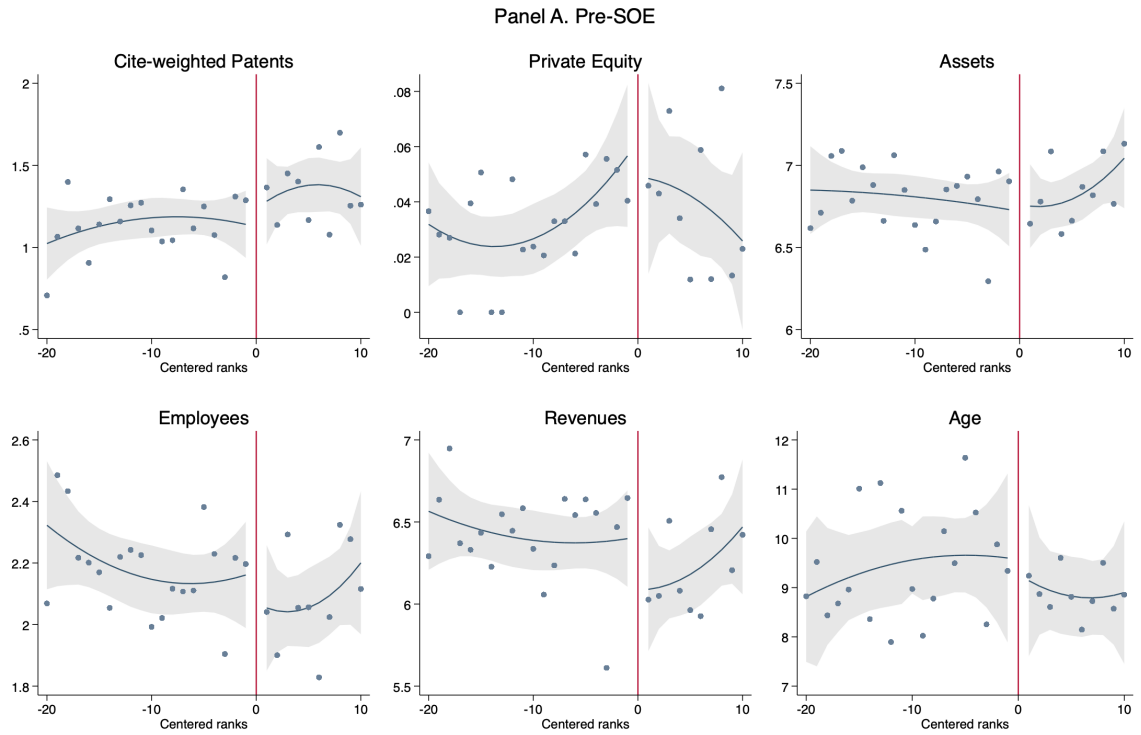
Notes: RD plots for firms with centered ranks between -20 and 10. Circles represent rank-level means of firm-level investment. Fitted lines from local polynomial regressions with a quadratic fit together with 95% confidence intervals.

Fig. A3: Graphical evidence of continuity in pre-assignment observables



Notes: RD plots for firms with centered ranks between -20 and 10. Circles represent rank-level means of the firm-level outcomes. Fitted lines from local polynomial regressions with a quadratic fit together with 95% confidence intervals.

Fig. A4: Graphical evidence of continuity in pre-assignment observables (SOE threshold)



Notes: figure reports RD plots to provide evidence of local continuity across SOE-recipient and losing firms. The ranks are re-centered so that 1 is the last SOE-winning firm, and -1 is the first losing firm. Fitted lines from local polynomial regressions with a quadratic fit together with 95% confidence intervals.

Table A5: Balancing tests of baseline observables and pre-SOE outcomes

	1 st order polynomial			2 nd order polynomial		
	All	±10	±5	All	±10	±5
Citw patents ^{Pre}	0.243*** (0.058)	0.073 (0.163)	0.104 (0.248)	0.144* (0.071)	-0.042 (0.274)	-0.430 (0.509)
Private Equity ^{Pre}	0.010 (0.007)	-0.012 (0.023)	0.020 (0.033)	0.000 (0.009)	-0.004 (0.033)	-0.024 (0.064)
Revenues ^{Pre}	-0.071 (0.072)	-0.469* (0.221)	-0.205 (0.409)	-0.077 (0.096)	-0.230 (0.423)	-1.574* (0.817)
Assets ^{Pre}	0.065 (0.054)	-0.191 (0.166)	0.090 (0.263)	0.045 (0.072)	0.037 (0.300)	-0.761 (0.505)
Employees ^{Pre}	-0.022 (0.043)	-0.224* (0.129)	-0.002 (0.198)	-0.012 (0.051)	-0.015 (0.217)	-0.561 (0.435)
Age ^{Pre}	-0.001 (0.028)	-0.037 (0.081)	-0.075 (0.130)	-0.007 (0.039)	-0.051 (0.149)	0.026 (0.249)
Cash-flow ^{Pre}	-0.014 (0.011)	-0.008 (0.039)	0.066 (0.060)	-0.014 (0.015)	0.097 (0.067)	0.006 (0.132)
Profit margin ^{Pre}	-0.185 (1.209)	-7.150* (3.548)	-7.633 (5.967)	-0.791 (1.578)	-8.305 (6.143)	-0.787 (10.879)
High-tech	0.011 (0.013)	0.011 (0.044)	0.065 (0.070)	-0.005 (0.019)	0.025 (0.076)	0.056 (0.124)
GDP per capita	0.031* (0.014)	0.054 (0.042)	0.037 (0.067)	0.029 (0.019)	-0.003 (0.075)	-0.100 (0.143)
VC Hub	0.001 (0.013)	-0.040 (0.038)	-0.007 (0.060)	0.000 (0.019)	0.013 (0.068)	0.149 (0.121)

Notes: results obtained estimating the following RDD equation by means of OLS with pre-determined observables as dependent variables: $Y_{ic}^{Pre} = \alpha + \beta Seal_{ic} + f(Rank_{ic}) + \delta_c + \varepsilon_{ic}$. Differently from our baseline specification, the treatment variable is $Seal_{ic}$, indicating with 1 if a firm has received the SOE, and 0 otherwise. The ranks are re-centered so that 1 is the last SOE-winning firm, and -1 is the first losing firm. Estimates are obtained using different bandwidths around the threshold (i.e. an infinite one, ±10 or ±5 centered ranks). All regressions include linear or quadratic polynomials of the running variable on both sides of the threshold and competition fixed effects. VC Hub is a dummy variable taking 1 if the firm is located in one of the top 15 EU NUTS2 regions with the highest concentration of VC investors, and 0 otherwise. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Additional results on firm-level outcomes

In this section we report additional results concerning the effects of grants on firm-level outcomes.

Table B1 examines the impact of grants on investment over a longer time-span. The dependent variable is the cumulated investment including $t + 2$ scaled by pre-assignment total assets. Estimations are based only on firms applying during 2014, 2015 and 2016 since they feature enough post-treatment observations. Results indicate substantially larger treatment effects. Table B2 and Table B3 examine the effects on tangible and intangible investments, respectively. Effects are stronger for intangibles.

Table B4 reports a series of robustness tests concerning the effects on patenting that, overall, corroborate the main findings. These include models where: the normalization of the dependent variable is performed using positive values different than one; the dependent variable is transformed using the inverse hyperbolic sine; point estimates are obtained via Negative Binomial and Poisson Pseudo Maximum Likelihood regressions without log transforming the dependent variable; citations are limited to two years after the patent application, we find similar results (Table B5); the dependent variable is the (log) number of patents (Table B6). Table B7 and Table B8 show that the effects on patenting stem from both extensive and intensive margins.

Table B9 and Table B10 show that R&D grants help companies raising more funding amount and more founding rounds. The dependent variables for these two models are the log of equity received plus one and the log of the number of equity deals plus one, respectively.

Table B11 examine whether grants decreases firm failure, namely, exit through bankruptcy, dissolution, liquidation or insolvency by 2019. Grants lead to a statistically significant decrease in failure chances. Finally, we examine whether R&D subsidies have an impact on the probability of experiencing successful exits and estimate the effect of the SME Instrument on IPO or M&A events. Estimation results reported in Tables B12 and B13 document that the effect is not

statistically different from zero (this is not entirely surprising given the very low number of IPO and M&A observed after 2014).

Finally, Figure B1 and Figure B2 plot the treatment effects over time. These effects are generally increasing with time.

Table B1: The effects on investment over longer time span

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.685** (0.271)	0.802* (0.429)	0.388** (0.195)	0.830** (0.380)	1.271** (0.570)	1.643*** (0.476)	1.834* (1.080)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	3634	3634	3634	784	784	451	451
R-squared	0.06	0.06	0.06	0.19	0.20	0.29	0.30
BIC	14847.36	14863.01	14856.29	3276.69	3287.02	1854.21	1865.71

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the cumulated investments during time t , $t + 1$ and $t + 2$ scaled by total assets at $t - 1$. Regressions include total assets at $t - 1$. Both variables are winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable (log of assets at time $t - 1$) and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: The effects on investment in tangibles

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.077*** (0.027)	0.072* (0.041)	0.061*** (0.019)	0.066* (0.036)	0.109* (0.062)	0.121** (0.059)	0.280** (0.116)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	6790	6790	6790	1224	1224	689	689
R-squared	0.04	0.04	0.04	0.19	0.19	0.26	0.26
BIC	-174.96	-160.93	-161.72	32.23	44.94	44.24	52.92

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the cumulated investments in tangibles during time t and $t + 1$ scaled by total assets at $t - 1$. Regressions include total assets at $t - 1$. Both variables are winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable (log of assets at time $t - 1$) and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: The effects on investment in intangibles

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.228** (0.088)	0.213 (0.139)	0.235*** (0.061)	0.238** (0.103)	0.230 (0.178)	0.350** (0.145)	0.860*** (0.322)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	6782	6782	6782	1221	1221	688	688
R-squared	0.06	0.06	0.06	0.21	0.22	0.30	0.30
BIC	13356.28	13365.87	13363.94	2616.79	2624.47	1460.90	1468.66

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the cumulated investments in intangibles during time t and $t + 1$ scaled by total assets at $t - 1$. Regressions include total assets at $t - 1$. Both variables are winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable (log of assets at time $t - 1$) and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Additional results on patenting

Bandwidths:	All	All	± 10	± 10	± 5	± 5
OLS with DV: $\log(1 + \text{Cite-weighted Patents}_{ic}^{Post})$						
Grant	0.203*** (0.068)	0.282** (0.117)	0.147* (0.085)	0.236* (0.138)	0.314*** (0.113)	0.390* (0.230)
N	11095	11095	1822	1822	1050	1050
BIC	23502.73	23516.83	4221.02	4234.39	2318.97	2332.66
OLS with DV: $\log(0.75 + \text{Cite-weighted Patents}_{ic}^{Post})$						
Grant	0.228*** (0.075)	0.314** (0.129)	0.167* (0.094)	0.270* (0.153)	0.355*** (0.125)	0.442* (0.254)
N	11095	11095	1822	1822	1050	1050
BIC	25834.32	25848.52	4583.38	4596.72	2530.31	2543.97
OLS with DV: $\log(0.50 + \text{Cite-weighted Patents}_{ic}^{Post})$						
Grant	0.265*** (0.085)	0.361** (0.145)	0.196* (0.107)	0.320* (0.174)	0.416*** (0.142)	0.521* (0.290)
N	11095	11095	1822	1822	1050	1050
BIC	28908.80	28923.15	5061.16	5074.44	2809.34	2822.95
OLS with DV: $\log(0.25 + \text{Cite-weighted Patents}_{ic}^{Post})$						
Grant	0.332*** (0.103)	0.447** (0.175)	0.250* (0.131)	0.416* (0.214)	0.529*** (0.176)	0.663* (0.357)
N	11095	11095	1822	1822	1050	1050
BIC	33648.65	33663.20	5799.22	5812.40	3240.88	3254.41
OLS with DV: $\log(0.10 + \text{Cite-weighted Patents}_{ic}^{Post})$						
Grant	0.423*** (0.128)	0.567** (0.218)	0.326* (0.166)	0.553** (0.270)	0.690*** (0.223)	0.860* (0.455)
N	11095	11095	1822	1822	1050	1050
BIC	39042.72	39057.45	6644.03	6657.08	3734.86	3748.31
OLS with DV: <i>Inverse Hyperbolic Sine</i>						
Grant	0.256*** (0.084)	0.348** (0.143)	0.185* (0.105)	0.295* (0.171)	0.392*** (0.140)	0.493* (0.285)
N	11095	11095	1822	1822	1050	1050
BIC	28351.61	28365.91	4994.96	5008.39	2766.90	2780.55
PPML with DV: $\text{Cite-weighted Patents}_{ic}^{Post}$						
Grant	0.269** (0.130)	0.502*** (0.192)	0.303* (0.168)	0.456* (0.245)	0.616*** (0.204)	0.739 (0.469)
N	10946	10946	1681	1681	905	905
BIC	44584.72	44580.93	8215.14	8211.31	4311.95	4324.50
NBREG with DV: $\text{Cite-weighted Patents}_{ic}^{Post}$						
Grant	0.526** (0.235)	0.918*** (0.353)	0.462* (0.260)	0.492 (0.406)	0.780** (0.329)	0.624 (0.711)
N	11095	11095	1822	1822	1051	1051
BIC	27972.82	27261.16	6003.25	5904.67	4145.42	4179.83

Notes: the first six rows report results from the same specification (Eq. 1) but with different transformations of the dependent variable. Regressions are estimated by means of OLS and for different bandwidths around the threshold. The last two rows report results from negative binomial and Poisson Pseudo Maximum Likelihood estimations (**silva2006log**), respectively. In such cases the dependent variable is not log transformed. **Op2** columns include a linear control for ranks while even columns include a quadratic control for ranks. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: The effects on cite-weighted patents (two-year citation window)

Bandwidths:	All	All	All	± 10	± 10	± 5	± 5
Grant	0.197*** (0.072)	0.288** (0.122)	0.138*** (0.048)	0.151* (0.085)	0.226 (0.141)	0.296*** (0.112)	0.388* (0.230)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	11095	11095	11095	1822	1822	1050	1050
BIC	22674.60	22688.74	22683.04	4100.74	4114.70	2282.38	2296.02
R-squared	0.33	0.33	0.34	0.41	0.41	0.46	0.46

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the log of cite-weighted patents applications plus one filed starting from the year after the competition. Only citations within two years from the application year are considered. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the threshold. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B6: The effects on patents (count)

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.215*** (0.061)	0.301*** (0.105)	0.146*** (0.045)	0.159** (0.077)	0.243* (0.124)	0.307*** (0.102)	0.328 (0.212)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	11095	11095	11095	1822	1822	1050	1050
R-squared	0.35	0.35	0.35	0.44	0.44	0.50	0.50
BIC	21077.26	21089.90	21082.84	3810.73	3823.66	2105.45	2119.03

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the (log) number of patent applications plus one starting from the year after the competition, and 0 otherwise. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: The effects on patents (dummy)

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.105*** (0.033)	0.142*** (0.054)	0.079*** (0.025)	0.086* (0.044)	0.160** (0.073)	0.185*** (0.061)	0.222* (0.128)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	11095	11095	11095	1822	1822	1050	1050
R-squared	0.24	0.24	0.24	0.33	0.33	0.38	0.38
BIC	10215.83	10230.50	10216.99	1772.72	1785.32	960.55	973.78

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is a dummy indicating with 1 if a firm has applied for a patent starting from the year after the competition, and 0 otherwise. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: The effects on the extensive margin for patents

	Patents ^{Post} (d)							
	w/o Patents ^{Pre}				w/ Patents ^{Pre}			
	(1) All	(2) All	(3) ± 10	(4) ± 10	(5) All	(6) All	(7) ± 10	(8) ± 10
Grant	0.096** (0.042)	0.108 (0.073)	0.113** (0.054)	0.129 (0.092)	0.130*** (0.045)	0.181** (0.070)	0.095 (0.072)	0.256** (0.124)
Rank \times Grant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	Yes	No	Yes	No	Yes
N	7005	7005	999	999	4080	4080	803	803
R-squared	0.09	0.09	0.28	0.28	0.10	0.10	0.26	0.26
BIC	3411.03	3424.99	552.75	566.37	5518.50	5534.24	920.95	929.54

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is a dummy indicating with 1 if a firm has applied for a patent starting from the year after the competition, and 0 otherwise. Columns 1 to 4 report different specifications using the sample of firms without pre-competition patents. Columns 5 to 8 report different specifications using the sample of firms with pre-competition patents. All regressions include competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: The effects on private equity amount

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.386 (0.236)	0.969*** (0.369)	0.200 (0.123)	0.464** (0.228)	0.766** (0.366)	0.731** (0.302)	1.391** (0.660)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	8352	8352	8352	1358	1358	784	784
R-squared	0.06	0.06	0.06	0.16	0.16	0.28	0.28
BIC	29057.37	29055.44	29076.55	4940.83	4951.47	2852.59	2863.66

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the (log) of one plus the private equity amount received after the competition. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B10: The effects on private equity deals

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.082** (0.035)	0.158*** (0.054)	0.037* (0.019)	0.093** (0.037)	0.183*** (0.062)	0.156*** (0.050)	0.235** (0.109)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	8352	8352	8352	1358	1358	784	784
R-squared	0.07	0.07	0.07	0.20	0.20	0.30	0.30
BIC	-1185.83	-1180.51	-1158.91	13.60	21.67	28.81	40.67

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the (log) of one plus the number of private equity deals after the competition. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: The effects on firm failure

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	-0.044*** (0.012)	-0.055*** (0.012)	-0.020** (0.009)	-0.048*** (0.017)	-0.058* (0.031)	-0.067** (0.031)	-0.129** (0.057)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	11402	11402	11402	1787	1787	1011	1011
R-squared	0.03	0.03	0.04	0.10	0.10	0.14	0.15
BIC	-435.78	-417.44	-422.04	-781.32	-767.09	-568.16	-557.81

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is a dummy indicating a firm has failed in the years after the competition (as of March 2019). Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. In order to select the most appropriate polynomial order within a given bandwidth (e.g. columns 4-5 both using a ± 10 bandwidth), we report the BIC and choose the model with the minimum value as the preferred specification. All regressions include competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B12: The effects on IPO

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.005 (0.007)	-0.003 (0.008)	0.004 (0.005)	-0.001 (0.010)	-0.009 (0.011)	-0.002 (0.011)	-0.038* (0.022)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	8432	8432	8432	1365	1365	789	789
R-squared	0.02	0.02	0.02	0.09	0.09	0.16	0.16
BIC	-25299.23	-25283.18	-25283.15	-3435.67	-3421.72	-1874.66	-1863.94

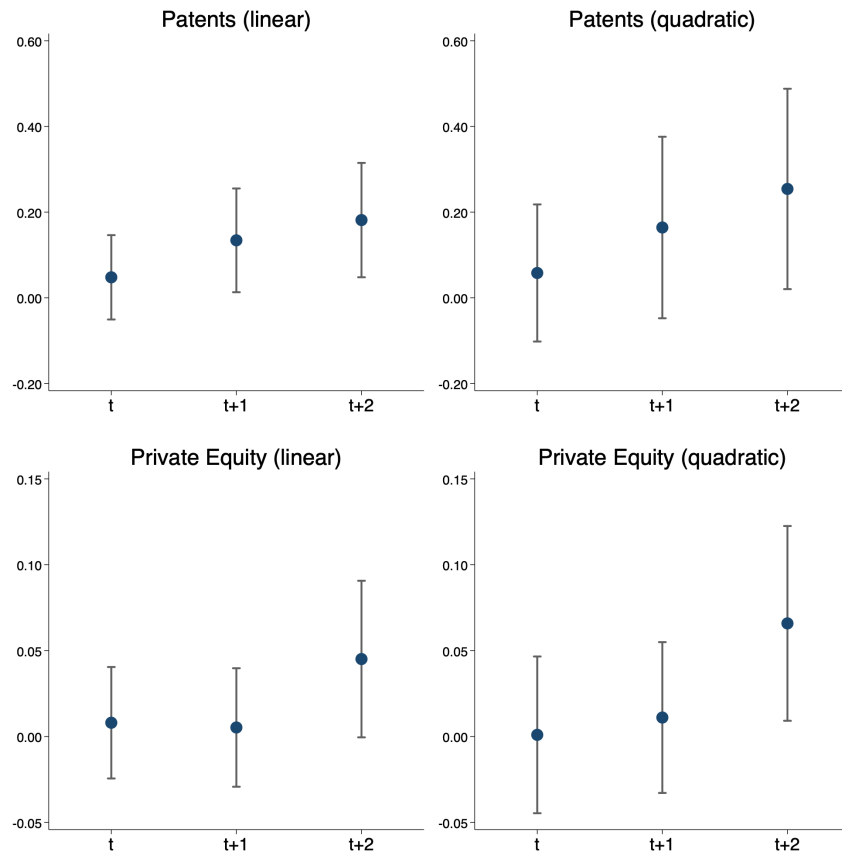
Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is a dummy indicating whether a firm has exited through an IPO (as of March 2019). Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B13: The effects on M&A

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	-0.010 (0.012)	-0.027** (0.012)	-0.002 (0.010)	-0.001 (0.015)	-0.022 (0.022)	-0.009 (0.018)	-0.045 (0.038)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	8432	8432	8432	1365	1365	789	789
R-squared	0.03	0.03	0.03	0.14	0.14	0.15	0.15
BIC	-8981.68	-8965.42	-8965.87	-1311.00	-1300.92	-1011.68	-999.91

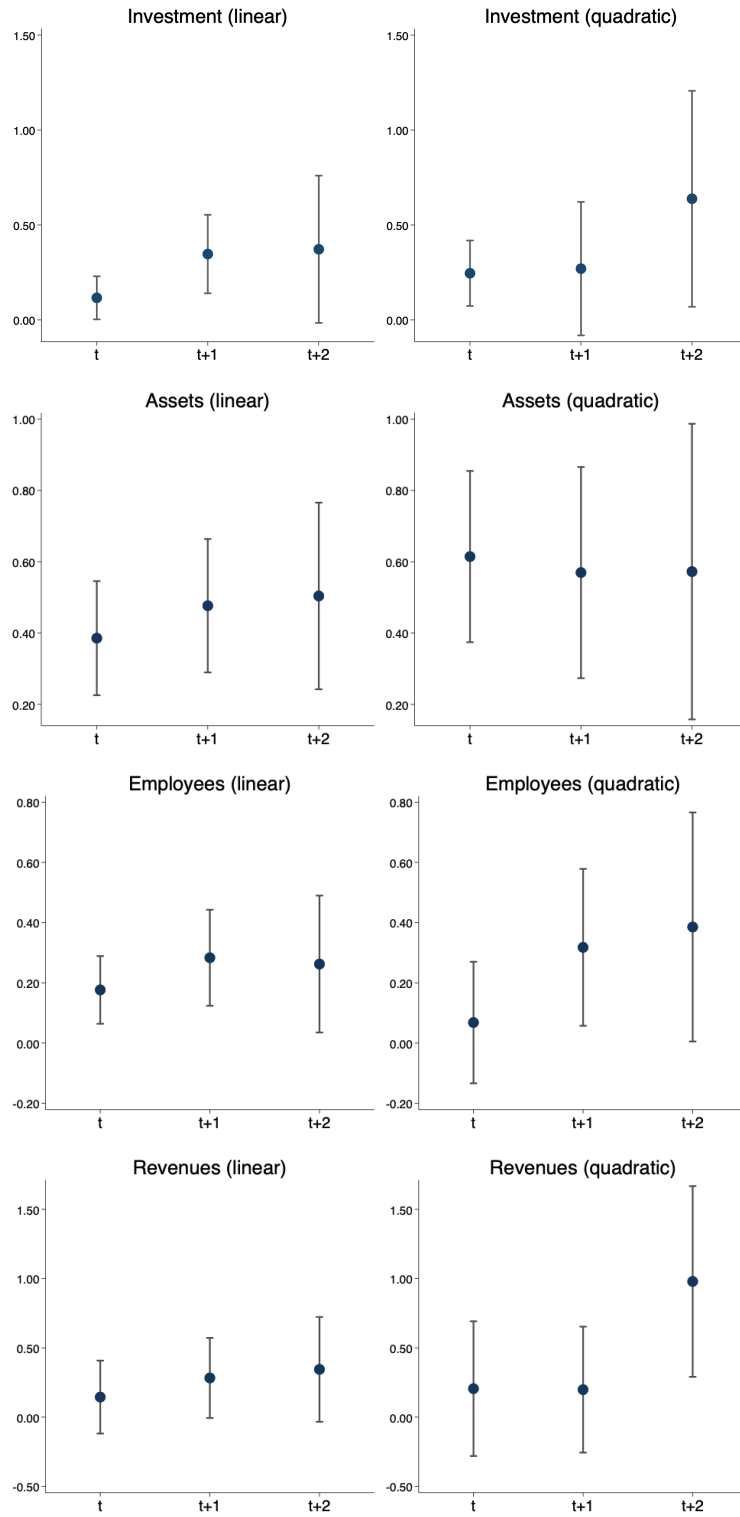
Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is a dummy indicating whether a firms has exited through an acquisition. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Fig. B1: Treatment effects over time



Notes: results obtained based on equation (1) with private equity (dummy) as dependent variable. Regressions include linear ranks on both sides (left), or quadratic ones (right), pre-grant dependent variable, competition fixed effects and based on firms within ± 10 centered ranks. Standard errors clustered at the competition-level. 95% confidence intervals reported.

Fig. B2: Treatment effects over time



Notes: results obtained based on equation (1) with private equity (dummy) as dependent variable. Regressions include linear ranks on both sides (left), or quadratic ones (right), pre-grant dependent variable, competition fixed effects and based on firms within ± 10 centered ranks. Standard errors clustered at the competition-level. 95% confidence intervals reported.

C Heterogeneous effects

This section contains the results on heterogeneous treatment effects. First, Table C1 reports estimates concerning the differential effects across firm-level proxies for financial constraints (i.e. firm age and size). Second, in Table C2 we examine the differential effect of R&D grants across sectoral-level measures of financial frictions. For example, firms operating in sectors with more tangible assets can pledge more collateral and might find it easier to secure external finance (Braun and Larrain, 2005), thus experiencing a lower sensitivity to the receipt of R&D grants. Likewise, sectors featuring the ability to generate more cash-flow might be find it easier to self-finance their investment and display a lower responsiveness to the receipt of R&D grants (Brown et al., 2009). We use two proxies for a sector’s financial vulnerability: asset tangibility and liquidity. We compute these proxies using firm-level balance-sheet data for the period preceding the competition, so that the effects of the grant do not contaminate the sectoral proxies. We first calculate asset tangibility (i.e. tangible assets over total assets) and liquidity (cash-flow over total assets) at the firm-level and use the median value across all firms within a 2-digit NACE rev. 2 sector. We then group the sectors depending on whether they are above or below the corresponding median. We report estimation results in Table C2 and show that firms operating in sectors that are financially stronger (i.e. characterized by higher asset tangibility and liquidity) display smaller treatment effects with respect to firms active in financially weaker sectors.

In Table C3 we test whether the impact of R&D grant varies according to the economic development of the recipients’ country. In the same table, we we investigate the heterogeneous response across different levels of financial development. If R&D grants alleviate financial constraints, one might expect larger treatment effects for those firms located in countries characterized by lower availability of credit. To test this hypothesis we employ country-level data on the ratio of domestic credit to the private sector over GDP and divide the sample based on the

corresponding median level.⁴⁸ Results indicate a negative relationship between financial development levels and treatment effects for most outcomes. In other words, firms in countries with lower credit availability tend to reap larger benefits from R&D grants. The only exception for both economic and financial development is private equity: countries with the highest levels of economic and financial development feature the largest treatment effects. One potential explanation for this is related to the supply-side: private equity plays a bigger role in countries with highly developed capital markets, and private equity firms, which are more abundant in these countries, tend to invest in firms that are closer to them.

C4 reports heterogeneous effects across European regions (NUTS2) depending on their GDP per capita. Firms in less economically developed regions generally benefit more from grants. Finally, Table C5 tests whether firms in more patent-intensive sector benefit more from grants. We find that firms in sectors with higher patenting intensity benefit more, though this is not statistically significant.

⁴⁸ Data refer to 2013 and are drawn from the World Bank Global Financial Development Database. While financial development is linked with economic development, the samples differ slightly from each other (i.e. five countries change quintile).

Table C1: Heterogeneous effects across firm-level proxies of financial constraints

	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.260*** (0.080)	0.097*** (0.033)	0.733*** (0.182)	0.824*** (0.088)	0.469*** (0.085)	0.689*** (0.202)	-0.055*** (0.013)
Age > p50	-0.084*** (0.013)	-0.019*** (0.003)	-0.505*** (0.027)	-0.234*** (0.022)	-0.177*** (0.020)	-0.154*** (0.036)	-0.022*** (0.005)
Grant × Age > p50	-0.117 (0.081)	-0.049* (0.029)	-0.575*** (0.160)	-0.487*** (0.082)	-0.232*** (0.067)	-0.360** (0.181)	0.020 (0.013)
N	11003	8352	6871	7304	5491	5117	11313
R-squared	0.36	0.07	0.10	0.77	0.80	0.77	0.04
	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.287*** (0.086)	0.089*** (0.030)	0.872*** (0.185)	0.923*** (0.083)	0.509*** (0.078)	0.802*** (0.168)	-0.059*** (0.018)
Size > p50	0.049*** (0.018)	0.018*** (0.005)	-0.433*** (0.029)	0.145*** (0.042)	0.174*** (0.026)	0.493*** (0.049)	-0.015*** (0.004)
Grant × Size > p50	-0.087 (0.080)	-0.003 (0.030)	-0.863*** (0.181)	-0.715*** (0.083)	-0.330*** (0.072)	-0.659*** (0.163)	0.033** (0.016)
N	8121	6217	6873	7306	5249	5029	8339
R-squared	0.36	0.08	0.09	0.77	0.79	0.77	0.05

Notes: results obtained using equation (1) augmented with a firm-level financial constraint proxy. Age is a dummy variable taking the value of 1 if a firm is above the median age, and 0 otherwise.. Size is a dummy variable taking the value of 1 if a firm is above the median pre-determined assets, and 0 otherwise. All regressions include linear ranks on both sides of the thresholds, pre-grant dependent variables and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Heterogeneous effects across sector-level proxies of financial constraints

	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.226*** (0.077)	0.098*** (0.035)	0.543*** (0.179)	0.693*** (0.085)	0.471*** (0.076)	0.523*** (0.189)	-0.053*** (0.016)
CFA > p50	-0.040*** (0.014)	-0.012*** (0.004)	-0.112*** (0.028)	-0.006 (0.023)	0.085*** (0.016)	0.112*** (0.033)	-0.034*** (0.005)
Grant × CFA > p50	-0.083 (0.081)	-0.049* (0.029)	-0.286* (0.171)	-0.282*** (0.088)	-0.237*** (0.075)	-0.058 (0.170)	0.013 (0.018)
N	10580	7927	6563	6971	5263	4885	10874
R-squared	0.36	0.07	0.05	0.77	0.79	0.77	0.04
	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.204** (0.080)	0.073** (0.031)	0.487*** (0.155)	0.611*** (0.078)	0.361*** (0.066)	0.537*** (0.157)	-0.041*** (0.014)
TNG > p50	-0.054*** (0.019)	-0.008* (0.005)	-0.098*** (0.031)	-0.063** (0.028)	-0.006 (0.017)	0.058* (0.034)	0.001 (0.007)
Grant × TNG > p50	-0.061 (0.108)	0.010 (0.034)	-0.288* (0.160)	-0.240*** (0.086)	-0.089 (0.073)	-0.176 (0.161)	-0.012 (0.017)
N	10580	7927	6563	6971	5263	4885	10874
R-squared	0.36	0.07	0.05	0.77	0.79	0.77	0.04

Notes: results obtained using equation (1) augmented with a sectoral-level financial constraint proxy. We compute the sectoral-level measures of financial constraints using firm-level balance-sheet data for the period preceding the competition, so that the effects of the grant do not contaminate the sectoral proxies. We then take the median only for those 2-digit NACE rev. 2 sector with at least 50 observations. CFA is a dummy variable taking the value of 1 if a sector is above the median level of cash-flow over total assets, and 0 otherwise. TNG is a dummy variable taking the value of 1 if a sector is above the median level of tangible assets over total assets, and 0 otherwise. All regressions include the pre-grant dependent variables and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Heterogeneous effects across country-level economic and financial development

	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.147* (0.088)	0.010 (0.021)	1.013** (0.486)	1.048*** (0.200)	0.567*** (0.103)	1.085*** (0.284)	0.009 (0.035)
GDP > p50	0.140*** (0.016)	0.025*** (0.003)	0.006 (0.050)	0.111*** (0.033)	0.152*** (0.021)	0.222*** (0.036)	0.039*** (0.007)
Grant × GDP > p50	0.063 (0.105)	0.067*** (0.017)	-0.629 (0.504)	-0.542*** (0.197)	-0.272*** (0.098)	-0.700*** (0.261)	-0.060 (0.036)
N	11095	8352	6873	7306	5493	5119	11402
R-squared	0.36	0.07	0.05	0.77	0.79	0.77	0.04
	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.098 (0.097)	0.024 (0.030)	1.102** (0.488)	1.069*** (0.210)	0.604*** (0.106)	1.223*** (0.310)	0.000 (0.029)
FIN > p50	-0.017 (0.018)	0.011*** (0.004)	0.009 (0.048)	0.104*** (0.034)	0.152*** (0.021)	0.220*** (0.042)	0.037*** (0.005)
Grant × FIN > p50	0.114 (0.111)	0.054** (0.026)	-0.703 (0.502)	-0.548*** (0.204)	-0.286*** (0.093)	-0.843*** (0.277)	-0.052* (0.030)
N	11055	8319	6839	7268	5468	5089	11360
R-squared	0.36	0.07	0.05	0.77	0.79	0.77	0.04

Notes: results obtained using equation (1) augmented with a country-level proxies for economic and financial development. We compute the country-level measures of economic and financial development using, respectively, GDP-per capita and domestic credit to the private sector for 2013. GDP is a dummy variable taking the value of 1 if a country is above the median level of GDP-per capita, and 0 otherwise. FIN is a dummy variable taking the value of 1 if a country is above the median level of domestic credit to the private sector, and 0 otherwise. All regressions include linear ranks on both sides of the thresholds, pre-grant dependent variables and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C4: Heterogeneous effects across regional economic development

	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.248*** (0.074)	0.065* (0.034)	0.841*** (0.209)	0.756*** (0.099)	0.501*** (0.074)	0.675*** (0.175)	-0.003 (0.023)
REG > p50	0.110*** (0.014)	0.022*** (0.004)	0.071** (0.032)	0.105*** (0.022)	0.116*** (0.015)	0.194*** (0.029)	0.029*** (0.006)
Grant × REG > p50	-0.049 (0.085)	0.006 (0.030)	-0.518** (0.218)	-0.258*** (0.095)	-0.236*** (0.075)	-0.296* (0.153)	-0.050** (0.023)
N	10130	7879	6705	7112	5281	4982	10419
R-squared	0.36	0.07	0.05	0.77	0.79	0.77	0.04

Notes: results obtained using equation (1) augmented with a region-level proxy for economic development. We compute the region-level measure of economic development using the NUTS2 GDP-per capita for 2013. REG is a dummy variable taking the value of 1 if a NUTS2 region is above the median level of GDP per capita, and 0 otherwise. All regressions include linear ranks on both sides of the thresholds, pre-grant dependent variables and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C5: Heterogeneous effects across sectoral patenting intensity

	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Investment ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.148* (0.086)	0.077* (0.039)	0.321** (0.158)	0.543*** (0.081)	0.300*** (0.090)	0.422** (0.174)	-0.032 (0.021)
PAT	0.087*** (0.015)	0.003 (0.004)	0.122*** (0.031)	0.038* (0.021)	0.017 (0.018)	0.009 (0.028)	-0.001 (0.005)
Grant × PAT	0.091 (0.083)	-0.001 (0.030)	0.176 (0.171)	0.012 (0.085)	0.042 (0.080)	0.087 (0.168)	-0.010 (0.020)
N	10200	7698	6850	7275	5479	5107	10455
R-squared	0.36	0.08	0.05	0.77	0.79	0.77	0.04

Notes: results obtained using a variant of equation (1). PAT is a dummy variable taking the value of 1 if a firm operates in a 4-digit NACE sector that is considered patent-intensive according to EUIPO-EPO, [2016](#), and 0 otherwise. All regressions include linear ranks on both sides of the thresholds, pre-grant dependent variables and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Additional results on certification vs funding

In this section we provide evidence on which of the two channels (i.e. certification vs funding) embedded in the grant prevails. In principle, while the baseline results on private equity financing are consistent with certification effects, the results on patenting and the other outcomes are more difficult to ascribe to this causal mechanism. If certification is at work, the effects on patenting should work through the receipt of private equity after the competition. That is, only those firms receiving private equity thanks to the certification effect of the grant should witness an increase in patenting. To check if the effects on patenting are due to equity financing, we re-run our models by splitting the sample into firms that do or do not receive private equity after the grant. Results show that the increase in patenting is mainly driven by those firms not receiving private equity (Appendix Table D2). On the contrary, the effect on private equity appears to be driven by post-competition patenting. Indeed, those firms filing patent applications after the receipt of the grant have higher probabilities of obtaining private equity than those that do not patent (Appendix Table D3). This is consistent with the funding channel: grants are used by firms to perform R&D activities that result in patentable technology; this, in turn, provides a ‘quality stamp’ that attracts private finance. Hence, if anything, certification effects (stemming from patents) are at work via funding effects.

As already mentioned, the positive effects of R&D grants on private equity are consistent with the certification hypothesis. In this scenario, the quality signal generated by the grant is expected to benefit winners as soon as the competition results are announced. Hence, the impact of certification is expected to materialize immediately after competitions. We therefore estimate treatment effects over time (i.e. from time t to time $t + 2$). Results show that the positive effects of the grant on private equity appear only at time $t + 2$ whereas those recorded for patenting

emerge earlier (see Appendix Figure B1).⁴⁹ This delay in private equity financing constitutes evidence against the certification effect.

An additional indirect test on certification vs funding is related to grant size. If funding is the main mechanism we should observe that firms receiving larger grants enjoy greater performance premia (Lerner, 2000). On the contrary, if certification were the main channel, a large subsidy would yield a similar effect to a small one. To explore this, we exploit variation in R&D grant size (the amount varies from 0.5 up to 2.5 million). We run our baseline models letting the $Grant_{ic}$ coefficient vary depending on whether the winning firm obtains an above-median or below-median grant size. Although coefficients of these two interactions are not always statistically different from one another, we can observe that firms obtaining larger amounts systematically drive the overall results (Appendix Table D4). Although we cannot altogether exclude that larger grants themselves constitute stronger signals for investors when firms seek external finance, we interpret this as further evidence that certification is not the main mechanism through which the SME Instrument generates positive outcomes.

We also check whether ranks are correlated with outcomes conditional on firms receiving the R&D grant. This is informative because private investors observe winners (and SOE-certified firms) within each competition and this could be perceived as a signal of firm quality. We explore this issue by examining the coefficients on ranks in all models predicting follow-on equity finance, and find no statistically significant relationship. Also, we examine whether ranks are predictive of future private equity financing when the sample is composed only by grant-winning and SOE-certified firms. In this set-up, both groups of firms receive some form of certification and we could expect ranks to be correlated with subsequent financing events on both sides of the threshold. However, ranks have relatively small explanatory power and we do not detect any statistically significant association.

⁴⁹ The rather quick response of patenting to grants is in line with the evidence reported by Howell (2017). It is also worth recalling that in our setting grants target product development activities, that is, innovation projects at an advanced stage for which time-to-patent is arguably shorter than those at a proof-of-concept stage.

Finally, we exploit balance-sheet data to study whether R&D grants affect the amount and composition of firm debt. This is interesting since certification effects would arguably increase the chances for grant-winning firms to receive bank credit (Meuleman and De Maeseneire, 2012). As for private equity investors, the signal about firm quality contained in the grant may help lenders in their evaluation of borrowers by reducing information asymmetries and the perceived risk of innovative projects. Moreover, certification might entail an effect on the type of debt: banks might be more willing to provide long-term debt to firms that already received a positive screening by government agencies. Hence, for certification to be the main channel, these conjectures should be reflected into i) an increase in the amount of debt and ii) a re-balancing towards long-term debt. We begin by testing whether R&D grants cause an increase in total debt over total assets. Results indicate that the effects are negative but generally not statistically significant, thus rejecting the certification effect (Appendix Table D5). Next, we examine the effects of the R&D grants on the ratio between long-term debt over total debt. Point estimates are positive but small in magnitude and never statistically significant (Appendix Table D6).

In sum, all of the above tests do not provide any evidence, direct or indirect, in favor of certification, and point instead quite clearly to the funding channel as the primary mechanism behind the results.

Table D1: Grant-winning firms vs SOE recipient firms

	Patents ^{Post}				PE ^{Post}			
	(1) All	(2) All	(3) ±10	(4) ±10	(5) All	(6) All	(7) ±10	(8) ±10
Grant	0.203*** (0.068)	0.206*** (0.073)	0.147* (0.085)	0.177* (0.091)	0.071** (0.028)	0.066** (0.027)	0.081*** (0.028)	0.079*** (0.028)
N	11095	5340	1822	1632	8218	3710	1329	1170
R-squared	0.36	0.38	0.45	0.48	0.07	0.09	0.17	0.19
	Assets ^{Post}				Employees ^{Post}			
	(1) All	(2) All	(3) ±10	(4) ±10	(5) All	(6) All	(7) ±10	(8) ±10
Grant	0.561*** (0.065)	0.522*** (0.071)	0.477*** (0.095)	0.514*** (0.100)	0.330*** (0.062)	0.322*** (0.064)	0.283*** (0.081)	0.300*** (0.083)
N	7306	3651	1311	1166	5493	2782	962	856
R-squared	0.77	0.73	0.74	0.72	0.79	0.78	0.80	0.80
	Revenues ^{Post}				Exit ^{Post}			
	(1) All	(2) All	(3) ±10	(4) ±10	(5) All	(6) All	(7) ±10	(8) ±10
Grant	0.489*** (0.136)	0.360** (0.144)	0.283* (0.146)	0.256* (0.146)	-0.044*** (0.012)	-0.033*** (0.012)	-0.033** (0.016)	-0.027* (0.016)
N	5119	2548	867	776	11402	5474	1872	1674
R-squared	0.77	0.76	0.78	0.78	0.03	0.04	0.12	0.11

Notes: results obtained using different specifications of equation (1) by means of OLS. Odd columns report our baseline estimates using the entire sample of firms whereas even columns display results obtained by limiting the sample to grant-winning firms and SOE-winning firms only. All regressions include the pre-grant dependent variable, linear polynomials for the centered ranks separately for each side of the threshold and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D2: The effects on patents for firms (not) receiving private equity

	Patents ^{Post}			
	(1) w/ PE ^{Post}	(2) w/o PE ^{Post}	(3) w/ PE ^{Post}	(4) w/o PE ^{Post}
Grant	0.063 (0.365)	0.233*** (0.071)	-0.247 (0.521)	0.266** (0.132)
Rank × Grant	Yes	Yes	Yes	Yes
Rank ² × Grant	No	No	Yes	Yes
N	242	7921	242	7921
R-squared	0.69	0.40	0.70	0.40

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the log of cite-weighted patents plus one. Odd columns reports estimations using only firms with post-grant private equity. Even columns use only firms without post-grant private equity. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D3: The effects on private equity for firms (not) patenting

	PE ^{Post}			
	(1) w/o Pat ^{Post}	(2) w/ Pat ^{Post}	(3) w/o Pat ^{Post}	(4) w/ Pat ^{Post}
Grant	0.040 (0.031)	0.099** (0.047)	0.054 (0.056)	0.200*** (0.063)
Rank × Grant	Yes	Yes	Yes	Yes
Rank ² × Grant	No	No	Yes	Yes
N	6424	1911	6424	1911
R-squared	0.06	0.13	0.06	0.13

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is a dummy variable indicating whether a firm has received private equity financing after the competition. Odd columns limit the sample to firms without post-grant patenting activities while even columns include only firms with post-grant patenting activities. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D4: Sensitivity to grant size

	Patents ^{Post}		PE ^{Post}		Revenues ^{Post}	
	(1) All	(2) ±10	(3) All	(4) ±10	(5) All	(6) ±10
Grant × Small	0.127 (0.081)	0.103 (0.100)	0.072* (0.031)	0.081*** (0.030)	0.328** (0.160)	0.102 (0.160)
Grant × Large	0.259*** (0.080)	0.205** (0.083)	0.084*** (0.031)	0.086*** (0.031)	0.592*** (0.146)	0.428*** (0.162)
Rank × Grant	Yes	Yes	Yes	Yes	Yes	Yes
N	11095	1822	8352	1358	5119	867
R-squared	0.36	0.45	0.06	0.17	0.77	0.78
	Employees ^{Post}		Assets ^{Post}		Failure ^{Post}	
	(1) All	(2) ±10	(3) All	(4) ±10	(5) All	(6) ±10
Grant × Small	0.295*** (0.072)	0.224** (0.090)	0.537*** (0.079)	0.399*** (0.102)	-0.045** (0.017)	-0.035** (0.017)
Grant × Large	0.347*** (0.069)	0.319*** (0.085)	0.573*** (0.074)	0.520*** (0.103)	-0.055*** (0.014)	-0.034** (0.015)
Rank × Grant	Yes	Yes	Yes	Yes	Yes	Yes
N	5493	962	7306	1311	11402	1872
R-squared	0.79	0.80	0.77	0.74	0.03	0.12

Notes: results obtained using different specifications of equation (1) by means of OLS. The award coefficient is interacted with a dummy variable that indicates with 1 the receipt of a R&D grant that is above the median value, with 0 below the median value. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D5: The effects on debt over total assets

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	-0.092 (0.063)	-0.130 (0.088)	-0.065* (0.039)	-0.125 (0.089)	-0.174 (0.145)	-0.016 (0.135)	-0.425* (0.244)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	4438	4438	4438	926	926	553	553
R-squared	0.79	0.79	0.79	0.75	0.75	0.75	0.75
BIC	16207.81	16225.19	16225.00	2680.54	2693.32	1509.61	1510.69

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the ratio between total debt and total assets at time $t + 1$ (with t being the year of the competition). The pre-award variable is the ratio between total debt and total assets at time $t - 1$. Both variables are winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D6: The effects on long-term debt over total debt

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Grant	0.019 (0.029)	0.023 (0.044)	-0.021 (0.018)	0.008 (0.042)	0.039 (0.064)	0.027 (0.058)	0.052 (0.110)
Rank \times Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank ² \times Grant	No	Yes	No	No	Yes	No	Yes
Rank quintiles	No	No	Yes	No	No	No	No
N	6440	6440	6440	1156	1156	655	655
R-squared	0.04	0.04	0.04	0.15	0.15	0.25	0.25
BIC	2389.77	2400.64	2391.30	329.58	343.14	117.07	129.22

Notes: results obtained using different specifications of equation (1) by means of OLS. The dependent variable is the ratio between long-term debt and total debt at time $t + 1$ (with t being the year of the competition). The pre-award variable is the ratio between total debt and total assets at time $t - 1$. Both variables are winsorized at the 2% and 98% of the distribution over the whole sample. Columns 1 to 3 report estimates using infinite bandwidths (i.e. all firms). Columns 4-5 and 6-7 report estimates obtained using bandwidths of, respectively, 10 and 5 ranks around the cut-off. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D7: SOE spillovers on entrepreneurship

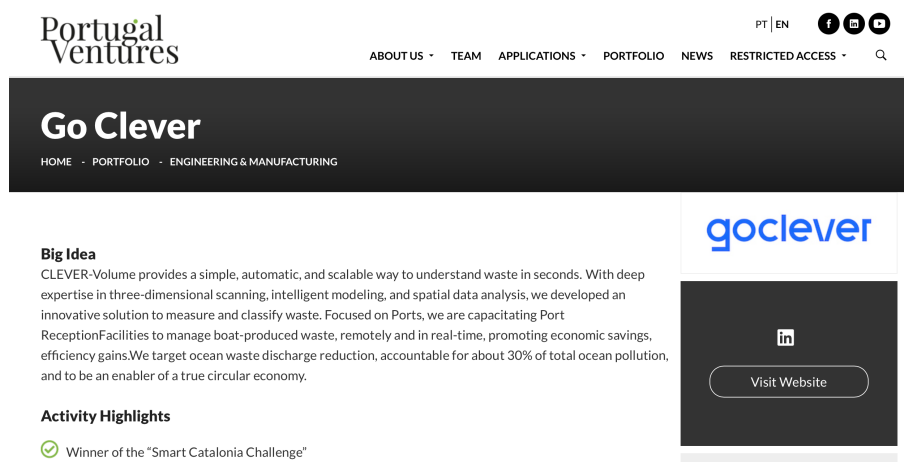
	Entry^{Pre}		ΔEntry^{Pre}		$\Delta\text{Entry}^{Post}$	
	(1) All	(2) ± 10	(3) All	(4) ± 10	(5) All	(6) ± 10
Seal	-0.003 (0.003)	0.047 (0.037)	-0.002 (0.005)	0.047 (0.066)	0.005 (0.003)	-0.006 (0.040)
Rank \times Seal	Yes	Yes	Yes	Yes	Yes	Yes
N	4197	426	4197	426	4197	426
R-squared	0.98	0.98	0.52	0.50	0.52	0.64
BIC	-10218.00	-1081.43	-7147.38	-670.36	-6405.67	-726.15

Notes: results obtained using different specifications of equation (1) by means of OLS. The treatment variable (Seal) is a dummy variable indicating whether a firm has received the Seal of Excellence. Ranks are re-centered so that 0 lies between the last SOE-winning firms and the first SOE-losing firm. Columns 1-4 report balancing tests on pre-assignment entry rates (i.e. number of newly-born enterprises as a proportion of the total number of active enterprises) at the region-sector level. The dependent variable in columns 1-2 (Entry^{Pre}) is the log of entry rates at $t - 1$; in columns 3-4 (ΔEntry^{Pre}) is the log difference in entry rates between $t - 3$ and $t - 1$. Columns 5-6 report the treatment effects of the Seal on subsequent growth of entry at the region-sector level. The dependent variable ($\Delta\text{Entry}^{Post}$) is the log difference in entry rates between $t - 1$ and $t + 1$. All regressions include year and NUTS3 \times NACE fixed effects. Standard errors are robust and clustered at the NUTS3 and NACE level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E The signal of the SOE

Our results indicate that SOE certifications do not lead to any improvement in firm performance. One possible explanation behind these findings is that SOEs are just not salient in the market, and therefore private investors do not respond to the signal as they are simply unaware of its existence. While it not easy to assess the strength of the SOE, we can reasonably rule out that the results are driven by the fact that the European private and public equity community is not aware of the certificate. For instance, after receiving the SOE, firms are listed on a website called [Euroquity.com](https://euroquity.com). This is an online matchmaking platform for the main EU business angels, VCs, and corporate investors maintained by Bpifrance (the French public investment bank), with a special section dedicated to SOE-awarded companies. Additionally, SOE recipients are often selected to participate in matchmaking events and pitching sessions with representatives of crowdfunding portals, business angels, venture capital, and private equity funds (e.g. [1], [2], [3], [4], [5]).

Fig.E1: SOE on VC funds' websites

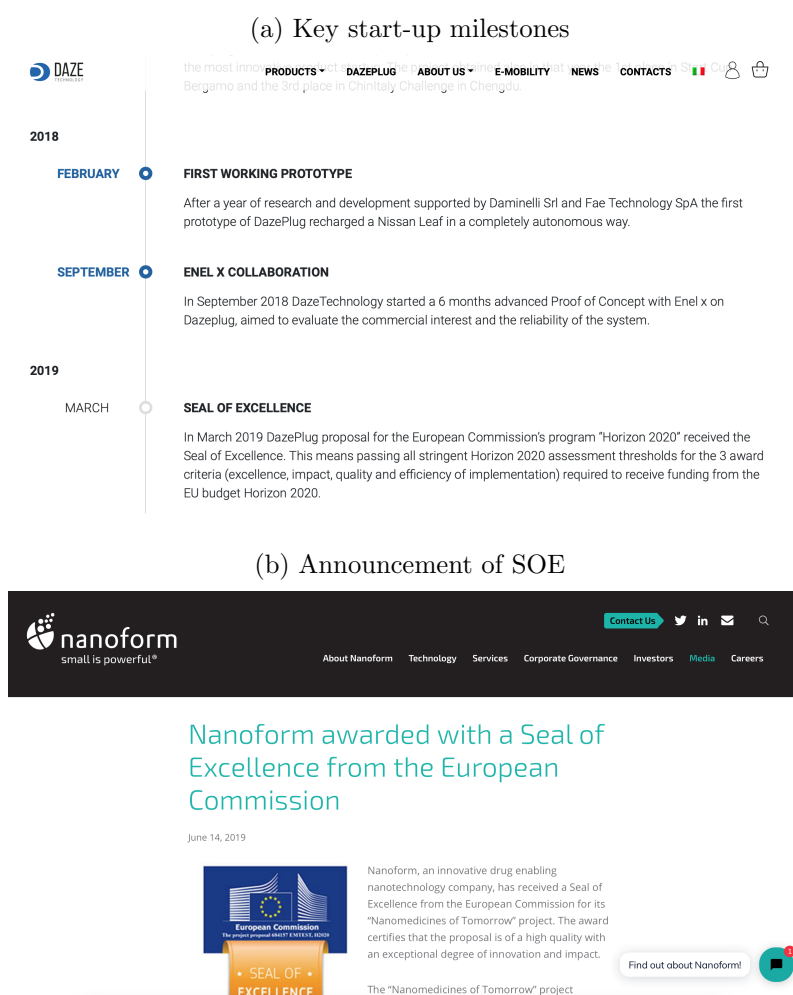


Source: <https://www.portugalventures.pt/en/portfolio/clever/>.

It is also plausible to expect that, as they have invested in companies that won the grant, these same funds are aware of the existence of the SOE. For instance, some venture capital funds' websites display the SOE as a distinguished feature of their portfolio firms. One example

is Portugal Ventures, a venture capital firm that invests in seed rounds of Portuguese startups (see Figure E1).

Fig. E2: SOE on companies' websites

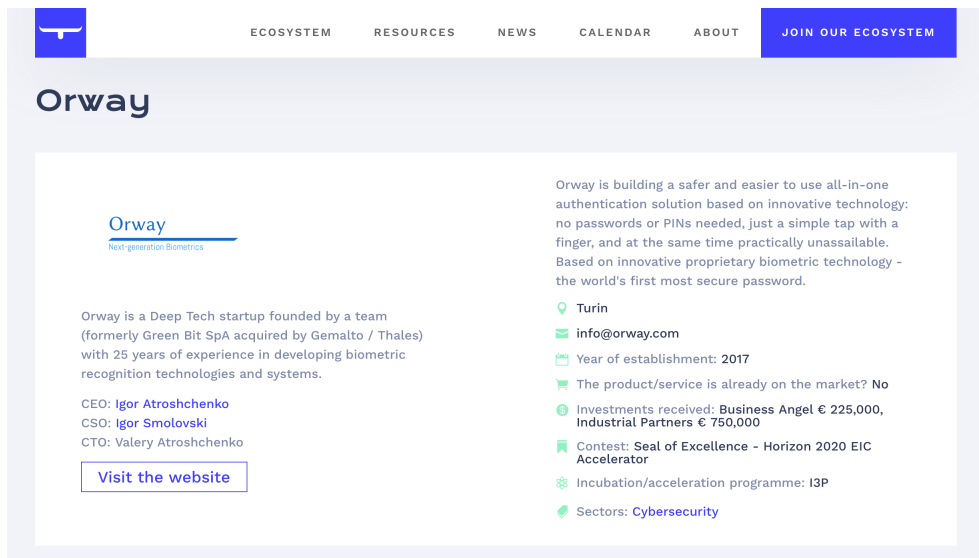


Source: <https://www.dazetechnology.com/company/> and <https://nanoform.com/en/nanoform-awarded-with-a-seal-of-excellence-from-the-european-commission/>.

Concerning the use of the SOE by recipients, though we do not have a specific figure concerning the percentage of firms that list their certificate on their web pages or share it through their social media accounts, there are many different companies that have done so in the past. Figure E2 provides two examples of companies reporting this information on their webpages. Figure E3 reports an example of a company profile on a start-up platform mentioning the SOE along with investments received by business angels and equity funds. Other examples of com-

panies advertising their SOE award via their websites or social media accounts are e.g. [1], [2], [3], [4], [5], [6], [7], [8], [9], [10], [11], [12], [13], [14], [15], [16], [17]. These suggest that being awarded a SOE is considered as an important milestone by early-stage companies, which should hopefully increase their visibility towards external investors.

Fig. E3: Company profile on start-up platform



Orway
Next-generation Biometrics

Orway is a Deep Tech startup founded by a team (formerly Green Bit SpA acquired by Gemalto / Thales) with 25 years of experience in developing biometric recognition technologies and systems.

CEO: **Igor Atroshchenko**
CSO: **Igor Smolovski**
CTO: **Valery Atroshchenko**

[Visit the website](#)

Orway is building a safer and easier to use all-in-one authentication solution based on innovative technology: no passwords or PINs needed, just a simple tap with a finger, and at the same time practically unassailable. Based on innovative proprietary biometric technology - the world's first most secure password.

- Turin**
- info@orway.com**
- Year of establishment: 2017**
- The product/service is already on the market? No**
- Investments received: Business Angel € 225,000, Industrial Partners € 750,000**
- Contest: Seal of Excellence - Horizon 2020 EIC Accelerator**
- Incubation/acceleration programme: I3P**
- Sectors: Cybersecurity**

Source: <https://torinotechmap.it/en/ecosistema/orway/>.

F Benchmark with private sector patent-to-R&D ratio

In order to benchmark the performance of the SME Instrument, we provide an illustrative comparison between the patents per R&D euro generated by the program with the patents per R&D euro generated by the private sector. Data concerning R&D expenditures and patent applications of the US private sector are sourced from the Business R&D and Innovation Survey (BRDIS). The BRDIS is the primary source of information on domestic R&D expenditures for companies operating in the US and it is conducted by the National Science Foundation in conjunction with the US Census Bureau. We consider the time horizon 2010-2016 as data after 2016 do not include companies with 5-9 employees. Table F1 shows that US companies generate an average of 0.42 patent applications every million \$US of R&D (or 0.48 every million €). However, as our sample is composed by SMEs (i.e. companies with less than 250 employees), a more meaningful comparison would involve looking at the patent-to-R&D ratio of smaller companies. In line with empirical evidence reporting a negative relation between firm size and the patent-to-R&D ratio (Acs and Audretsch, 1991; Bound et al., 2007), data indicate that their average ratio is indeed somewhat higher for SMEs and around 0.60 applications every million \$US of R&D (or 0.69 every million €). To provide an additional benchmark, as the grant program examined in our study is mostly implemented in European countries, we compute the patent-to-R&D ratio for EU-28 economies. Differently from the statistics on R&D expenditures performed by the private sector, data on patent applications by private companies located in EU countries are less readily available. Eurostat reports the number patent applications by private companies to the European Patent Office up until 2011. Nonetheless, to have a benchmark for the UE as well, we use the available data and report them in Table F2. The patent-to-R&D ratio appears to be lower than that of the US, reaching on average 0.33 patent applications every million \$US of R&D (or 0.29 every million €). Unfortunately, the available data do not allow calculating the patent-to-R&D ratio for companies of different sizes.

Table F1: Patent-to-R&D ratio for the US private sector

	2010	2011	2012	2013	2014	2015	2016	Mean
All companies								
Applications	141.544	128.960	131.775	149.818	125.892	129.562	135.702	134.750
R&D (mln \$)	290.680	294.093	302.250	322.528	340.728	355.821	374.685	325.826
Patent-to-R&D ratio (mln \$)	0,49	0,44	0,44	0,46	0,37	0,36	0,36	0,42
Patent-to-R&D ratio (mln €)	0,56	0,50	0,50	0,53	0,42	0,42	0,42	0,48
SMEs								
Applications	46.704	35.758	29.537	32.201	13.523	11.502	11.386	25.802
R&D (mln \$)	47.918	43.838	38.698	40.814	42.570	44.028	40.374	42.606
Patent-to-R&D ratio (mln \$)	0,97	0,82	0,76	0,79	0,32	0,26	0,28	0,60
Patent-to-R&D ratio (mln €)	1,12	0,94	0,88	0,91	0,37	0,30	0,32	0,69
Large								
Applications	94.840	93.202	102.238	117.615	112.368	118.061	124.316	108.949
R&D (mln \$)	242.762	250.255	263.553	281.715	298.158	311.793	334.311	283.221
Patent-to-R&D ratio (mln \$)	0,39	0,37	0,39	0,42	0,38	0,38	0,37	0,39
Patent-to-R&D ratio (mln €)	0,45	0,43	0,45	0,48	0,43	0,44	0,43	0,44

Notes: data are sourced from the Business R&D and Innovation Survey (BRDIS), available at <https://www.nsf.gov/statistics/srvyberd/>. Data after 2016 are not reported as they do not include companies with 5-9 employees. R&D figures (in million \$US) refer to domestic R&D performed by US companies. Applications is the total number of patent applications of companies located in the US that performed or funded R&D. SMEs are companies with 249 or fewer employees. Large companies are those with 250 or more.

Table F2: Patent-to-R&D ratio for the EU-28 private sector

	2005	2006	2007	2008	2009	2010	2011	Mean
All companies								
Applications	50.153	50.935	50.881	48.807	48.077	48.239	40.379	48.210
R&D (mln €)	127.106	137.160	146.038	151.695	146.630	152.776	164.189	146.513
Patent-to-R&D ratio (mln €)	0,39	0,37	0,35	0,32	0,33	0,32	0,25	0,33
Patent-to-R&D ratio (mln \$)	0,34	0,32	0,30	0,28	0,29	0,27	0,21	0,29

Notes: data are sourced from the Eurostat Science and Technology database, available at <https://ec.europa.eu/eurostat/web/science-technology-innovation/data/database>. R&D figures (in million €) refer to R&D expenditures performed by the business enterprise sector (BERD) in EU-28 countries. Applications is the total number of patent applications to the European Patent Office by the business enterprise sector in EU-28 countries.

G Robustness: Alternative fixed effects and standard error adjustment

In our baseline estimation approach we include competition fixed effects which control for differences across sectoral topics. This approach might fail to capture more granular differences across sectors. In order to test the sensitivity of our results, we augment the models by including a larger number of fixed effects such as competition, sector (two-digit NACE), cohort, and country dummies. We also include dummies for firms with multiple applications and for those firms winning Phase I grants. Results shown in Table G1 are largely unaltered. We also tested the robustness of our results to alternative standard errors clustering choices. Lee and Card, 2008 argue that in presence of a discrete running variable one should use standard errors clustered at each of the different values taken by the rank. However, Kolesár and Rothe, 2018 recommend against using confidence intervals based on standard errors clustered at the rank-level by providing theoretical and empirical evidence showing that they do not guard against model misspecification and that they have poor coverage properties. Instead, they argue that Eicker-White heteroskedasticity-robust standard errors not clustered by rank generally have better coverage properties. Following this recommendation we employ Eicker-White heteroskedasticity-robust standard errors and obtain similar patterns of statistical significance (Table G2). Results are confirmed when using standard errors clustered at the rank-level or at the firm-level.

Table G1: Alternative fixed effect structure

	(1)	(2)	(3)	(4)	(5)	(6)
Patents^{Post}	All	All	±10	±10	±5	±5
Grant	0.203*** (0.068)	0.221*** (0.069)	0.147* (0.085)	0.120 (0.090)	0.314*** (0.113)	0.249** (0.117)
N	11095	10681	1822	1819	1050	1023
# FE	1	6	1	6	1	6
R-squared	0.36	0.41	0.45	0.47	0.51	0.56
Private Equity^{Post}	All	All	±10	±10	±5	±5
Grant	0.070** (0.028)	0.072** (0.028)	0.080*** (0.027)	0.074** (0.030)	0.117*** (0.039)	0.081* (0.042)
N	8352	8153	1358	1308	784	743
# FE	1	6	1	6	1	6
R-squared	0.07	0.11	0.17	0.27	0.27	0.39
Investment^{Post}	All	All	±10	±10	±5	±5
Grant	0.437*** (0.129)	0.357*** (0.118)	0.481*** (0.169)	0.394** (0.165)	0.677*** (0.224)	0.746*** (0.231)
N	6873	6843	1241	1215	698	665
# FE	1	6	1	6	1	6
R-squared	0.05	0.21	0.20	0.40	0.26	0.52
Assets^{Post}	All	All	±10	±10	±5	±5
Grant	0.561*** (0.065)	0.533*** (0.062)	0.477*** (0.095)	0.363*** (0.096)	0.545*** (0.138)	0.284* (0.152)
N	7306	7269	1311	1286	743	711
# FE	1	6	1	6	1	6
R-squared	0.77	0.79	0.74	0.80	0.74	0.83
Employees^{Post}	All	All	±10	±10	±5	±5
Grant	0.330*** (0.062)	0.301*** (0.061)	0.283*** (0.081)	0.211*** (0.080)	0.242** (0.120)	0.205* (0.123)
N	5493	5465	962	938	548	514
# FE	1	6	1	6	1	6
R-squared	0.79	0.81	0.80	0.85	0.83	0.90
Revenues^{Post}	All	All	±10	±10	±5	±5
Grant	0.489*** (0.136)	0.457*** (0.138)	0.283* (0.146)	0.269 (0.168)	0.190 (0.206)	0.240 (0.249)
N	5119	5100	867	840	480	443
# FE	1	6	1	6	1	6
R-squared	0.77	0.79	0.78	0.82	0.80	0.85
Failure^{Post}	All	All	±10	±10	±5	±5
Grant	-0.044*** (0.012)	-0.044*** (0.012)	-0.033** (0.016)	-0.043** (0.017)	-0.040 (0.024)	-0.052* (0.031)
N	11402	10985	1872	1804	1077	1026
# FE	1	6	1	6	1	6
R-squared	0.03	0.11	0.12	0.21	0.19	0.32

Notes: results obtained using different fixed effects. Odd columns contain only competitions fixed effects as in our benchmark specification, whereas even columns add country, cohort, sector fixed effects, a dummy for firms with multiple applicants, and a dummy for those firms that won a Phase I grant. All regressions include the pre-grant dependent variable and linear polynomials of the running variable on both sides of the threshold. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table G2: Alternative standard errors adjustment

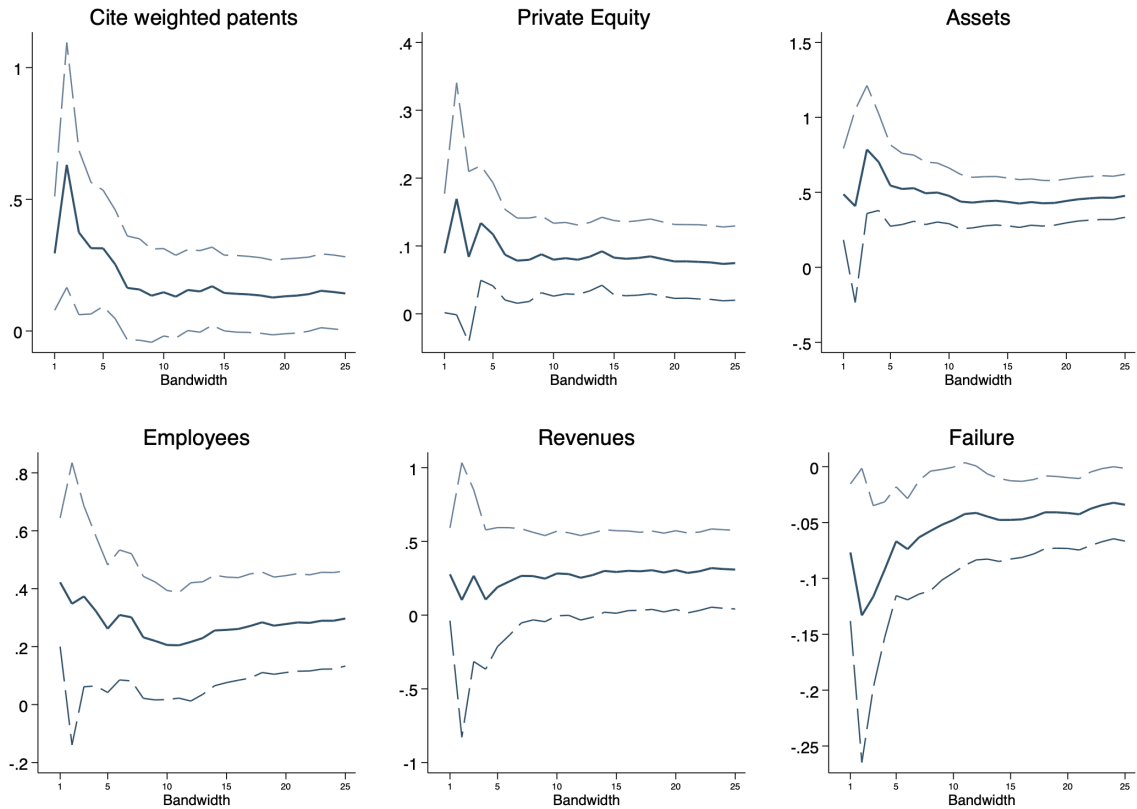
Patents^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	0.203*** (0.068)	0.203*** (0.071)	0.147* (0.085)	0.147* (0.085)	0.314*** (0.113)	0.314*** (0.113)
N	11095	11095	1822	1822	1050	1050
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.36	0.36	0.45	0.45	0.51	0.51
Private Equity^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	0.070** (0.028)	0.070** (0.027)	0.080*** (0.027)	0.080*** (0.029)	0.117*** (0.039)	0.117*** (0.040)
N	8352	8352	1358	1358	784	784
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.07	0.07	0.17	0.17	0.27	0.27
Investment^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	0.437*** (0.129)	0.437*** (0.135)	0.481*** (0.169)	0.481*** (0.163)	0.677*** (0.224)	0.677*** (0.237)
N	6873	6873	1241	1241	698	698
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.05	0.05	0.20	0.20	0.26	0.26
Assets^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	0.561*** (0.065)	0.561*** (0.067)	0.477*** (0.095)	0.477*** (0.096)	0.545*** (0.138)	0.545*** (0.150)
N	7306	7306	1311	1311	743	743
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.77	0.77	0.74	0.74	0.74	0.74
Employees^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	0.330*** (0.062)	0.330*** (0.061)	0.283*** (0.081)	0.283*** (0.082)	0.242** (0.120)	0.242** (0.114)
N	5493	5493	962	962	548	548
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.79	0.79	0.80	0.80	0.83	0.83
Revenues^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	0.489*** (0.136)	0.489*** (0.127)	0.283* (0.146)	0.283* (0.152)	0.190 (0.206)	0.190 (0.232)
N	5119	5119	867	867	480	480
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.77	0.77	0.78	0.78	0.80	0.80
Failure^{Post}	(1) All	(2) All	(3) ±10	(4) ±10	(5) ±5	(6) ±5
Grant	-0.044*** (0.012)	-0.044*** (0.012)	-0.048*** (0.017)	-0.048*** (0.017)	-0.067** (0.031)	-0.067** (0.031)
N	11402	11402	1787	1787	1011	1011
SE	Comp.	EHW	Comp.	EHW	Comp.	EHW
R-squared	0.03	0.03	0.10	0.10	0.14	0.14

Notes: results obtained using different standard errors clustering. Odd columns contain estimates with standard errors clustered at the competition-level as in the baseline models. Even columns report results obtained using Eicker-Huber-White heteroskedasticity-robust standard errors. All regressions include the pre-grant dependent variable and linear polynomials on both sides of the threshold. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

H Robustness: Alternative bandwidths

The baseline approach uses two different bandwidths around the threshold. To test the sensitivity of our results to bandwidth choice, we run the models varying the bandwidth between 1 and 25 on both sides of the thresholds. Results are displayed in Figure H1 and show that the estimates are consistently above zero for all firm outcomes variables but revenues, which is reassuring of the robustness of our main findings. Additionally, we report results from specifications where we limit the sample to the two narrowest bandwidths around the threshold (i.e. ± 2 and ± 1 centered ranks) without controlling for rank or pre-grant dependent variables. Table H1 shows that point estimates and patterns of statistical significance are in line with the baseline results.

Fig.H1: Point estimates by bandwidth



Notes: results obtained by estimating equation (1) with varying bandwidths from ± 1 to ± 25 . All regressions include linear ranks on both sides, pre-grant dependent variable, competition fixed effects. Standard errors clustered at the competition-level. 95% confidence intervals reported.

Table H1: Estimates using narrow bandwidths around the threshold

	Patents				Private Equity			
	± 2	± 2	± 1	± 1	± 2	± 2	± 1	± 1
Grant	0.190** (0.095)	0.221*** (0.081)	0.259* (0.130)	0.295*** (0.110)	0.057** (0.028)	0.053* (0.028)	0.089* (0.046)	0.089* (0.045)
Pre-grant	No	Yes	No	Yes	No	Yes	No	Yes
N	478	478	194	194	322	322	112	112
R-squared	0.50	0.64	0.62	0.74	0.43	0.47	0.50	0.54
	Investment				Assets			
	± 2	± 2	± 1	± 1	± 2	± 2	± 1	± 1
Grant	0.699*** (0.159)	0.669*** (0.161)	0.619*** (0.227)	0.675*** (0.246)	0.521*** (0.194)	0.620*** (0.102)	0.728** (0.232)	0.488*** (0.155)
Pre-grant	No	Yes	No	Yes	No	Yes	No	Yes
N	289	270	108	98	298	298	110	110
R-squared	0.41	0.41	0.49	0.50	0.48	0.60	0.47	0.68
	Employees				Revenues			
	± 2	± 2	± 1	± 1	± 2	± 2	± 1	± 1
Grant	0.275*** (0.093)	0.242*** (0.086)	0.230* (0.132)	0.244* (0.137)	0.631*** (0.211)	0.389** (0.180)	0.283* (0.159)	0.277* (0.161)
Pre-grant	No	Yes	No	Yes	No	Yes	No	Yes
N	181	181	62	62	153	153	44	44
R-squared	0.49	0.59	0.58	0.58	0.56	0.68	0.85	0.87

Notes: results obtained estimating a variant of (Eq. 1) without controls for ranks. Regressions are estimated by means of OLS and using the narrowest bandwidths around the threshold (i.e. ± 2 and ± 1 centered ranks). Odd columns include the pre-assignment dependent variable whereas even columns do not. All regressions control for competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

I Robustness: Bias-corrected estimations with data-driven bandwidths

We explore how sensitive our baseline results are to the non-parametric local polynomial estimation method developed by Calonico et al., 2017. Relative to the parametric method, which is based on arbitrarily chosen bandwidths, this approach allows for automatic estimation of optimal bandwidth. We estimate models using a triangular kernel, a linear or quadratic polynomial of the running variable and by clustering standard errors at the competition level. Automatic bandwidth selection is performed using either a symmetric mean squared error optimal bandwidth (i.e. bandwidths are the same on each side of the threshold) or an asymmetric mean squared error optimal bandwidth (i.e. bandwidths are different on each side). These optimal bandwidths are generally larger than our fixed bandwidth of ± 10 ranks, indicating that our baseline models trade off higher variance in favor of lower bias. Results reported in Appendix Tables I1 and I2 show that point estimates and patterns of statistical significance are very similar to the ones obtained with parametric models.

Table II: Robust bias-corrected local polynomial estimates with data-driven bandwidths

Patents^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	0.161**	0.245**	0.171***	0.250**
Robust SE	0.108	0.154	0.101	0.145
Robust p-value	0.025	0.058	0.019	0.007
N left	2304	2632	4178	7144
N right	468	468	467	467
Order Poly.	1	2	1	2
BW left	18.2	21.8	37.8	78.0
BW right	18.2	21.8	15.0	15.0
Private Equity^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	0.087***	0.119***	0.087***	0.112***
Robust SE	0.039	0.057	0.036	0.053
Robust p-value	0.002	0.071	0.003	0.006
N left	1689	2151	4179	5643
N right	377	377	376	376
Order Poly.	1	2	1	2
BW left	18.2	24.1	55.2	88.4
BW right	18.2	24.1	15.0	15.0
Investment^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	0.442***	0.490**	0.383***	0.354*
Robust SE	0.194	0.305	0.201	0.313
Robust p-value	0.021	0.055	0.077	0.094
N left	1395	1610	3882	4830
N right	332	332	331	331
Order Poly.	1	2	1	2
BW left	17.0	19.0	60.9	86.3
BW right	17.0	19.0	14.0	14.0
Assets^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	0.469***	0.533***	0.459***	0.488***
Robust SE	0.101	0.156	0.086	0.142
Robust p-value	0.000	0.000	0.000	0.000
N left	1307	1627	3291	5032
N right	350	350	349	349
Order Poly.	1	2	1	2
BW left	14.9	18.1	44.2	84.1
BW right	14.9	18.1	14.0	14.0

Notes: results obtained using the estimator developed by Calonico et al., 2017. Odd columns report estimations using the mean-square error bandwidth (i.e. MSE-I) while even columns use the asymmetric mean-square error bandwidth (i.e. MSE-II). All regressions include the pre-grant dependent variable. Standard errors are robust and clustered at the competition level. Asterisks denote conventional p – values while robust standard errors and robust p – values are reported in the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table I2: Robust bias-corrected local polynomial estimates with data-driven bandwidths

Employees^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	0.276***	0.213**	0.272***	0.200***
Robust SE	0.058	0.088	0.048	0.072
Robust p-value	0.000	0.015	0.000	0.005
N left	968	1381	2615	4046
N right	259	259	258	258
Order Poly.	1	2	1	2
BW left	14.1	21.8	48.3	96.7
BW right	14.1	21.8	14.0	14.0
Revenues^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	0.288***	0.407**	0.349***	0.420**
Robust SE	0.168	0.241	0.167	0.218
Robust p-value	0.012	0.262	0.012	0.062
N left	1049	1213	1742	3383
N right	234	234	233	233
Order Poly.	1	2	1	2
BW left	17.9	21.0	31.2	78.3
BW right	17.9	21.0	14.0	14.0
Failure^{Post}	(1) MSE-I	(2) MSE-I	(3) MSE-II	(4) MSE-II
RD_Estimate	-0.036***	-0.040**	-0.031***	-0.029**
Robust SE	0.016	0.024	0.011	0.016
Robust p-value	0.008	0.277	0.010	0.094
N left	2019	2252	4758	7424
N right	479	479	478	478
Order Poly.	1	2	1	2
BW left	15.6	17.8	42.3	79.8
BW right	15.6	17.8	15.0	15.0

Notes: results obtained using the estimator developed by Calonico et al., 2017. Odd columns report estimations using the mean-square error bandwidth (i.e. MSE-I) while even columns use the asymmetric mean-square error bandwidth (i.e. MSE-II). All regressions include the pre-grant dependent variable. Standard errors are robust and clustered at the competition level. Asterisks denote conventional p -values while robust standard errors and robust p -values are reported in the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

J Robustness: Local randomization approach

When the running variable is discrete, a natural alternative to the standard RD methodology is the local randomization approach developed by Cattaneo et al., 2015. This formalizes the idea that RD designs behave like randomized experiments near the threshold by imposing explicit randomization-type assumptions that are stronger than the standard continuity-type conditions. However, estimation and inference based on large-sample approximations may be invalid where the sample size in a narrow bandwidth around the threshold is small. Cattaneo et al., 2015 propose a local randomization approach that leads to finite sample inference methods, which remain valid and can be used even with a handful of observations very close to the threshold. The approach assumes that a window exists around the threshold in which treatment is assigned as in a random experiment. When the running variable is discrete, as in our case, the choice of this window is straightforward: the two mass points on either side of the threshold. Hence, we take firms with centered ranks -1 and 1 and run our local randomization approach. A valid local randomization requires the absence of any systematic difference in predetermined covariates between treated and untreated firms. Appendix Table J1 shows that the difference-in-means between firms ranked -1 and 1 for each covariate is indistinguishable from zero, thus confirming the validity of the approach. Results are reported in Appendix Table J2 and largely corroborate our main findings.

Table J1: Local Randomization analysis for pre-determined covariates

	(1) Patents ^{Pre}	(2) PE ^{Pre}	(3) Invest ^{Pre}	(4) Assets ^{Pre}	(5) Employees ^{Pre}	(6) Revenues ^{Pre}	(7) Age ^{Pre}
Grant	0.012 [0.964]	-0.020 [0.748]	-0.046 [0.638]	-0.0004 [0.998]	-0.076 [0.696]	-0.065 [0.864]	-0.002 [0.982]
N	257	194	182	184	129	120	262

Notes: results obtained using the Local Randomization approach of Cattaneo et al., 2015 to test for differences in predetermined covariates between treated and untreated firms. All firms ranking just above and below the threshold (i.e. ranks -1 and 1) are included. Tables report difference-in-means and Fisherian p-values for finite sample inference in brackets. Estimates obtained using 1000 replications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table J2: Local Randomization estimates

	(1) Patents ^{Post}	(2) PE ^{Post}	(3) Invest ^{Post}	(4) Assets ^{Post}	(5) Employees ^{Post}	(6) Revenues ^{Post}	(7) Failure ^{Post}
Grant	0.227* [0.087]	0.115*** [0.002]	0.602*** [0.000]	0.575*** [0.000]	0.216 [0.246]	0.221 [0.472]	-0.057** [0.011]
N	257	194	182	184	129	120	262

Notes: results obtained using the Local Randomization approach of Cattaneo et al., 2015. All firms ranking just above and below the threshold (i.e. ranks -1 and 1) are included. Tables report difference-in-means and Fisherian p-values for finite sample inference in brackets. Estimates obtained using 1000 replications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

K Robustness: Difference-in-differences

The availability of a longitudinal dataset gives us the possibility to test whether our results hold when combining difference-in-differences (DID) with the RD design (Frandsen, 2014). This is useful in several ways. First of all, the larger number of observations generates an increase in statistical power. Second, the DID allows us to control for group-specific unobserved characteristics or, alternatively, for firm unobserved heterogeneity. We collapse data to two observations per firm, one before and one after the competition, and run the following equation using all firms (i.e. infinite bandwidth) as well as those closer to the threshold (i.e. bandwidths of ± 10 and ± 5):

$$Y_{i,c,t} = \gamma Grant_{i,c} + \delta Post_{i,c,t} + \beta Grant_{i,c} \times Post_{i,c,t} + f(Rank_{i,c}) + X_{ic}\Gamma + \varepsilon_{i,c,t} \quad (2)$$

where $Y_{i,c,t}$ is an outcome variable for firm i , in competition c , at time t . $Grant_{i,c}$ is a time-invariant dummy variable indicating whether firm i is awarded a grant in competition c , $Post_{i,c,t}$ is an indicator variable for the post-program period, and $Grant_{i,c} \times Post_{i,c,t}$ is equal to 1 for those observations in the treatment group in the post-program period and represents the coefficient of interest. All regressions control for linear polynomials of the running variable separately on each side of the threshold. Additionally, X_i is a vector of firm-level covariates encompassing competition, country, sector (2-digit NACE rev. 2), cohort, and time fixed effects. We also run different specifications including firm-by-application fixed effects, which control for unobserved firm heterogeneity.⁵⁰ Standard errors are clustered at the competition-level, though results hold when using either firm or rank clustering. Tables K2 and K1 report the results. Point estimates are positive and significant for assets, employees, revenues, as well as for the number of cite-weighted patents and the amount of private equity received. Hence, this represents corroboratory evidence and indicates that time-invariant firm differences do not drive the findings reported in previous sections.

⁵⁰ This is done to accommodate the presence of firms that may apply more than once, and it amounts to consider each application made by a firm as a different unit following Cellini et al., 2010.

Table K1: Difference-in-differences

	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Panel A: Patents							
Grant \times Post	0.312*** (0.055)	0.318*** (0.056)	0.311*** (0.055)	0.182*** (0.062)	0.180*** (0.060)	0.137* (0.070)	0.140** (0.066)
Rank \times Grant	Yes	Yes	No	Yes	No	Yes	No
Sector FE	No	Yes	No	Yes	No	Yes	No
Cohort FE	No	Yes	No	Yes	No	Yes	No
Competition FE	Yes	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No
Firm-application FE	No	No	Yes	No	Yes	No	Yes
N	22193	21375	22190	3555	3644	2051	2102
# competitions	176	176	176	176	176	176	176
R-squared	0.11	0.27	0.96	0.39	0.96	0.48	0.96
	(1) All	(2) All	(3) All	(4) ± 10	(5) ± 10	(6) ± 5	(7) ± 5
Panel B: Private Equity							
Grant \times Post	0.242** (0.105)	0.243** (0.107)	0.242** (0.105)	0.235** (0.114)	0.235** (0.110)	0.208 (0.137)	0.207 (0.129)
Rank \times Grant	Yes	Yes	No	Yes	No	Yes	No
Sector FE	No	Yes	No	Yes	No	Yes	No
Cohort FE	No	Yes	No	Yes	No	Yes	No
Competition FE	Yes	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No
Firm-application FE	No	No	Yes	No	Yes	No	Yes
N	16704	16320	16704	2676	2716	1562	1584
# competitions	176	176	176	176	176	176	176
R-squared	0.03	0.11	0.84	0.23	0.82	0.36	0.79

Notes: two-period DID estimations using different specifications of equation (2) by means of OLS. The dependent variables are the average (log) of patents (or private equity amount) for the 10 years preceding the treatment and the average (log) of patents (or private equity amount) for the 5 years after treatment. All specifications include the standalone Grant and Post variables as well as linear controls for centered ranks on both sides of the threshold and year fixed effects. Columns 1 to 4 use infinite bandwidths whereas columns 4-5 and 6-7 employ bandwidths of 10 and 5, respectively. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table K2: Difference-in-differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	All	All	± 10	± 10	± 5	± 5
Panel A: Revenues							
Grant \times Post	0.375*** (0.090)	0.349*** (0.076)	0.279*** (0.071)	0.243** (0.098)	0.225*** (0.081)	0.295*** (0.112)	0.220** (0.095)
Rank \times Grant	Yes	Yes	No	Yes	No	Yes	No
Sector FE	No	Yes	No	Yes	No	Yes	No
Cohort FE	No	Yes	No	Yes	No	Yes	No
Competition FE	Yes	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No
Firm-application FE	No	No	Yes	No	Yes	No	Yes
N	12281	12235	11064	2039	1852	1163	1058
# competitions	175	175	175	175	174	167	166
R-squared	0.07	0.48	0.95	0.62	0.94	0.69	0.94
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	All	All	± 10	± 10	± 5	± 5
Panel B: Assets							
Grant \times Post	0.476*** (0.053)	0.438*** (0.050)	0.422*** (0.049)	0.395*** (0.061)	0.387*** (0.053)	0.390*** (0.073)	0.409*** (0.062)
Rank \times Grant	Yes	Yes	No	Yes	No	Yes	No
Sector FE	No	Yes	No	Yes	No	Yes	No
Cohort FE	No	Yes	No	Yes	No	Yes	No
Competition FE	Yes	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No
Firm-application FE	No	No	Yes	No	Yes	No	Yes
N	16888	16807	15848	2913	2786	1661	1594
# competitions	176	176	175	176	175	176	175
R-squared	0.06	0.38	0.95	0.50	0.94	0.57	0.93
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	All	All	± 10	± 10	± 5	± 5
Panel C: Employees							
Grant \times Post	0.135*** (0.042)	0.179*** (0.038)	0.177*** (0.031)	0.171*** (0.043)	0.167*** (0.035)	0.151*** (0.058)	0.192*** (0.045)
Rank \times Grant	Yes	Yes	No	Yes	No	Yes	No
Sector FE	No	Yes	No	Yes	No	Yes	No
Cohort FE	No	Yes	No	Yes	No	Yes	No
Competition FE	Yes	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No
Firm-application FE	No	No	Yes	No	Yes	No	Yes
N	13852	13731	11950	2378	2076	1365	1204
# competitions	175	175	175	175	174	172	171
R-squared	0.05	0.37	0.96	0.51	0.96	0.58	0.96

Notes: two-period DID estimations using different specifications of equation (2) by means of OLS. The dependent variables are computed as the average (log) between time $t - 2$ and $t - 1$ for the period preceding the treatment and the average (log) between time t and $t + 1$ for the treatment period. All specifications include the standalone Grant and Post variables as well as linear controls for centered ranks on both sides of the threshold and year fixed effects. Columns 1 to 4 use infinite bandwidths whereas columns 4-5 and 6-7 employ bandwidths of 10 and 5, respectively. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

L Robustness: Falsification tests

We investigate the presence of discontinuities in firm-level outcome variables away from the true threshold that assigns treatment. Obtaining statistically significant estimates in correspondence with placebo thresholds would cast doubts on the ‘smoothness’ assumption behind the RD design, thus suggesting spurious results. We vary the threshold arbitrarily to test whether the effects are determined by the grant. Instead of considering the centered rank threshold at 0, we use a placebo threshold between ranks 1 and 2 or, alternatively, between rank -2 and -1. To avoid “contamination” from real treatment effects, we restrict observations respectively to treated (untreated) firms for the artificial threshold above (below) the actual cut-off. We run these models for all firm outcomes and find no systematic relationship between the placebo thresholds and firm outcomes (see Table [L1](#)).

Table L1: Placebo tests

	Patents ^{Post}				Private Equity ^{Post}			
	(1) 0;10	(2) -10;0	(3) 0;5	(4) -5;0	(5) 0;10	(6) -10;0	(7) 0;5	(8) -5;0
Placebo Grant (2)	-0.391 (0.253)		-0.204 (0.198)		-0.045 (0.049)		-0.028 (0.080)	
Placebo Grant (-2)		-0.044 (0.086)		-0.032 (0.123)		-0.038* (0.020)		-0.036 (0.035)
N	418	1362	317	685	326	989	242	479
R-squared	0.58	0.47	0.60	0.58	0.35	0.18	0.40	0.30
	Assets ^{Post}				Employees ^{Post}			
	(1) 0;10	(2) -10;0	(3) 0;5	(4) -5;0	(5) 0;10	(6) -10;0	(7) 0;5	(8) -5;0
Placebo Grant (2)	0.084 (0.288)		0.041 (0.498)		0.137 (0.130)		-0.032 (0.195)	
Placebo Grant (-2)		0.105 (0.163)		0.024 (0.307)		-0.061 (0.083)		0.062 (0.161)
N	180	636	122	285	205	704	143	323
R-squared	0.82	0.82	0.81	0.86	0.87	0.81	0.88	0.84
	Revenues ^{Post}				Failure ^{Post}			
	(1) 0;10	(2) -10;0	(3) 0;5	(4) -5;0	(5) 0;10	(6) -10;0	(7) 0;5	(8) -5;0
Placebo Grant (2)	0.062 (0.142)		-0.217 (0.243)		0.003 (0.028)		0.010 (0.044)	
Placebo Grant (-2)		-0.104 (0.136)		0.043 (0.208)		0.020 (0.025)		0.029 (0.039)
N	291	968	202	465	427	1401	324	704
R-squared	0.77	0.77	0.78	0.78	0.27	0.13	0.36	0.21

Notes: results obtained using a placebo threshold between ranks -2 and -1 or, alternatively, between rank 1 and 2. For the placebo threshold above the actual one, estimates are obtained using bandwidths from centered ranks 0 to 10 (or 0 to 5). For the placebo threshold below the actual one, estimates are obtained using bandwidths from centered ranks -10 to 0 (or -5 to 0). All regressions include linear ranks on both sides of the threshold, the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

M Robustness: External validity

Our econometric strategy allows to infer local average treatment effects (LATE) and not average treatment effects (ATE) that can be generalized to the entire population of interest. In other words, the causal impact is obtained by comparing firms near the threshold but do not necessarily refer to the entire number of applicants. Therefore, we test the stability of our RD estimates in order to understand whether the estimated effects can be potentially extended to firms that are marginally away from the threshold. To do so we follow Dong and Lewbel, [2015](#) and Cerulli et al., [2017](#) and compute the treatment effects derivative (TED). The TED is the derivative of the RD treatment effect with respect to the running variable. If TED is large in magnitude and statistically significant, this is evidence of instability and hence a potential lack of external validity. In contrast, having TED near zero provides evidence in support of the stability of RD estimates. We report the TED for our baseline models in Appendix Table [M1](#) and show that point estimates are small in magnitude and not statistically significant, thus reassuring us on the stability and external validity of results.

Table M1: Treatment effects derivatives

Patents^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	-0.013 (0.021)	-0.090 (0.067)	-0.073* (0.038)	-0.130 (0.209)
N	1822	1822	1050	1050
R-squared	0.45	0.45	0.51	0.51
Private Equity^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	-0.011 (0.007)	-0.034 (0.023)	-0.020* (0.012)	-0.017 (0.064)
N	1358	1358	784	784
R-squared	0.17	0.17	0.27	0.27
Investment^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	0.004 (0.035)	0.224 (0.162)	0.123 (0.084)	-0.170 (0.447)
N	1241	1241	698	698
R-squared	0.20	0.20	0.26	0.27
Assets^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	-0.016 (0.021)	0.051 (0.078)	0.054 (0.053)	-0.058 (0.238)
N	1311	1311	743	743
R-squared	0.74	0.74	0.74	0.74
Employees^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	-0.022 (0.017)	0.029 (0.065)	0.018 (0.035)	0.041 (0.181)
N	962	962	548	548
R-squared	0.80	0.80	0.83	0.83
Revenues^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	-0.064** (0.033)	-0.150 (0.142)	-0.107 (0.092)	-0.055 (0.398)
N	867	867	480	480
R-squared	0.78	0.78	0.80	0.80
Failure^{Post}	(1) ±10	(2) ±10	(3) ±5	(4) ±5
TED	0.006 (0.005)	-0.005 (0.015)	0.011 (0.009)	-0.046 (0.047)
N	1872	1872	1077	1077
R-squared	0.12	0.12	0.19	0.19

Notes: reported coefficients are treatment effects derivatives (Dong and Lewbel, 2015; Cerulli et al., 2017). Odd columns report estimates using linear controls for centered ranks whereas even columns report estimates obtained quadratic controls for centered ranks. All regressions include the pre-grant dependent variable and competition fixed effects. Standard errors are robust and clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Supplementary References

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