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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).

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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

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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 Busines 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.

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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.

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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

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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 SME[5], including newly created companies and start-ups; ii) the applicant is established in a EU Member State or a Horizon2020-associated country[6]; 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 (Hünermund 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.

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and application development. Grants cover 70% of all eligible costs of the proposed project[8] 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.

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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

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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 database[15] 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.

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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`).

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

$$
\begin{aligned}
Y_{ic}^{Post} = & \alpha + \beta Grant_{ic} + f\left(Rank_{ic}\right) + \gamma Y_{ic}^{Pre} + \delta_c + \varepsilon_{ic} \\
& \text{where } -r \leq Rank_{ic} \leq r
\end{aligned}
\tag{1}
$$

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 $f\left(Rank_{ic}\right)$ 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 it[20], 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-Huber-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.

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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.

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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.

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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.

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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

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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 BIC[28] (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.

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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).

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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).

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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).

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(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.

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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).

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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.

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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.

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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 revenues[45], 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.

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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 × 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 × 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.

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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

	Mean	SD	p50	N
Panel A: competitions (raw data)				
# 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.

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

Notes: results obtained estimating our baseline RDD equation by means of OLS with predetermined 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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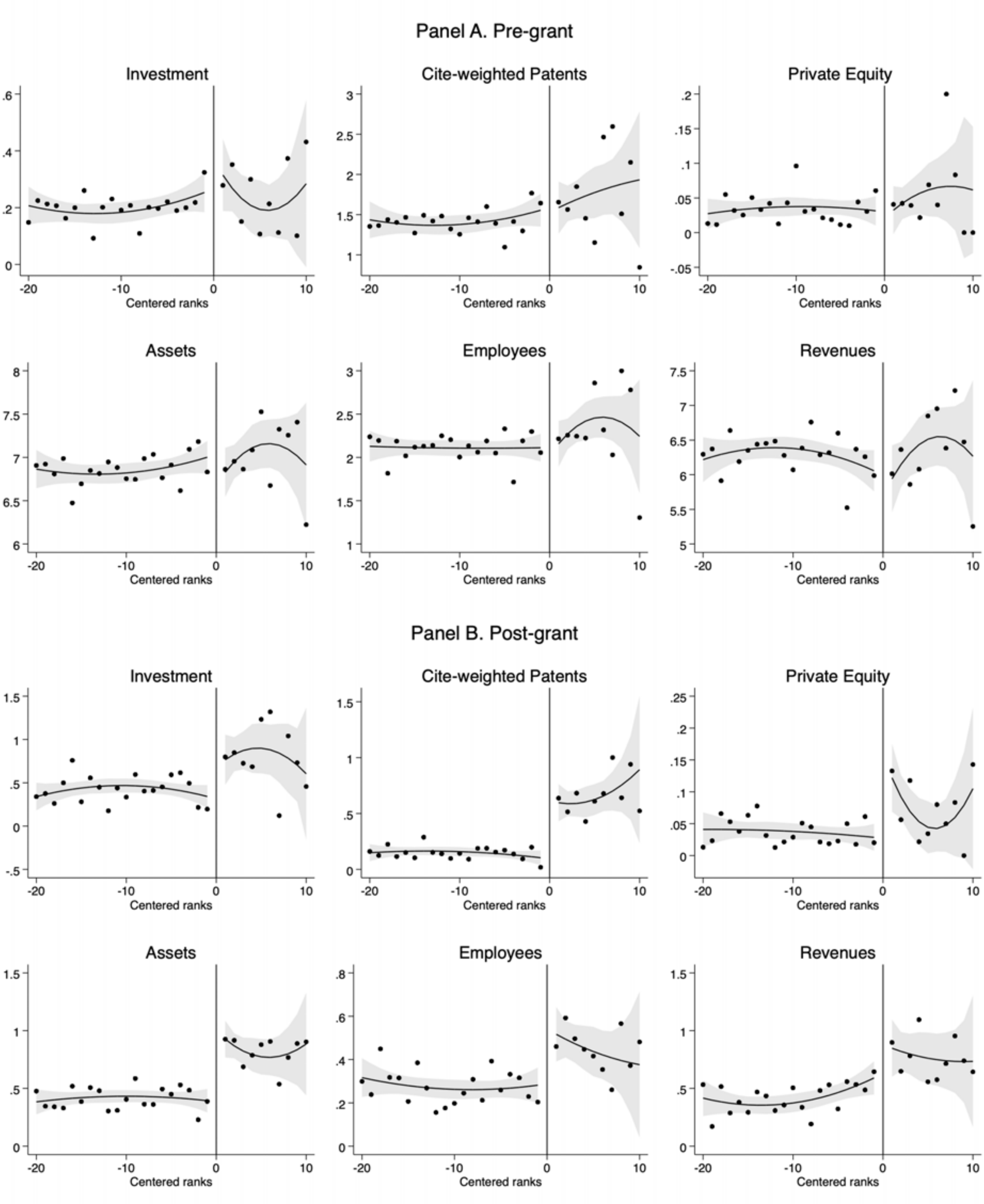
Fig. 1: RDD plots before and after the grant



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.

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Table 3: The effects on investment

	(1) All	(2) All	(3) All	(4) ±10	(5) ±10	(6) ±5	(7) ±5
Grant	0.437***	0.369*	0.388***	0.481***	0.481*	0.677***	1.595***
	(0.129)	(0.211)	(0.090)	(0.169)	(0.274)	(0.224)	(0.524)
Rank × Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank^2 × 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

Panel A: Citw-patents	(1) All	(2) All	(3) All	(4) ±10	(5) ±10	(6) ±5	(7) ±5
Grant	0.203***	0.282**	0.148***	0.147*	0.236*	0.314***	0.390*
	(0.068)	(0.117)	(0.051)	(0.085)	(0.138)	(0.113)	(0.230)
Rank × Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank^2 × 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	(1) All	(2) All	(3) All	(4) ±10	(5) ±10	(6) ±5	(7) ±5
Grant	0.070**	0.126***	0.036**	0.080***	0.123***	0.117***	0.157*
	(0.028)	(0.045)	(0.015)	(0.027)	(0.047)	(0.039)	(0.085)
Rank × Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank^2 × 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table 5: The effects on firm growth

Panel A: Assets	(1) All	(2) All	(3) All	(4) ±10	(5) ±10	(6) ±5	(7) ±5
Grant	0.561***	0.578***	0.437***	0.477***	0.570***	0.545***	1.037***
	(0.065)	(0.099)	(0.050)	(0.095)	(0.150)	(0.138)	(0.321)
Rank × Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank^2 × 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	(1) All	(2) All	(3) All	(4) ±10	(5) ±10	(6) ±5	(7) ±5
Grant	0.330***	0.256***	0.219***	0.283***	0.318**	0.242**	0.234
	(0.062)	(0.092)	(0.038)	(0.081)	(0.132)	(0.120)	(0.222)
Rank × Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank^2 × 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	(1) All	(2) All	(3) All	(4) ±10	(5) ±10	(6) ±5	(7) ±5
Grant	0.489***	0.664***	0.370***	0.283*	0.199	0.190	0.152
	(0.136)	(0.199)	(0.083)	(0.146)	(0.230)	(0.206)	(0.439)
Rank × Grant	Yes	Yes	No	Yes	Yes	Yes	Yes
Rank^2 × 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table 6: SOE recipient firms vs rest of losing firms

	Citw Patents^{Post}		Private Equity^{Post}		Assets^{Post}	
	(1) All	(2) ±10	(3) All	(4) ±10	(5) All	(6) ±10
Seal	0.025	0.037	-0.001	-0.006	0.030	-0.003
	(0.023)	(0.066)	(0.007)	(0.016)	(0.030)	(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) All	(2) ±10	(3) All	(4) ±10	(5) All	(6) ±10
Seal	0.018	0.072	0.076**	0.052	-0.011*	-0.030
	(0.022)	(0.073)	(0.035)	(0.120)	(0.007)	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table 7: Spillovers on entrepreneurship

	Entry^{Pre}		ΔEntry^{Pre}		$\Delta\text{Entry}^{Post}$	
	(1) All	(2) ±10	(3) All	(4) ±10	(5) All	(6) ±10
Grant	-0.013	-0.005	-0.004	-0.000	0.037**	0.060**
	(0.009)	(0.012)	(0.021)	(0.032)	(0.017)	(0.024)
Rank × 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 (ΔEntry^{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 ($\Delta\text{Entry}^{Post}$) is the log difference in entry rates between $t-1$ and $t+1$. All regressions include year and NUTS3 × NACE fixed effects. Standard errors are robust and clustered at the NUTS3 and NACE level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.