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Executive Summary

In recent years, innovative start-ups have gained significant attention in both policy discussions and academic research. These firms are widely recognized for their potential to drive innovation, create jobs, and stimulate economic growth. However, their performance often faces substantial challenges due to market frictions. To address these obstacles, many national and local governments worldwide have implemented programs in their support, resulting in a substantial increase in funding for early-stage ventures. While there is substantial literature evaluating the effectiveness of national-level policies for start-ups, there has been a noticeable gap in assessing the impact of local policies, primarily due to data limitations. This paper provides the first quasi-experimental evidence on the combined effects of local public programs targeting innovative start-ups focusing on Italy, a country that has been particularly active in supporting this type of firms. Between 2012 and 2021, the study identifies 136 different local initiatives disbursing over €500 million through competitive selection processes. Using official sources, we hand-collect data on 2,302 applicants and leverage discontinuities in program design to estimate the causal impact of these programs. The results indicate that these incentives do not lead to improved innovation outcomes. Both the likelihood of patenting and the number of patents remain unaffected, even when considering the quality of patents. Additionally, there is no increased likelihood of attracting external private investment in the form of venture capital for winning firms. To provide a more comprehensive assessment, the paper explores the impact on balance-sheet outcomes, including investment and firm size. The findings suggest that these programs do not boost investment or contribute to an increase in firm size. The only noticeable effect is a temporary reduction in the likelihood of failure, suggesting that these programs lead to crowding out. Surprisingly, the study reveals that while these programs may not effectively enhance productive outcomes, they do increase the chance of securing further public subsidies. Firms that win local policies experience an substantial increase in the probability of receiving subsequent public subsidies. This result hints at a “Matthew effect” where reputation plays a significant role in subsidy allocation, even for firms that have previously received public funding without increasing their commitment in innovative activities. In conclusion, this research emphasizes the

need for more scrutiny over local programs supporting innovative start-ups. Such programs may attract firms seeking to reduce their cost of capital rather than fostering true innovation, while also incentivizing firms to become “subsidy entrepreneurs”, an arguably unproductive form of entrepreneurship. These findings contribute to the literature on entrepreneurship and innovation policies by leveraging program design discontinuities to estimate causal effects. It also sheds light on subsidy interactions across different government levels and their potential implications for future subsidy allocation, providing valuable insights for policymakers and researchers alike.

Spurring Subsidy Entrepreneurs

Pietro Santoleri[†] Emanuele Russo[‡]

Abstract

In the attempt to boost innovation, policy-makers have enacted a myriad of programs targeting innovative start-ups in recent years. Empirical evidence on these initiatives has almost exclusively focused on national-level programs, overlooking those implemented at the local level. This paper provides the first quasi-experimental evidence on the joint effects of local policies focusing on Italy, where regional governments have been very active in providing financial support to these firms. By leveraging discontinuities in program design, we adopt a local randomization approach and document a null effect of these programs over a wide range of firm-level outcomes. However, we find that securing local subsidies increases start-ups' probability to obtain additional public subsidies, which points in the direction of a *vicious* "Matthew effect" in subsidy allocation. Consistent with a reputation/certification mechanism, the increase in follow-on subsidies occurs for funds disbursed at the local level only, whereas no effect is detected for subsidies allocated by national or international authorities.

Keywords: Regression discontinuity design, Innovation Policy, Place-based Policy, Start-ups
JEL: D22, G24, G32, L53, O31, O38, R58

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1 Introduction

In recent years, innovative start-ups have been at the forefront of policy agendas and academic debates. These firms are widely regarded as key contributors to innovation, job creation and economic growth (Haltiwanger et al., 2017). Yet, their performance is often plagued by severe market frictions (Hall, 2010; Kerr and Nanda, 2015). As a result, innovative start-ups have been prime targets for policy initiatives with the specific aim of mitigating the obstacles characterizing their establishment and development. This has translated into a proliferation of national policies across the globe (Audretsch et al., 2020), arguably contributing to the substantial increase in governments' efforts to finance early-stage ventures in the last decades.[1] This policy trend at the national level has been accompanied by a myriad of initiatives implemented by local institutions. While the literature provides several studies addressing the effectiveness of national-level policies targeting start-ups (see, e.g., Autio and Rannikko 2016; Gonzalez-Uribe and Leatherbee 2018; Hottenrott and Richstein 2020; Manaresi et al. 2021), evidence on the effects of local policies has been largely overlooked, arguably due to the difficulty in getting systematic data on these programs (Bai et al., 2021). Assessing the effectiveness of sub-national schemes is important given that they are not mere duplicates of national level programs[2] and in light of the prominent debates concerning smart specialization policies (Foray, 2014) and place-based polices (Barca et al., 2012).

Against this backdrop, this paper provides the first quasi-experimental evidence on the combined effects of local public programs targeting innovative start-ups. To that end, we focus on all policies implemented in Italy, a country that has been very active in providing local support to these firms (Albanese et al., 2019), especially after the promulgation of a national *Start-up Act* in 2012 (Menon et al., 2018). In particular, during the time span 2012-2021, we identified a total of 136 different local initiatives disbursing more than €500 million through grants. Using official sources, we hand-collect data on all local policies that offered monetary incentives through

[1] Bai et al. (2021) report that public financing for early-stage ventures worldwide has gone up from roughly $50 billion in 1995 to more than $170 billion in 2019.

[2] Due to their extensive knowledge of local market conditions and of the possible beneficiaries, local policy-makers may be able to effectively choose projects with high social returns but low private returns. On the contrary, they may fall under the influence of lobbying efforts and fund initiatives that would still be undertaken even without subsidies.

a competitive selection process. This entails a discontinuity in the assignment mechanism as incentives are awarded to all eligible firms following a technical evaluation of their proposals, until funds run out. We retrieve data on 2,302 applicants across 40 local competitions (awarding €45,000 on average), their rankings and funding decisions. Leveraging this setting, we adopt a local randomization inference approach to gauge the causal impact of these programs.

Results document that these incentives do not trigger any additionality in innovation outcomes. In more detail, we find a null effect when considering both the likelihood to file a patent and the number of patents. These results hold even when considering quality-adjusted patents, indicating that these programs do not increase innovation output nor its average quality. We then examine whether local start-up policies increase the likelihood to attract external private investment in the form of venture capital (VC) and private equity. Results show that winners do not enjoy higher chances to do so.

While patenting and external equity are widely used measures to evaluate the impact of programs targeting innovative start-ups, they are not necessarily sufficient to rule out additionality altogether. For instance, incentives might lead to the commercialization of new products and services that are not patented. Likewise, firms might seek finance other than that provided by equity investors, especially in contexts where VC markets tend to be underdeveloped like Italy. To provide additional evidence on the effects of these local policies, we consider the impact on balance-sheet outcomes. In particular, we examine the effects on investment and firm size. The idea is that if these programs do not increase intermediate outcomes (e.g. investment), it is unlikely that they will affect ultimate outcomes (e.g. the introduction of new products and services). Results corroborate this conjecture, as incentives do not boost investment, nor are they conducive to any increase in firm size. The only detectable impact on firm performance is a reduction in failure likelihood, though quite short-lived. In sum, results indicate that these programs lead to crowding out.

While largely ineffective in generating any additionality in firm-level productive outcomes, we do find an impact of these programs: a sizable increase in the chance of securing further public subsidies. Using data on the universe of Italian firms that have received subsidies by either regional or national bodies, we show that the probability of receiving subsequent public subsidies increases by 80% after winning one of these local policies. This result points in the direction of a *vicious* "Matthew effect" in the allocation of subsidies, where persistence in receiving public money is based on sheer reputation, even towards firms that have actually substituted their internal funds with prior public subsidies (Antonelli and Crespi, 2013).[3] Consistent with a reputation/certification effect, we show that the increase in follow-on subsidies occurs for funds disbursed at the local level only, whereas no effect is detected for subsides allocated by central authorities. Similarly, we also show that these schemes do not increase the odds of participating nor winning grants for innovative start-ups awarded by European authorities through the Horizon framework program.

Overall, these results call for more scrutiny over local programs supporting innovative start-ups. These might in fact attract participation from firms featuring a low innovative potential that are simply seeking to reduce their cost of capital rather than secure funding that is not otherwise available. Additionally, apart from being ineffective, these schemes might create incentives for firms to become "subsidy entrepreneurs" (Gustafsson et al., 2020), which could be an unproductive form of entrepreneurship (Baumol, 1996).

The paper adds to the literature leveraging discontinuities in program design to estimate causal effects stemming from entrepreneurship and innovation policies. Howell (2017) and Santoleri et al. (2022) examine the effects of direct public R&D grants towards small and young ventures documenting a sizable impact on several firm-level outcomes. McKenzie (2017), Barrows et al. (2018), and Howell (2020) all find that privately-sponsored grant prizes lead to substantial increases in firm performance. While these studies focus on national or supra-national programs, the literature has also addressed the effects of single local policies. Bronzini and Iachini (2014) focuses on a regional R&D grant scheme in Emilia-Romagna (Italy) and uncover beneficial effects for smaller firms. Accetturo (2022) finds that subsidies for innovative start-ups in Trentino-Alto Adige (Italy) stimulate firm entry but not their innovative potential as measured by patenting activity. Zhao and Ziedonis (2020) examine a Michigan-based program finding positive effects on survival and external financing, while detecting no change in patent-

[3] On the contrary, we can exclude the presence of a *virtuous* "Matthew effect", that is, when persistence in the provision of public funding concerns firms that have effectively used those resources to increase their performance (Antonelli and Crespi, 2013).

ing. Lanahan and Feldman (2018) report positive effects from the SBIR State Match program in Kentucky and North Carolina. Our paper diverges from the above literature, which looks at a single program at a time, as it provides the first assessment of the combined causal effects stemming from local policies.

The paper also speaks to the limited empirical evidence on subsidy interactions across different government levels (e.g. Lanahan 2016), and to the literature focusing on path-dependency in State aid towards private business firms. Hussinger (2008), Aschhoff (2010), Antonelli and Crespi (2013), Gustafsson et al. (2020), Albanese et al. (2021), among others, all document a strong correlation between past and future receipt of public subsidies using firm-level data from different European countries.[4] The key difference between this study and previous work is that the empirical design allows for a causal interpretation of this result.

2 Institutional setting

Deploying measures to encourage the creation and development of innovative start-ups is a common trait of innovation and entrepreneurial policies around the world. Public intervention in their support is motivated both by their substantial contribution to aggregate economic dynamism and by the need to correct market failures to which innovative start-ups are more subject (Audretsch et al., 2020). As innovation has the characteristics of a public good, market forces alone are not able to guarantee the optimal level of investment in innovation that maximizes social welfare. Furthermore, innovative investments are by nature more complex to evaluate and therefore subject to greater information asymmetries and financial frictions. Moreover, innovative start-ups, due to their young age, cannot exploit a consolidated reputation: barriers to entry and financial restrictions are thus exacerbated for young innovative businesses (see, e.g., Gordon (2018)).

Against this background, the Italian government, along with many others[5], launched a policy

[4] In the US, a long-standing debate concerns new ventures that are capable of repeatedly winning a disproportionate amount of SBIR grants (i.e. "SBIR mills"). This is often regarded as a source of inefficiencies in subsidy allocation as grants awarded to these firms feature decreasing returns (Lerner et al., 1999; Link and Scott, 2010; Howell, 2017).

[5] Examples of national programs supporting innovative start-ups are Start-up Chile, Startup India, the Start-up Plan in Belgium, the Jeunes Entreprises Innovantes in France, the Young Innovative Companies program

framework for innovation-driven entrepreneurship known as the *Start-up Act* in October 2012. The primary goal of this extensive regulatory framework was to create a favorable environment for innovative start-ups during their initial years of operations through a variety of complementary tools such as equity incentives, a public guarantee program, and tax credits for hiring highly skilled employees.[6] After the introduction of the national policy, interventions in favor of innovative start-ups at a local level flourished throughout almost all Italian regions.[7] These policy measures are intended to boost innovation by supporting the establishment and development of start-ups with innovative potential. They range from providing proof-of-concept grants, to the support of young companies and university spin-offs with high technological potential, to support entrepreneurial investment in the early-stage phase, up to the implementation of actions to accompany R&D activities. Albanese et al. (2019), who survey all regional initiatives during the period 2012-2018, find a total of 101 programs, disbursing resources for €515 million. These schemes rely on a set of financial instruments, mostly grants, followed by equity and venture capital funding and subsidized loans.

A noticeable feature of these interventions is that they are frequently co-financed by the European Cohesion Policy through the 2014-2020 Regional Operational Programs (Albanese et al., 2019). In particular, funds are channeled through the European Regional Development Fund

in Finland, and España Nación Emprendedora in Spain. By 2016, 12 countries in the EU had established regulatory frameworks or special statuses for startups (see European Digital Forum, 2016). Additionally, a comprehensive list of international public policies can be found on the Startup Nations Atlas of Policies website at `https://www.genglobal.org/startup-nations/snap`.

[6]The *Start-up Act* establishes a set eligibility criteria to identify start-ups that are expected to be or become innovative and can benefit from policy support. To meet the criteria, a company must: be in operation for less than 5 years, be based in Italy, have an annual revenue of less than 5 million euros, not be the result of a branch split or merger, have a mission statement focused on innovation, be a limited company and not publicly listed, and not have distributed profits. Additionally, the company must meet at least one of the following three conditions: have at least a 15% R&D expenditure ratio; have one-third of employees who hold PhDs or are researchers and/or two-thirds who hold a Master's degree; be the holder, depository, or licensee of a patent or owner/author of registered software (see Manaresi et al. (2021)).

[7]This trend is common to many local governments across European countries. Examples are: NewCo Factory (Helsinki-Uusimaa Region, Finland), Start of Business (Zlínský kraj, Czech Republic), Gründung Innovativ (Brandenburg, Germany), Welfare Tech Invest (Region of Southern Denmark), Frühphasenfonds (Brandenburg, Germany), the Bavarian Program to support Technology-oriented Startups (Bayern, Germany), Flügge Program (Bayern, Germany), the 'Start? Zuschuss!' Competition (Bayern, Germany), the Markteinführung innovativer Produkte (Saxony, Germany), Technologiegründerfonds Sachsen (TGFS) (Saxony, Germany), techstart NI (Northern Ireland, Ireland), Innovation Fund (East Netherlands, Netherlands), Capital Riesgo Start Up (Andalucia, Spain), ACCIO (Catalunya, Spain), Start-up Catalonia Programme (Catalunya, Spain), Barcelona Accelera (Barcelona, Spain), Ayudas para el desarrollo de jóvenes empresas innovadoras de base tecnológica (Comunidad de Madrid, Spain), Ayudas para la Puesta en marcha y funcionamiento de empresas jóvenes e innovadoras (La Rioja, Spain), Capital Investment Fund Malopolska (Malopolska, Poland), Startup Braga Acceleration Program (Braga, Portugal), Startup Lisboa (Lisbon, Portugal), Hertfordshire Start-up Programme (Hertfordshire, UK).

(ERDF) within the intervention field "SME business development, support to entrepreneurship and incubation (including spin-offs and spin-outs)". The use of European Cohesion Policy funds to implement programs supporting innovative start-ups is not an isolated case in the European context. During the last programming cycle, the European Cohesion Policy has doubled the resources allocated to promote entrepreneurship and support small businesses' growth (from €70 billion in 2007-2013 to €140 billion in 2014-2020).[8]

Local interventions in Italy assigned funds following two main procedures: approximately one half disbursed incentives on a first-come-first-served basis (i.e. "procedimento a sportello"); the other half awarded incentives through a competitive selection process (i.e. "procedimento a bando") (Albanese et al., 2019). The latter entails that firms submit a proposal which is then evaluated by an independent technical committee appointed by regional governments.[9] As long as the proposal (firm) is considered eligible, the technical committee scores and ranks each proposal. Funding is then assigned by regional governments following the final ranking up until monetary resources run out. As these competitions entail a discontinuity in the assignment mechanism, our analysis focuses on them.

2.1 Data and descriptive statistics

We start by collecting information on the 101 programs targeting innovative start-ups identified by Albanese et al. (2019). On top of those, we also found an additional 35 programs organized by either regional governments or local Chambers of Commerce throughout the period 2012-2021. As mentioned above, we focus on those awarding incentives through a competitive selection process, which represent around 50% of all programs. Consequently, we did not include those competitions disbursing incentives on a first-come-first-served basis (i.e. "procedura a sportello"). Additionally, we discarded those programs featuring a competitive selection process that granted incentives to all participating firms (see Data Appendix).

[8]Within the European Cohesion Policy, the ERDF is the primary source for start-up funding. Out of the 227 ERDF programs supporting SMEs, 133 have targeted start-ups. The ERDF planned to support more that 150,000 start-ups across the EU during 2014-2020. On average, new enterprises represent around 14% of all enterprises targeted to receive ERDF funding (https://cohesiondata.ec.europa.eu/stories/s/In-profile-Support-to-new-enterprises/y7wf-mgd5/).

[9]Evaluation criteria, which might differ across competitions, largely mimic those used in Horizon2020 competitive procedures. They generally encompass i) impact, ii) excellence, and iii) quality and efficiency of implementation.

Using official sources, we hand-collect data on 2,302 applicants participating to 40 local competitions in 8 Italian regions. Summary information on the selected competitions are reported in Table A1. All the programs considered provide support in the form of grants below €200,000 to meet the *de minimis* rule set by the EU legislation on state aids. The mean incentive is approximately €45,000 (the median being €30,000). On average, a competition features 58 eligible applicants and 33 winners (the median values are respectively 30 and 18) .[10]

For each competition, our final dataset contains information on applicant identifiers, scores, rankings, whether the applicant has eventually received incentives or not, and the amount financed. In principle, the availability of competition scores would allow to use them as running variable in a RD setting. However, scores are not assigned using homogeneous scales across all competitions. Hence, we resort to rankings as our running variable (see Section 3). In more detail, rankings are centered in zero given that the number of applicants and winners across competitions is heterogeneous.[11]

We then linked applicants to Bureau van Dijk's ORBIS by combining exact matching on firms' VAT and fuzzy matching on names (for more details, see Data Appendix). We were able to find a valid match for 81% of all applicants (i.e. 1,872). This allowed us to retrieve information on a wide variety of firm-level outcomes. In more detail, we are particularly interested in testing whether local programs boost firm-level innovation activities. We rely on the number of patent families –regardless of where the patents are filed– as our main measure of innovation.[12] This is sourced from ORBIS Intellectual Property and serves as a proxy for the number of inventions a firm produces. Using patents as a measure of innovation has several recognized limitations. Notably, not all inventions are patented, though the most valuable ones arguable are, hence counting patents only captures high-value inventions. Nonetheless, because patents exhibit a remarkable heterogeneity, we also consider indicators that weight patents based on their quality.

To examine whether local incentives act as a catalyst of follow-on equity investment, we

[10]We do not retain those firms that applied but were not considered eligible for the incentives.

[11]In some competitions, applicants have tied scores. Tied scores are assigned the same rank, so there may be multiple applicants at the same rank. If there are, for example, multiple highest scoring non-winners, these highest scoring non-winners are all ranked at -1.

[12]The set of patents filed in different countries related to the same invention is called a patent family. The vast majority of patent families include only one patent. To avoid double-counting inventions that are protected in several countries, we use the DOCDB patent family indicator.

retrieve data on both private equity and venture capital financing deals from ORBIS Zephyr. ORBIS also provides information on company financial statements, allowing us to test whether incentives lead to increase in investment and firm size. Finally, we also retrieve data concerning failure events.

Table 1 reports summary statistics for the whole sample of applicants referring to the year before the competition.

3 Empirical strategy

The identification strategy leverages the policies assignment mechanism: applications are ranked according to the technical committees' evaluation and funding availability ultimately determines the number of incentives awarded in each competition. This discontinuity can be exploited to employ a standard sharp RD approach, which entails the estimation of the following model:

$$Y_{ic}^{Post} = \alpha + \beta Incentive_{ic} + f\left(Rank_{ic}\right) + \delta_c + \varepsilon_{ic} \tag{1}$$

where Y_i^{Post} is the firm outcome during the post-assignment period for firm i in competition c, $Rank_{ic}$ is the (centered) rank assigned by experts to firm i in competition c, $Incentive$ is an indicator for firm i winning the competition c (i.e. $Rank_{ic} > 0$). $f\left(Rank_{ic}\right)$ is a polynomial control for centered ranks, and δ_c represents competition fixed effects.

Conventional continuity-based inference approaches for RD designs rely on non-parametric local polynomial techniques and large-sample approximations (Hahn et al., 2001). However, since our running variable is discrete and has few mass points (i.e. values of the variable that are shared by many units), we rely on a local randomization approach. The latter assumes that treatment assignment can be approximated by a local random experiment near the threshold (Cattaneo et al., 2015, 2016).

An important feature of this alternative framework is that, unlike the standard RD continuity-based approach, estimation proceeds as in an experiment, and finite sample adjustments ensure that the method has power even for small samples close to the threshold. (Cattaneo et al.,

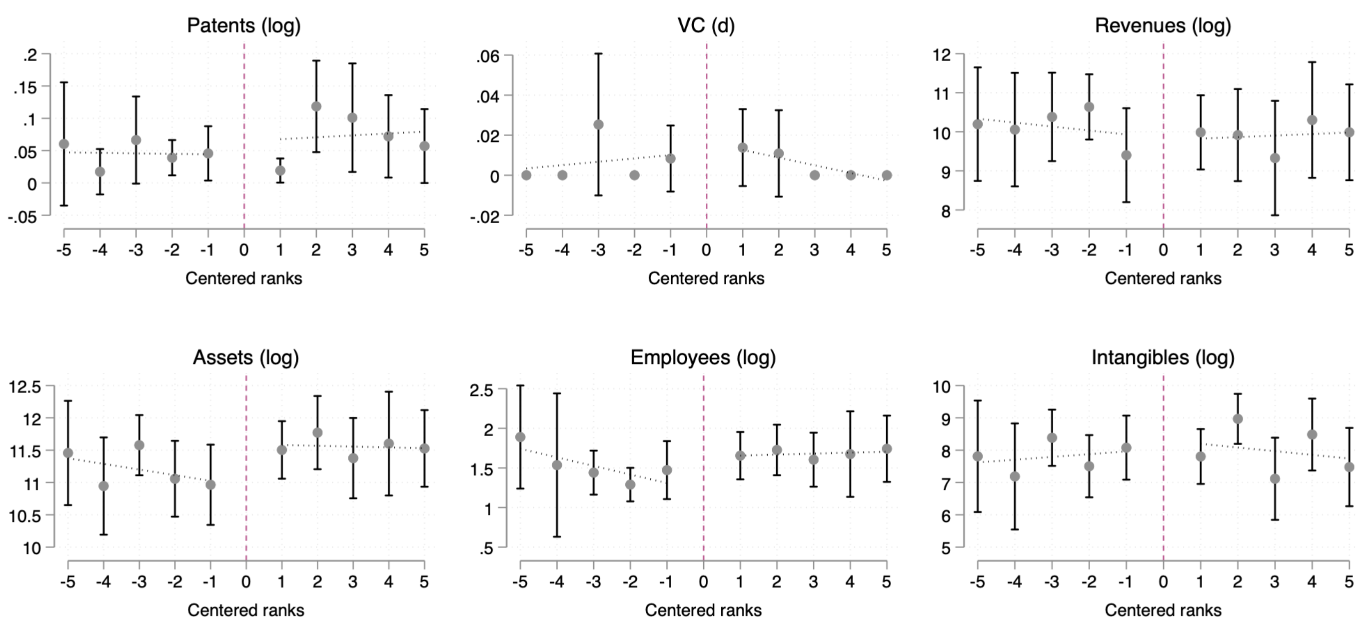
Table 1: Summary statistics

	(1) Mean	(2) SD	(3) Min	(4) Max	(5) N
Patent (d)	0.07	0.26	0	1	1872
Patents	0.18	1.03	0	30	1872
VC (d)	0.01	0.09	0	1	1872
VC deals	0.01	0.12	0	3	1872
VC amount	9.82	190.49	0	7000	1872
Revenues	1090.90	3993.65	0	51096	1086
Assets	1158.55	4212.14	0	48663	1028
Tangibles	231.52	1444.92	0	30145	1028
Intangibles	76.04	286.84	0	6709	1028
Subsidy (d)	0.14	0.34	0	1	1872
Apply (d)	0.02	0.14	0	1	1872
Win (d)	0.00	0.06	0	1	1872
PLC (d)	0.75	0.43	0	1	1871
Age	4.06	8.38	0	71	1785
ISUP	0.38	0.48	0	1	1872
High-tech (d)	0.59	0.49	0	1	1780
R&D intensity (d)	0.51	0.50	0	1	1780
GDP per capita	34024.87	9848.18	15000	51300	1789
IQI	0.76	0.19	0	1	1789

Notes: the table reports summary statistics for all applicants in the pre-competition period. The variables considered are: Patent (d): dummy indicating with 1 if firm has filed patents in the five years before the competition, and 0 otherwise; Patents: number of patent families filed in the five years before the competition plus one; VC (d): dummy indicating with 1 if firm has received VC or private equity in the five years before the competition, and 0 otherwise; VC deals: number of equity deals received in the five years before the competition; VC amount: euro amount of external equity received in the five years before the competition; Revenues: revenues in the year before the competition; Assets: total assets in the year before the competition; Tangibles: tangible assets in the year before the competition; Intangibles: intangible assets in the year before the competition; Subsidy (d): dummy indicating with 1 if firm has received other local or national subsidies in the three years before the competition, and 0 otherwise; Apply (d): dummy indicating with 1 if firm has applied to grants from the European SME Instrument in the three years before the competition, and 0 otherwise; Win (d): dummy indicating with 1 if firm has received grants from the European SME Instrument in the three years before the competition, and 0 otherwise; PLC (d): dummy indicating with 1 if firm is a private liability corporation, and 0 otherwise; Age: age of the firm in the year before the competition; High-tech (d): dummy indicating with 1 if firm operates in high-tech or knowledge intensive sector, and 0 otherwise (Eurostat); R&D intensity (d): dummy indicating with 1 if firm operates in R&D intensive sector, and 0 otherwise (OECD); ISUP (d): dummy indicating with 1 if firm is registered as innovative start-up in the year before the competition, and 0 otherwise; GDP per capita: GDP per capita of province (i.e. NUTS-3) where firm is located; IQI: Institutional Quality Index of province (i.e. NUTS-3) where firm is located (Nifo and Vecchione, 2014).

2015).[13] With a discrete running variable we can easily determine the exact location of the smallest window around the threshold: this is the interval of the running variable that contains the two mass points, one on each side, that are immediately consecutive to the threshold. In our case, assuming that the window including the first unsuccessful firm and the last winner is where randomization is plausibly at its peak, we take firms ranked -1 and 1 and run our local randomization approach. As local randomization estimators do not accommodate covariates (Cattaneo et al., 2015), in our baseline specification we demean the dependent variables at the competition-level. By doing so, we effectively restrict the comparison to applicants on either side of the threshold, but within the same competition, thus controlling for time and geography specific factors.

Figure 1: Pre-competition RDD plots



Notes: Circles represent rank-level means of the pre-competition firm-level outcomes. The sample includes firms with centered ranks between -5 and 5. Bars report 95% confidence intervals.

A valid local randomization requires the absence of any systematic difference in predeter-

[13]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. (2017) strongly suggest the use of local randomization inference rather than non-parametric estimation techniques when the running variable is discrete.

mined covariates between treated and untreated firms around the threshold. We start by examining this aspect graphically. RDD plots reported in Figure 1 provides support for local continuity across the threshold for a number of pre-competition outcomes.[14] Additionally, Table 2 reports estimates from our baseline specification using several pre-competition variables, including firm-level innovative outcomes, financing events, size, age, legal entity, sectoral and geographical information. The difference-in-means between firms ranked -1 and 1 for both pre-treatment outcomes and observables is indistinguishable from zero, thus reassuring on the validity of the approach. [15]

Table 2: Balancing tests for pre-determined covariates

	(1) Diff.	(2) *p-value*	(3) N^{left}	(4) N^{right}	(5) N
Patent (d)	-0.013	0.536	120	145	265
Patents (log)	-0.021	0.302	120	145	265
VC (d)	0.000	0.914	120	145	265
VC deals (log)	0.000	0.914	120	145	265
VC amount (log)	0.027	0.718	120	145	265
Revenues (log)	-0.104	0.862	60	85	145
Assets (log)	0.010	0.996	50	84	134
Tangibles (log)	0.201	0.762	50	84	134
Intangibles (log)	-0.452	0.434	50	84	134
Subsidy (d)	0.040	0.188	118	142	260
Apply (d)	0.001	0.998	120	145	265
Win (d)	0.000	1.000	120	145	265
PLC (d)	-0.018	0.650	120	145	265
Age (log)	-0.009	0.950	110	136	246
ISUP (d)	0.017	0.738	111	136	247
High-tech (d)	-0.061	0.166	111	136	247
R&D intensity (d)	0.007	0.886	120	145	265
GDP per capita (log)	-0.025	0.248	111	136	247
IQI (log)	-0.024	0.138	111	136	247

Notes: the table reports balancing tests for both pre-competition outcomes and observables. Continuous variables are in log (for patents and VC variables we add one before taking logs). Estimates are obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Dependent variables are demeaned to account for competition fixed effects. See Table 1 for the definition of the variables.

[14] Balancing plots for further variables are reported in Appendix Figure A2.

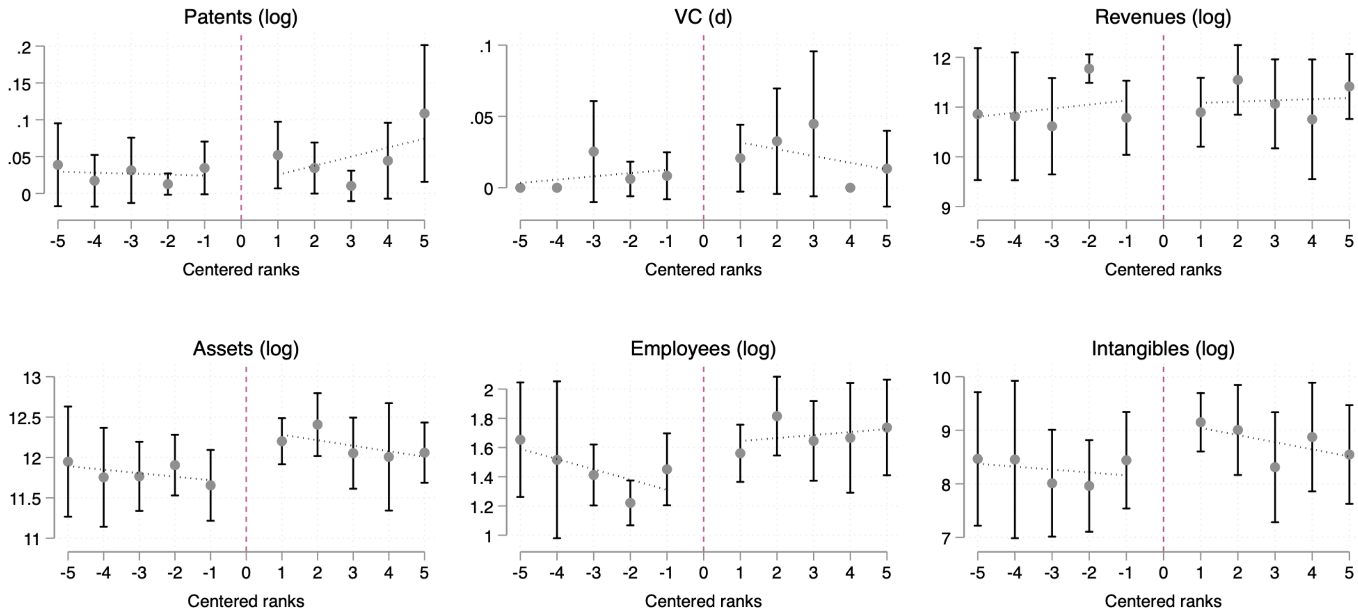
[15] Due to the discrete nature of the running variable, we cannot resort to the standard McCrary (2008) density test. Yet, we analyze the density of the running variable within our selected window [-1, 1], i.e. whether the number of firms just above the threshold is similar to the number of firms just below it (Cattaneo et al., 2017). The number of control firms immediately below the cutoff (161) and treatment firms above the cutoff (175) is slightly unbalanced. However, a binomial test that the probability of being treated is 0.5 does not reject the null (p-value=0.478).

4 Results

Innovation outcomes. We start out by investigating whether local programs supporting startups trigger an increase in innovative activity in the three years following the competition. To that end, we proxy innovation output using patenting, consistent with the innovation literature. In more detail, we test whether after the competition marginally winning firms experience an increase in the probability to file a patent (or increase in the number of patents) with respect to marginally losing ones. Graphical evidence does not indicate any major discontinuity in post-competition patenting (see Figure 2). Consistent with this, estimation results reported in columns (1) and (2) of Table 3 indicate that winners do not enjoy higher chances to file a patent, nor to increase the overall number of patents. However, as patents vary in quality, one potential concern is that these policies might induce firms to file high-quality patents. Columns (3) and (4) report estimations using two alternative ways to account for patent quality. Column (3) weights patents by their forward citations, a widely accepted proxy for patent value in the literature (Trajtenberg, 1990). Column (4) weights patents by the size of their families, i.e. the number of patent offices in which each patent is filed. As the process of obtaining a patent is costly, the number of countries where a patent is filed represents a proxy for its economic value (Harhoff et al., 2003). Even when considering these two alternative quality-adjusted patenting measures, we do not detect any meaningful impact. If anything, weighting patents by quality results into point estimates that are marginally more negative.

External financing. Apart from potentially affecting innovation outcomes, public incentives might affect the likelihood of receiving follow-on external equity by reducing information asymmetries which tend to be particularly severe for innovative young firms (Howell, 2017; Santoleri et al., 2022). External equity, and in particular venture capital, is a crucial source of external finance for startups, enabling their goods to reach the market more quickly (Hellmann and Puri, 2000) and providing non-monetary resources like managerial expertise and networking (Bronzini et al., 2020). Testing the effects on follow-on equity allows to understand whether incentives crowd out private investment. Also, receiving equity injections indicates that a firm offers a privately profitable opportunity, and is a good early-stage proxy for market success in a

Figure 2: Post-competition RDD plots



Notes: Circles represent rank-level means of the post-competition firm-level outcomes. The sample includes firms with centered ranks between -5 and 5. Bars report 95% confidence intervals.

context where outcome data are difficult to obtain (Howell, 2017). We run our baseline models using different dependent variables: column (1) uses a dummy indicating whether a start-up raises venture capital or private equity in the three years following the competition; column (2) uses the number of equity deals raised in the three years following the competition; column (3) uses the euros amount of all equity deals. Results reported in Table 4 show, across all outcome variables, no indication that local programs spur follow-on equity financing.

Investment, firm size and survival. While patenting and external equity are widely used measures to evaluate the impact of programs targeting innovative start-ups, they are not necessarily sufficient to rule out additionality altogether. Incentives might lead to the commercialization of new products and services that are not patented. Likewise, firms might seek finance other than that provided by equity investors, especially in contexts where VC markets tend to be underdeveloped like Italy. To provide additional evidence on the effects of these local policies, we consider the impact on balance-sheet outcomes. In particular, we examine the

Table 3: Effects on patents

	(1) Patent (d)	(2) Patents (log)	(3) Citw patents (log)	(4) Famw patents (log)
Diff-in-Means	-0.004	-0.004	-0.009	-0.010
p-value	0.834	0.848	0.774	0.840
Window	1	1	1	1
N_{left}	120	120	120	120
N_{right}	145	145	145	145
N	265	265	265	265

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: (1) dummy equal to one if firm has filed at least one patent family in the three years following the competition, and 0 otherwise; (2) log of all patent families filed in the three years following the competition plus one; (3) variable (2) weighted by forward citations; (4) variable (2) weighted by the size of the family i.e., the total number of jurisdictions in which each invention is patented. Dependent variables are demeaned to account for competition fixed effects.

Table 4: Effects on raising external equity

	(1) VC (d)	(2) VC deals (log)	(3) VC amount (log)
Diff-in-Means	0.009	0.010	0.063
p-value	0.594	0.432	0.536
Window	1	1	1
N_{left}	120	120	120
N_{right}	145	145	145
N	265	265	265

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: (1) dummy equal to one if firm has raised venture capital or private equity in the three years following the competition, and 0 otherwise; (2) log of the number of equity deals raised in the three years following the competition plus one; (3) log of the euros amount of all equity deals raised in the three years following the competition plus one. Dependent variables are demeaned to account for competition fixed effects.

effects on investment, and firm size. The idea is that if these programs do not increase intermediate outcomes (e.g. investment), it is unlikely that they will affect ultimate outcomes (e.g. the introduction of new products and services). Investment is computed using the cumulated annual variation in total fixed assets between time t and $t+2$. The same approach is used to compute investments in tangible and intangible assets, with the latter often used to proxy innovation efforts when R&D expenditures are not observed (Bronzini and Iachini, 2014). Table 5 shows null effects across all three measures. We then move to testing effects on firm size, using revenues, assets and employees across both time $t+1$ and $t+2$. Even in this case, we find no indication that programs trigger an increase in firm performance (see Table 6).[16]

Table 5: Effects on investments

	(1) Investment	(2) Tangibles	(3) Intangibles
Diff-in-Means	-7.896	2.897	-10.792
p-value	0.844	0.912	0.602
Window	1	1	1
N_{left}	52	52	52
N_{right}	87	87	87
N	139	139	139

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: (1) cumulated annual variation in total fixed assets between time t and $t+2$ (thousand euros); (2) cumulated annual variation in tangible fixed assets between time t and $t+2$ (thousand euros); (3) cumulated annual variation in intangibles assets between time t and $t+2$ (thousand euros). Dependent variables are demeaned to account for competition fixed effects.

Overall, all results concerning balance-sheet measures corroborate the absence of any direct additionality stemming from local policies. The only detectable impact on firm performance is a reduction in failure likelihood, though it is only statistically significant for the year after the competition (see Table 7).

In sum, results indicate that these programs lead to full crowding out.[17] It is critical to

[16]These results should be interpreted with some caution as balance-sheet variables in ORBIS do not provide an optimal coverage for small and young firms due to national accounting regulations allowing those firms not to report them.

[17]The average null effect documented so far could mask substantial heterogeneity. In Appendix Table A2, we report a number of tests addressing potential differences across a set of observables characteristics (e.g. firm location, sector, etc.). We do not find remarkable differences. Yet, given the limited number of observations, our

Table 6: Effects on size

	(1) Revenues$_{t+1}$	(2) Assets$_{t+1}$	(3) Employees$_{t+1}$
Diff-in-Means	-0.234	0.102	-0.071
p-value	0.644	0.636	0.550
Window	1	1	1
N$_{left}$	85	68	58
N$_{right}$	122	110	83
N	207	178	141
	(4) Revenues$_{t+2}$	(5) Assets$_{t+2}$	(6) Employees$_{t+2}$
Diff-in-Means	0.071	-0.040	-0.114
p-value	0.888	0.824	0.324
Window	1	1	1
N$_{left}$	84	63	59
N$_{right}$	117	104	82
N	201	167	141

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: log of firm size (proxied by revenues, assets or employment) measured at time $t+1$ (top) or $t+2$ (bottom). Dependent variables are demeaned to account for competition fixed effects.

emphasize that this does not preclude the possibility of any effect whatsoever; there might be beneficial impacts along dimensions that are beyond our ability to evaluate. Yet, null effects are more informative than statistically significant effects (Abadie, 2020), especially when the prior is that a policy will be effective. Expecting positive effects is reasonable in our context given that the literature documents a positive impact from policies allocating support to small, early-stage firms through competitive selection at the local level (see, e.g., Bronzini and Iachini 2014; Bronzini and Piselli 2016; Lanahan and Feldman 2018; Cerqua and Pellegrini 2014; Zhao and Ziedonis 2020; Cingano et al. 2022) or at the national level with comparable amounts of financial resources disbursed to similar firms (Barrows et al., 2018; McKenzie, 2017; Kleine et al., 2022).[18]

setting is admittedly not best positioned to provide an exhaustive characterization, hence we refrain from deriving strong conclusions from these estimates.

[18] For instance, Kleine et al. (2022) finds positive effects on SMEs' innovation outcomes stemming from a £5,000 voucher scheme in the United Kingdom subsidizing R&D collaboration; McKenzie (2017) reports substantial performance gains from grant prizes of $50,000 for entrepreneurs in Nigeria; Barrows et al. (2018), in a cross-country setting, finds that start-up programs assigning $26,000 on average benefit firms across several outcome measures. In our case, the average incentive is quite similar to the last two studies (i.e. €45,000).

Table 7: Effects on failure

	(1) $\text{Failure}_{t+1}(d)$	(2) $\text{Failure}_{t+2}(d)$	(3) $\text{Failure}_{t+3}(d)$
Diff-in-Means	-0.049	-0.038	0.012
p-value	0.000	0.138	0.712
Window	1	1	1
N_{left}	115	116	116
N_{right}	141	142	142
N	256	258	258

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: dummy equal to 1 whether a firm exits via liquidation, insolvency, or bankruptcy at different time intervals, and 0 otherwise. Dependent variables are demeaned to account for competition fixed effects.

Follow-on public subsidies. Finally, we move to investigating whether securing incentives from local programs lead start-ups to receive further public subsidies later on. In principle, winning a competition might grant an advantage in terms of both resources and status thus generating persistence in subsequent access to public funds. This is often referred to as "Matthew effect" (Merton, 1968) in the economics of science.[19] According to Antonelli and Crespi (2013), the presence of such an effect in allocating public funding is not necessarily bad. They distinguish between *vicious* and *virtuous* "Matthew effects". The latter refers to the repeated allocation of grants to firms that have been actually able to use prior subsidies to effectively increase innovation efforts. In this case, this can be considered a genuine persistence reflecting dynamic increasing returns in the generation of technological knowledge. On the contrary, the *vicious* "Matthew effect" occurs when persistence in public subsidy assignment is based on reputation, even if the firms have reduced their commitment to innovation after receiving previous subsidies.

In our context, given that the analysis finds a null effect of local policies, we test whether such programs lead to a *vicious* "Matthew effect". To that end, we use data from Opencup, a

[19] Public offices may not have the necessary information and capabilities to optimally select beneficiaries, so decisions are often made based on the firm's prior achievements. The "Matthew effect" refers to the possibility that this leads to a situation where allocation is based not only on the firm's capabilities or the quality of the submitted project, but on its reputation. Additionally, allocating funds to well-known and established firms with a successful track-record enhances the evaluation of the public office, as it improves its statistics. Finally, frequent applicants are better equipped to submit new projects as they are familiar with the funding schemes and how to apply for them, whereas first-time applicants may not have the same level of knowledge about the public funding system.

database on the universe of firms that have received subsidies from Italian authorities.[20] We match our data on applicants to local competitions for startups with information on subsidies allocated by other public programs. These include recipients since 2007 up to 2021. Differently from our hand-collected data, Opencup only reports beneficiaries of the programs and there are no information on non-awarded applicants. Moreover, Opencup reports the funding entity and it is possible to distinguish between subsidies provided by local or central authorities.

As reported in Table 2, firms that will be eventually treated are not different from control firms in terms of having received funding before the competition takes place.[21]. Yet, after securing the incentive, winning firms enjoy an increase in the probability of receiving further public subsidies of around 12 percentage points (see column (2) of Table 8). Relative to a 14% mean, this effect translates roughly into an 85% increase in the chances of getting follow-on subsidies. In sum, this result confirms the presence of a *vicious* "Matthew effect".[22]

Table 8: Effects on receiving follow-on public subsidies

	(1) $\text{Subsidy}_{Post}(d)$	(2) Local $\text{Subsidy}_{Post}(d)$	(3) Central $\text{Subsidy}_{Post}(d)$
Diff-in-Means	0.115	0.135	0.039
p-value	0.012	0.002	0.218
Window	1	1	1
N_{left}	123	123	123
N_{right}	148	148	148
N	271	271	271

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: (1) dummy equal to one if firm has received a public subsidy in the three years before the competition, and 0 otherwise; (2) dummy equal to one if firm has has received a public subsidy in the three years following the competition, and 0 otherwise. Dependent variables are demeaned to account for competition fixed effects.

One possible mechanism behind this finding is that winning a local competition acts as a certification device which increases the chances to get more funds at the local level. Yet, this certification may not be salient enough to exert effects towards institutions allocating subsidies

[20] Cf. the Data Appendix for a description of the dataset and for details about data processing.

[21] Graphical evidence suggesting that treated and untreated groups around the threshold are similar in terms of pre-competition funding is reported in Appendix Figure A3.

[22] Results reported in the Appendix show that our main findings hold when using the continuity-based RD framework (Table A7), the use of staggered difference-in-differences, and staggered difference-in-differences combined with the RD approach (Tables A8 and A9).

at the national or international level. Hence, we estimate these specifications using recipients from local and central authorities separately (see columns (2) and (3) in Table 8). Results indicate that what drives the aggregate estimates is funds disbursed at the local level, whereas no effect is detected for subsidies allocated by central authorities. In other words, the evidence suggests that the *vicious* "Matthew effect" is at work at the local level only, a result that is consistent with reputation getting weaker with geographical distance.[23]

We then test whether similar results are obtained when considering the most important (and highly competitive) grant program in the EU for small and medium enterprises: the SME Instrument. This scheme, which is the European equivalent of the US SBIR, allows early-stage ventures with high growth and innovative potential to secure funding of either €50,000 (Phase I) or between €500,000 and €2,5 million (Phase II) (Santoleri et al., 2022).

In Table 2 we provided evidence of no pre-competition imbalancing in either the likelihood of applying or winning the SME Instrument. However, these null results are confirmed even after the award of the local incentives (see Table 9), indicating that these programs do not increase the probability that these firms either participate or win these grants. This provides further confirmation that the certification effects of local incentives only lead to follow-on public subsidies at the local level, whereas they do not affect the likelihood of securing additional subsidies from central authorities nor they increase application or winning more competitive and prestigious awards allocated at the European level.[24]

4.1 Robustness

We check the robustness of our results in several ways. First, we test the sensitivity of the baseline estimates to our window choice (i.e. [-1,1]). We replicate our analysis expanding the window around the threshold to [-2,2] centered ranks. Results largely confirm our main findings (see Table A3).

Second, we address one potential concern related to the heterogeneity in the running variable

[23]Given that we only observe beneficiaries of other policies in our data, we cannot disentangle whether the increased likelihood in securing follow-on public subsidies is driven by a rise in application propensity.

[24]While the SME Instrument arguably represents the best candidate within the European Commission Horizon 2020 framework program to finance innovation for individual small and young companies, firms in our sample may also apply and win other schemes within H2020. To check whether results may change in this instance, we re-run our regressions considering the entirety of Horizon 2020 and confirm our findings (see Appendix Table A11).

Table 9: Effects on applying and winning the SME Instrument

	(1) $\text{Apply}_{Post}(d)$	(2) $\text{Win}_{Post}(d)$
Diff-in-Means	0.020	0.003
p-value	0.432	0.958
Window	1	1
N_{left}	120	120
N_{right}	145	145
N	265	265

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: (1) dummy equal to one if firm has applied to the SME Instrument (either to Phase I or II) in the three years following the competition, and 0 otherwise; (2) dummy equal to one if firm has won a SME Instrument (either to Phase I or II) grant in the three years following the competition, and 0 otherwise. Dependent variables are demeaned to account for competition fixed effects.

across competitions. Start-ups ranked first among losers (i.e. with centered rank equal to -1) might be assigned scores that are not very close to the last winning firm (i.e. with centered rank equal to 1). In such scenario, we would be comparing firms that, while close to each other in terms of rankings, are not necessarily close to each other in terms of scores. To that end, we re-run the analysis keeping only those competitions in which the distance in scores between the first losing firm and the last winning firm is below 10%.[25] Results are largely unaltered (see Table A4). A similar concern refers to the possibility that our results stem from comparing marginal firms that are positioned very low in the final (un-centered) rankings within a given competition. To address this, we restrict our analysis to those competitions in which the success rate (i.e. the number of awardees over the number of applicants) does not exceed 75%. Even with this alternative sample, discarding 10 competitions, our main findings hold (see Table A5).

Third, we re-run our estimates following a donut hole strategy. The rationale being that, in presence of endogenous sorting across the threshold, this would happen among units whose rankings are close to the cutoff; as a result, when such observations are excluded, the treatment effect might alter. Hence, we discard firms ranked immediately below or above the threshold

[25] This amounts to excluding 10 out of 40 competitions.

(i.e. -1 and 1) and include only those ranked 2 and -2. Estimates reported in Appendix Table A6 show very similar findings if compared with the baseline.

Fourth, we repeat the entire analysis using the standard RD continuity-based approach (Calonico et al., 2014a). Overall, results shown in Table A7 point in the same direction as our baseline estimates.

Fifth, we use a panel setting, where observations are collapsed across the pre- and post-competition periods. This approach offers several advantages. First, it allows to adopt a difference-in-differences (DID) strategy to estimate the effects of the policy. While the parameter differs from the one recovered via the local randomization approach, it allows to test whether local policies for innovative start-ups have an effect on all treated firms independently from their rankings. It may well be that these programs only impact high-quality firms that rank very high in the competitions, something that the local randomization approach would not capture. Table A8 reports the staggered DID estimates, which confirm the null effect of the policies with the exception of the coefficients on patenting outcomes that preserve their negative sign but gain statistical significance.

Additionally, we can combine the DID strategy with the RDD approach. This has two advantages: i) the panel specification increases statistical power; ii) as shown by Frandsen (2021), a panel setting can add a DID aspect to the RDD design, enabling the much weaker condition of local continuity in differences, and local continuity conditional on characteristics. To that end, we run our DID regressions limiting the sample to those firms ranked [-1;1] as in our baseline approach. Estimates displayed in Table A9 largely corroborate our main findings.

Finally, we investigate the external validity of our results by estimating the treatment effect derivative, i.e., the change in the slope of the trendline at the threshold (Dong and Lewbel, 2015). To do so, we estimate parametric RD models using a bandwidth of [-5,5] around the cut-off (see Table A10). Because our estimate of the treatment effect derivative is small and statistically insignificant, we proceed below under the assumption that it is equal to zero, i.e., we assume our local estimates also apply to firms away from the threshold.

5 Conclusions

In recent years, innovative start-ups have been targeted by many policy initiatives around the world. While evidence on their effectiveness exists for those enacted at the national-level, programs implemented by local authorities have been overlooked. To bridge this gap, the paper provides quasi-experimental evidence on the effectiveness of public programs implemented at the local level in Italy. Using a local randomization approach, we find a null effect of such programs on a wide number of firm-level outcomes. Apart from documenting the absence of any additionality, we show that securing local subsidies increases start-ups' probability to obtain even more funding later on, indicating the presence of a *vicious* "Matthew" effect in subsidy allocation. We argue that these local initiatives attract participation from ventures with low innovative potential seeking to reduce their cost of capital, and not to gain access to otherwise-unavailable funding. As a result, more scrutiny over these programs is needed as these, apart from being ineffective, might create incentives for firms to become "subsidy entrepreneurs", which could be an unproductive form of entrepreneurship.

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Online Appendix

A1 Data Appendix

This appendix describes the methodology used to construct our database. We combine data from four main sources: (i) hand-collected data on local competitions for innovative start-ups, (ii) data on subsidies disbursed by Italian local and national authorities, (iii) research and innovation subsidies provided by Horizon 2020; (iv) firm performance outcomes and financing events from ORBIS.

Local competitions for innovative start-ups. Our primary dataset features hand-collected information on subsidies disbursed by Italian local authorities to start-ups. Gathering information on these policies, especially from a local perspective, is challenging as there is no systematic data collection on these programs, their applicants and beneficiaries. For instance, while data on public subsidies recipients exist for Italy, it is often impossible to screen which are effectively intended for start-ups. To identify the set of policies, we relied upon the work of Albanese et al. (2019) who extensively surveyed all local initiatives in Italy during the period 2012-2018. Based on the list of 101 interventions provided by Albanese et al. (2019) (see page 23), we browsed all the official websites of the listed local authorities to retrieve information on applicants and recipients in each program. In our effort to collect information on these programs, we identified an additional set of interventions towards innovative start-ups organized by either regional governments or local Chambers of Commerce throughout the period 2018-2021, which is not covered by Albanese et al. (2019). Information on programs, their applicants, rankings and the funding decisions were generally reported in PDF format on the regional government authority website or the regional agency for economic development website. After a first screening, we focused on the subset of programs awarding incentives through a competitive selection process (i.e. "producedura a bando"). Firms that are willing to secure the incentive have to submit a proposal which is then evaluated by an independent technical committee appointed by the local government. As long as the proposal (firm) is considered eligible, the technical committee scores and ranks each proposals. Funding is then assigned by regional governments following the final ranking up until monetary resources run out. As an example, Figure A1 displays the results from a competitive selection program in the Marche Region called "Sostegno allo sviluppo ed al consolidamento di start up ad alta intensita di applicazion di conoscenza". Applicants ranked 1st to 46th are considered eligible and financed (see column "esito"). Applicants ranked from the 47th position onward are eligible but not financed due to having reached the budget threshold (i.e. €1,376,559.85).

Figure A1: Example of results from a competitive selection program

POS.	ID DOM.	DENOMINAZIONE IMPRESA	P.IVA/C.F	TITOLO PROGETTO	PUNTEGGIO	ESITO	INVESTIMENTO AMMESSO	CONTRIBUTO CONCEDIBILE	
1	11573	NTP NANO TECH PROJECTS SRL	02494970417	NED: soluzione innovativa per l	82,633	AMMESSA E FINANZIATA	160.400,00	100.000,00	
...	...	...	...	...	...	...	...	...	
44	11552	WINELAB SRL	02250750441	LIFE: BIOFUNCTIONAL DRIN	64,633	AMMESSA E FINANZIATA	82.000,00	57.400,00	
45	11622	KEYEVOLUTION S.R.L. UNIPERSONALE	02686440427	Smart Shelf Unit	64,267	AMMESSA E FINANZIATA	135.000,00	94.500,00	
46	11589	BIMIDEAL S.R.L.	13653041007	BIMBANG: un percorso innovat	64,033	AMMESSA E FINANZIATA	143.341,00	100.000,00	1.376.559,85
47	11567	Z4TEC S.R.L.	02738340427	IOVisio (Inside and Outside Vis	63,733	AMMISSIBILE	144.000,00	100.000,00	
48	11337	AJET S.R.L.	02270960442	KENDRAG - Nuova pompa di a	63,433	AMMISSIBILE	115.650,00	57.825,00	
49	11727	SISON S.R.L.	02741310425	System for the Automation in M	63,200	AMMISSIBILE	141.560,00	99.092,00	
...	...	...	...	...	...	...	...	...	
73	11523	OIKOS 2.0 SOCIETA' COOPERATIVA	02239070440	BORGO DIGITALE	49,433	AMMISSIBILE	80.000,00	56.000,00	

Notes: This document refers to program "Sostegno allo sviluppo ed al consolidamento di start up ad alta intensita di applicazion di conoscenza" of the region Marche. It is retrievable at: https://bandi.regione.marche.it/Allegati/347/DDPF%20233_Allegato%20A%20Scorrimento%20Linea%20A.pdf. Firms ranked between 2nd and 43rd places, and those between 50th and 72nd places are not shown for visualization purposes.

We discarded from our sample those competitions disbursing incentives on a first-come-first-served basis (i.e. "procedura a sportello"). First-come-first-served basis programs may entail a discontinuity in assignment as resources are distributed to applicants until funds are exhausted. In that setting, one could estimate a regression discontinuity with time as the running variable. However, data on applicants to these programs are seldom available and almost no official website we consulted reported the exact time of application submission thus impeding us to run a regression discontinuity in time.

Within the subset of programs with a competitive selection process, we discarded those that i) published information solely for recipient firms, ii) did not report data on rankings and scores, iii) had enough budgetary resources from the start to grant incentives to all participating firms, and iv) those that initially did not have enough resources for all applicants but eventually awarded incentives to all participating firms. We ended up with a sample of 40 local competitions in 8 Italian regions, which saw the participation of 2,302 applicants. The full list of competitions considered in our sample is reported in Table A1.

ORBIS. We rely on ORBIS data to measure firm performance outcomes and financing events. ORBIS is a comprehensive global database of company information and financial data maintained by Bureau van Dijk, a Moody's Analytics company. The ORBIS dataset provides detailed information about companies worldwide, including their financial performance, ownership structure, and industry classifications. For a substantial number of applicant firms public records display their VAT numbers. In those instances, we perform an exact matching between the VAT reported by local authorities and those present in ORBIS. When VATs are not available, we match firms' names based on probabilistic matching via the ORBIS batch search functionality and retained only the matches with the highest quality (i.e. A scores). We performed extensive manual checks to ensure the accuracy of the fuzzy matching. We were able to find a valid match for 81% of all applicants (i.e. 1,872 out of 2,302). This allows us to retrieve firms' BvD unique identifiers and use them to retrieve information on i) firms' balance-sheets and survival, ii) patenting (via ORBIS Intellectual Property), iii) venture capital and private equity deals (via ORBIS Zephyr). The BvD unique identifiers are also instrumental to retrieve information from additional data sources, such as follow-on public subsidies from local, national and European authorities.

Additional public subsidies. Our main source of information is Opencup, an administrative dataset covering the universe of public transfers related to three main categories of investment projects: (i) public works; (ii) incentive to firms; (iii) transfers to areas hit by natural disasters.[26] We focus exclusively on projects labeled as "incentive to firms" (accounting for about 70% of total observations) since these generally identify public subsidies awarded to private companies. Differently from our hand-collected data, Opencup only reports beneficiaries of the programs and there are no information on non-awarded applicants. Opencup was launched in 2007, however, data coverage sharply increases after 2014, in correspondence of the new programming period 2014-2020 for EU cohesion funds.[27] Each subsidy in Opencup is associated to

[26] Data are publicly available at: `https://www.opencup.gov.it/portale/web/opencup/homepage`.

[27] This has limited impact on our analysis given that most of the competitions included in our dataset are concentrated in the 2014-2020 period (only for 4 interventions the decision year is 2013).

a unique identifier, the "Codice Unico di Progetto" (CUP). Also, the dataset reports a set of useful information including: the name of the awarding entity, the year of decision, the name and VAT of the beneficiary, the region of implementation. Using the VAT we matched public subsidy data for 1,872 applicants in our primary dataset, i.e. those firms for which we could retrieve the VAT from ORBIS. Moreover, we classified public incentives between those awarded by local and central authorities leveraging the classification of public entities available in Opencup. For the purpose of our analysis, it is also important to filter out focal subsidies, i.e. those provided by the local startup programs included in our dataset. This is necessary in order to disambiguate the main incentive from public support allocated by other public programs and avoid double-counting when estimating the effects on follow-on subsidies. Unfortunately, PDF documents available on local government websites generally do not report the CUP identifier. Furthermore, administrative data are subject to missreporting so that one should not expect a one-to-one correspondence with information disclosed by local governments. Taking into account these issues, we implemented a two-step procedure to identify the focal subsidy in Opencup data. First, for each competition, we restricted the observations using the VAT of awarded firms as well as other information available from PDF documents (e.g. region of implementation, funding entity, year of decision). In a second step, we exploited the variable "DESCRIZIONE_INTERVENTO" which reports a short description of the intervention, often containing the name of the program. By visual inspection, we identified a textual pattern in DESCRIZIONE_INTERVENTO matching the name of the local program. After performing additional manual checks, we were able to find in Opencup 90% of the incentives associated to awarded applicants with VAT available from ORBIS. Yet, to further refine the matching we relied upon two data sources: the National State Aid Registry and Opencoesione.[28] For both datasets, we replicated the same procedure adopted for Opencup. At the end of this process, 93% of the focal subsidies were matched in at least one of the three datasets considered. Hence, we excluded these observations from the count of follow-on subsidies in our baseline estimates. For the remaining 7% of awarded applicants, the incentive associated to the local startup competition did not have a match in

[28]The National State Aid Registry is a dataset including all public incentives classified as state aids by the EU regulation since 2017. Data are available at: https://www.rna.gov.it/sites/PortaleRNA/it_IT/open_data. Opencoesione is a database tracking all the public projects co-financed by EU cohesion funds since 2007. Data are available at: https://opencoesione.gov.it/it/opendata/#!progetti_section.

administrative data. Although this may stem from reporting errors, for those firms, we could not unequivocally disambiguate the focal subsidy from additional public support. Therefore, as a robustness check, we re-run our baseline estimates dropping observations for un-matched applicants and find unaltered results (cf column (2) in Table A12).

SME Instrument and H2020. Horizon 2020 (H2020) is the EU flagship program to finance research and innovation. The programme is running from 2014 to 2020 with a €80 billion budget. It provides research and innovation funding for multi-national collaboration projects as well as for individual researchers, private companies encomassing both large businesses and SMEs. We use confidential data compiled by the European Commission's Directorate-General for Research and Innovation (DG-RTD) on all applications to H2020 over its entire duration, which include both successful and unsuccesfull proposals. We extracted data on all unique proposals submitted by Italian organizations (73,413) which amount to 18,476 unique applicants. Then we retained data concerning private companies only, which amount to 40,091 unique proposals and 15,795 unique applicants. We then linked these data with ORBIS using VAT information when available or company names through fuzzy string matching. Firm names were first standardized by removing non-alphabetic characters and converting all strings to uppercase characters. Additionally, we omitted legal entity endings (e.g. INC, LTD, CORP) based on the information provided by the European Central Bank (https://www.ecb.europa.eu/stats/money/aggregates/anacredit/shared/pdf/List_of_legal_forms.xlsx). We retained only those matches equal or above 95% according to both bigrams and Jaro-Winkler string comparators. This resulted in matching 91% unique projects and 75% unique applicants with a valid BvD ID. We then proceed to match these data with our sample of applicants to local start-up competitions in Italy. Of the 1,872 firm-applications with a valid BvD ID in our sample, we found 240 correspondences with at least one application present in the H2020 dataset. In our baseline analysis, we use data concerning applicants and awardees of the SME Instrument. The SME instrument is a program established in 2014 and managed by EASME to support innovation in individual European SMEs in the framework of the Horizon 2020. Similar to the US SBIR, the scheme provides relatively young and small companies with R&D grants to develop groundbreaking innovative ideas.

Additional data. We complement the above data with a number of additional sources. First, using VAT identifiers, we link firms in our dataset with the Innovative Start-up Registry (https://startup.registroimprese.it/isin/home). This publicly available data record whether a given firm has registered as innovative start-up. Second, we use the Institutional Quality Index (IQI) database developed by Nifo and Vecchione (2014) which provides a composite indicator that assesses institutional quality in Italian provinces (accessible at https://sites.google.com/site/institutionalqualityindex/dataset/iqi-dataset). We link data on IQI corresponding to 2015 to firms in our dataset based on their NUTS-3 location reported in ORBIS. Similarly, we source GDP per capita figures for 2015 at the NUTS-3 level from Eurostat (https://ec.europa.eu/eurostat/databrowser/view/NAMA_10R_3GDP/default/table?lang=en) and link them to firms in our dataset based on their NUTS-3 location reported in ORBIS. Finally, for each firm, we use information on their sector of activity (NUTS rev. 2 at the 2-digit level) to classify whether the firm operates in high-tech (based on the classification provided by Eurostat) and R&D intensive industries (based on the classification provided by the OECD).

Table A1: Local competitions: summary characteristics

Id	Program name	Region	Year	Average incentive	Applicants	Awarded firms	Incentive type
1	Avviso Pubblico "Start and go"	Basilicata	2016	54488	123	52	Grant
2	Avviso Pubblico "Go and grow"	Basilicata	2016	168567	95	22	Grant
3	Startup e spin-off (Elenco A)	Basilicata	2013	130033	73	21	Grant
4	Startup e spin-off (Elenco B)	Basilicata	2013	114831	22	13	Grant
5	Agevolazioni innovazione - bando capitalizzazione 2013	Bolzano	2013	200000	6	4	Refund equity investment
6	Agevolazioni innovazione - bando 2014	PA Bolzano	2014	133750	15	4	Refund equity investment
7	Agevolazioni innovazione - bando 2016	PA Bolzano	2016	200000	15	6	Refund equity investment
8	Bando di contributi per lo sviluppo di progetti di innovazione tecnologica 4.0 promossi da imprese start up di Milano Monza Brianza Lodi	Lombardia	2018	26563	75	64	Grant
9	Start up per Expo	CC Milano	2015	15000	135	100	Grant
10	Sostegno alle startup piemontesi - Torino	Piemonte	2014	4000	53	20	Grant
11	Sostegno alle startup piemontesi - Alessandria e Asti	Piemonte	2014	4000	5	1	Grant
12	Sostegno alle startup piemontesi - Biella	Piemonte	2014	4000	5	2	Grant
13	Sostegno alla creazione di micro imprese innovative startup	Calabria	2019	116571	19	17	Grant
14	Sostegno alla creazione di micro imprese innovative startup	Calabria	2020	140351	31	29	Grant
15	Aiuto all'avviamento per nuove attivita innovative non agricole nelle aree rurali	Calabria	2017	50000	9	3	Grant

Table A1: Local competitions: summary characteristics

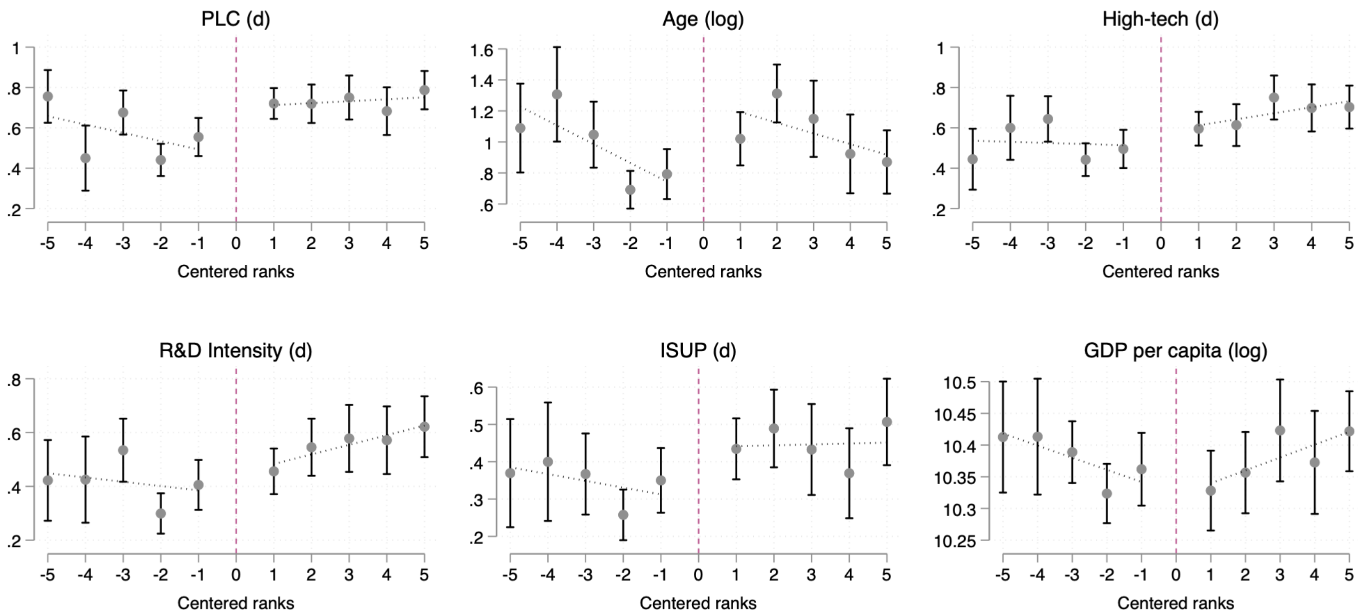
Id	Program name	Region	Year	Average incentive	Applicants	Awarded firms	Incentive type
16	Fondo per la nascita e lo sviluppo di imprese start-up innovative 2014	Lazio	2015	30000	13	8	Refund equity investment
17	Fondo per la nascita e lo sviluppo di imprese start-up innovative 2015	Lazio	2015	29615	16	13	Refund equity investment
18	Fondo per la nascita e lo sviluppo di imprese start-up innovative 2016	Lazio	2016	29667	19	9	Refund equity investment
19	Startup Lazio 2007-2013: Creativi Digitali app on	Lazio	2014	39813	58	45	Grant
20	Innovazione sostantivo femminile (bando 2015)	Lazio	2016	20759	81	18	Grant
21	Innovazione sostantivo femminile (bando 2017)	Lazio	2017	25222	8	4	Grant
22	Start up Lazio 2014-20: bando "pre-seed"	Lazio	2017	45012	226	94	Grant
23	Premio Idea Innovativa, la nuova imprenditorialita al femminile 2013	Lazio	2013	5000	19	5	Grant
24	Premio Idea Innovativa, la nuova imprenditorialita al femminile 2014	Lazio	2014	5000	26	4	Grant
25	Premio Idea Innovativa, la nuova imprenditorialita al femminile 2015	Lazio	2015	5000	26	5	Grant
26	Premio Idea Innovativa, la nuova imprenditorialita al femminile 2017	Lazio	2017	5000	9	3	Grant
27	Premio Idea Innovativa, la nuova imprenditorialita al femminile 2019	Lazio	2019	5000	11	5	Grant

Table A1: Local competitions: summary characteristics

Id	Program name	Region	Year	Average incentive	Applicants	Awarded firms	Incentive type
28	Bando R&I 2015 - misura b terzo elenco	Lombardia	2015	20000	74	40	Grant
29	Bando R&I 2015 misura b - quarto elenco	Lombardia	2016	20000	4	3	Grant
30	Bando siavs – start up innovative a vocazione sociale	Lombardia	2017	76923	16	13	Grant
31	Bando R&I 2016 - misura A2	Lombardia	2016	20000	44	20	Grant
32	Bando R&I 2016 - misura B	Lombardia	2016	20000	28	18	Grant
33	Innodriver 2017 - misura A	Lombardia	2017	25000	102	99	Grant
34	Innodriver 2017 - misura A II finestra	Lombardia	2018	25000	103	96	Grant
35	Sostegno allo sviluppo ed al consolidamento di start up ad alta intensita di applicazion di conoscenza - linea a	Marche	2017	75035	73	46	Grant
36	Progetti di avvio e consolidamento di nuove imprese sul territorio della provincia autonoma di trento 2016	Trento	2016	25816	113	91	Grant
37	Seed money bando 1/2017	Trento	2017		70	33	Grant
38	Progetti di avvio e consolidamento di nuove imprese sul territorio della Provincia autonoma di Trento	Trento	2018	41852	183	91	Grant
39	Sostegno alla creazione e al consolidamento di start- up innovative ad alta intensita di applicazione di conoscenza e alle iniziative di spin-off della ricerca - sezione a ”creazione”	Veneto	2018	86957	153	69	Grant
40	Aiuti agli investimenti delle start up 2016	Veneto	2016	44611	146	126	Grant

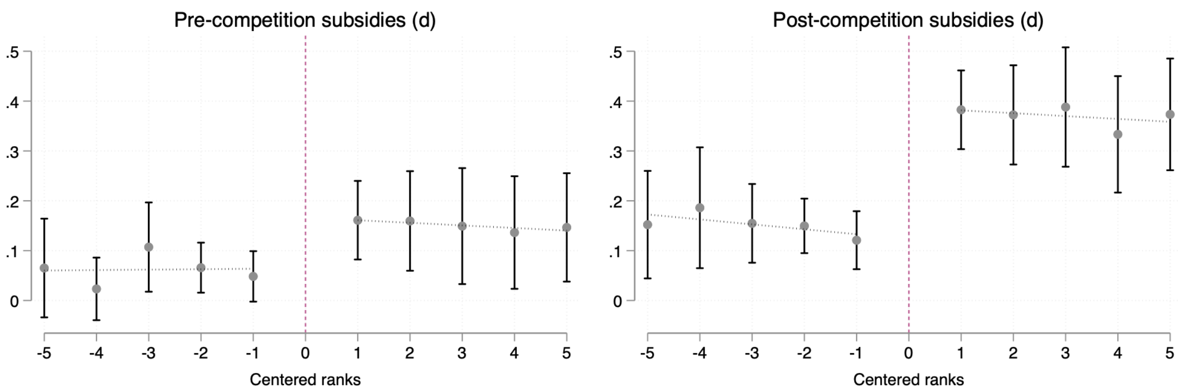
A2 Additional figures

Figure A2: Pre-competition RDD plots for additional observables



Notes: Circles represent rank-level means of the pre-competition firm-level outcomes. The sample includes firms with centered ranks between -5 and 5. Bars report 95% confidence intervals.

Figure A3: RDD plots for additional public subsidies



Notes: Circles represent rank-level means of the pre-competition firm-level outcomes (left plot) and post-competition firm-level outcomes (right plot). The sample includes firms with centered ranks between -5 and 5. Bars report 95% confidence intervals.

A3 Additional tables

Table A2: Heterogeneous effects

	Patent (d)		VC (d)	
	Low GDPpc	High GDPpc	Low GDPpc	High GDPpc
Diff-in-Means	0.012	-0.026	0.023	-0.002
p-value	0.766	0.410	0.468	0.918
Window	1	1	1	1
N_{left}	48	63	48	63
N_{right}	76	60	76	60
N	124	123	124	123
	Patent (d)		VC (d)	
	Small scale	Large scale	Small scale	Large scale
Diff-in-Means	0.005	-0.018	0.016	0.000
p-value	0.900	0.492	0.586	1.000
Window	1	1	1	1
N_{left}	66	54	66	54
N_{right}	90	55	90	55
N	156	109	156	109
	Patent (d)		VC (d)	
	Small grant	Large grant	Small grant	Large grant
Diff-in-Means	0.004	-0.016	0.016	0.000
p-value	0.942	0.506	0.600	1.000
Window	1	1	1	1
N_{left}	63	55	63	55
N_{right}	89	55	89	55
N	152	110	152	110
	Patent (d)		VC (d)	
	Non R&D intensive	R&D intensive	Non R&D intensive	R&D intensive
Diff-in-Means	-0.001	-0.013	0.009	0.013
p-value	0.908	0.794	0.832	0.668
Window	1	1	1	1
N_{left}	66	45	66	45
N_{right}	74	62	74	62
N	140	107	140	107
	Patent (d)		VC (d)	
	Non High-tech	High-tech	Non High-tech	High-tech
Diff-in-Means	0.000	-0.003	0.001	0.016
p-value	0.986	0.912	0.820	0.704
Window	1	1	1	1
N_{left}	56	55	56	55
N_{right}	55	81	55	81
N	111	136	111	136

Notes: results obtained employing our baseline local randomization approach and splitting the sample based on the following criteria: i) program in region with above or below median GDP per capita; ii) program with above or below median euro endowment (i.e. sum of all incentives disbursed); iii) program awarding average grant below or above the median; iv) firm operating in R&D intensive vs non R&D intensive sector; v) firm operating in high-tech vs non high-tech sector.

Table A3: Alternative window around the threshold

	Patent (d)	Patents (log)	Citw patents (log)
Diff-in-Means	-0.004	-0.001	-0.005
p-value	0.724	0.882	0.764
Window	2	2	2
N_{left}	283	283	283
N_{right}	237	237	237
N	520	520	520
	VC (d)	VC deals (log)	VC amount (log)
Diff-in-Means	0.009	0.007	0.048
p-value	0.406	0.434	0.440
Window	2	2	2
N_{left}	283	283	283
N_{right}	237	237	237
N	520	520	520
	Investment	Tangibles	Intangibles
Diff-in-Means	-158.944	-137.247	-21.697
p-value	0.130	0.218	0.174
Window	2	2	2
N_{left}	118	118	118
N_{right}	155	155	155
N	273	273	273
	$\text{Revenues}_{t+1}(log)$	$\text{Assets}_{t+1}(log)$	$\text{Employees}_{t+1}(log)$
Diff-in-Means	-0.427	0.064	0.079
p-value	0.142	0.718	0.338
Window	2	2	2
N_{left}	200	145	147
N_{right}	202	188	133
N	402	333	280
	$\text{Failure}_{t+1}(d)$	$\text{Failure}_{t+2}(d)$	$\text{Subsidy}_{Post}(d)$
Diff-in-Means	-0.022	-0.025	0.119
p-value	0.072	0.188	0.000
Window	2	2	2
N_{left}	276	277	283
N_{right}	231	232	237
N	507	509	520

Notes: results obtained employing the regression-discontinuity local randomization approach restricting the window around the threshold to [-2,2] (Cattaneo et al., 2015). Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Dependent variables are demeaned to account for competition fixed effects.

Table A4: Discarding competitions where firms have scores distant from the threshold

	Patent (d)	Patents (log)	Citw patents (log)
Diff-in-Means	-0.018	-0.021	-0.029
p-value	0.498	0.562	0.536
Window	1	1	1
N_{left}	83	83	83
N_{right}	125	125	125
N	208	208	208
	VC (d)	VC deals (log)	VC amount (log)
Diff-in-Means	0.009	0.012	0.070
p-value	0.442	0.376	0.426
Window	1	1	1
N_{left}	83	83	83
N_{right}	125	125	125
N	208	208	208
	Investment	Tangibles	Intangibles
Diff-in-Means	-6.887	4.240	-11.127
p-value	0.882	0.912	0.602
Window	1	1	1
N_{left}	45	45	45
N_{right}	79	79	79
N	124	124	124
	Revenues$_{t+1}(log)$	Assets$_{t+1}(log)$	Employees$_{t+1}(log)$
Diff-in-Means	-0.227	0.115	-0.157
p-value	0.702	0.652	0.346
Window	1	1	1
N_{left}	60	60	36
N_{right}	109	98	74
N	169	158	110
	Failure$_{t+1}(d)$	Failure$_{t+2}(d)$	Subsidy$_{Post}(d)$
Diff-in-Means	-0.023	0.008	0.124
p-value	0.176	0.798	0.020
Window	1	1	1
N_{left}	81	81	82
N_{right}	121	122	123
N	202	203	205

Notes: results obtained employing the regression-discontinuity local randomization approach restricting the window around the threshold to [-1,1] (Cattaneo et al., 2015). Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Specifications are identical to the ones in the main text except for discarding competitions in which the percentage distance in scores between the first losing firm and the last winning firm is larger than 10%. Dependent variables are demeaned to account for competition fixed effects.

Table A5: Discarding competitions where success rate is high

	Patent (d)	Patents (log)	Citw patents (log)
Diff-in-Means	-0.004	-0.004	-0.011
p-value	0.850	0.922	0.810
Window	1	1	1
N_{left}	93	93	93
N_{right}	111	111	111
N	204	204	204
	VC (d)	VC deals (log)	VC amount (log)
Diff-in-Means	0.012	0.014	0.082
p-value	0.608	0.398	0.554
Window	1	1	1
N_{left}	93	93	93
N_{right}	111	111	111
N	204	204	204
	Investment	Tangibles	Intangibles
Diff-in-Means	-21.179	4.717	-25.896
p-value	0.638	0.860	0.206
Window	1	1	1
N_{left}	44	44	44
N_{right}	66	66	66
N	110	110	110
	$\text{Revenues}_{t+1}(log)$	$\text{Assets}_{t+1}(log)$	$\text{Employees}_{t+1}(log)$
Diff-in-Means	0.032	0.064	-0.278
p-value	0.962	0.800	0.058
Window	1	1	1
N_{left}	67	53	45
N_{right}	95	83	69
N	162	136	114
	$\text{Failure}_{t+1}(d)$	$\text{Failure}_{t+2}(d)$	$\text{Subsidy}_{Post}(d)$
Diff-in-Means	-0.062	-0.043	0.103
p-value	0.000	0.198	0.032
Window	1	1	1
N_{left}	91	91	91
N_{right}	108	108	108
N	199	199	199

Notes: results obtained employing the regression-discontinuity local randomization approach restricting the window around the threshold to [-1,1] (Cattaneo et al., 2015). Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Specifications are identical to the ones in the main text except for discarding competitions in which the success rate (i.e. i.e. the number of awardees over the number of applicants) is higher than 50%. Dependent variables are demeaned to account for competition fixed effects.

Table A6: Donut hole

	Patent (d)	Patents (log)	Citw patents (log)
Diff-in-Means	-0.012	-0.008	-0.008
p-value	0.572	0.658	0.658
Window	2	2	2
N_{left}	163	163	163
N_{right}	92	92	92
N	255	255	255
	VC (d)	VC deals (log)	VC amount (log)
Diff-in-Means	0.012	0.005	0.051
p-value	0.446	0.642	0.588
Window	2	2	2
N_{left}	163	163	163
N_{right}	92	92	92
N	255	255	255
	Investment	Tangibles	Intangibles
Diff-in-Means	-218.417	-199.836	-18.581
p-value	0.288	0.336	0.420
Window	2	2	2
N_{left}	66	66	66
N_{right}	68	68	68
N	134	134	134
	Revenues$_{t+1}(log)$	Assets$_{t+1}(log)$	Employees$_{t+1}(log)$
Diff-in-Means	-0.549	0.004	0.179
p-value	0.040	0.922	0.084
Window	2	2	2
N_{left}	115	77	89
N_{right}	80	78	50
N	195	155	139
	Failure$_{t+1}(d)$	Failure$_{t+2}(d)$	Subsidy$_{post}(d)$
Diff-in-Means	0.012	0.004	0.119
p-value	0.422	0.880	0.008
Window	2	2	2
N_{left}	161	161	118
N_{right}	90	90	142
N	251	251	260

Notes: results obtained employing the regression-discontinuity local randomization approach restricting the window around the threshold to [-2,2] but excluding those firms ranked immediately around the threshold (i.e. [-1,1]) (Cattaneo et al., 2015). Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Dependent variables are demeaned to account for competition fixed effects.

Table A7: Non-parametric RDD estimates

	Patent (d)	Patents (log)	Citw patents (log)
RD Estimate	-0.010	-0.009	-0.030
	[0.018]	[0.024]	[0.023]
BW	6.1	5.9	7.1
Eff. Number of obs (left)	480	448	518
Eff. Number of obs (right)	506	444	556
Robust *p-value*	0.526	0.637	0.163
	VC (d)	VC deals (log)	VC amount (log)
RD Estimate	0.009	0.009	0.065
	[0.009]	[0.009]	[0.058]
BW	5.9	5.7	5.7
Eff. Number of obs (left)	448	448	448
Eff. Number of obs (right)	444	444	444
Robust *p-value*	0.404	0.442	0.372
	Investment	Tangibles	Intangibles
RD Estimate	-229.239	-156.702	-59.719
	[108.043]	[77.899]	[27.325]
BW	6.7	5.5	7.1
Eff. Number of obs (left)	231	218	249
Eff. Number of obs (right)	308	281	338
Robust *p-value*	0.077	0.102	0.089
	Revenues$_{t+1}$	Assets$_{t+1}$	Employees$_{t+1}$
RD Estimate	-0.854	-0.002	-0.243
	[0.483]	[0.260]	[0.227]
BW	4.5	4.8	3.3
Eff. Number of obs (left)	296	230	193
Eff. Number of obs (right)	309	289	172
Robust *p-value*	0.173	0.875	0.225
	Failure$_{t+1}$	Failure$_{t+2}$	Subsidy$_{Post}$
RD Estimate	-0.113	-0.106	0.188
	[0.034]	[0.043]	[0.047]
BW	3.5	3.8	4.8
Eff. Number of obs (left)	333	334	374
Eff. Number of obs (right)	286	287	350
Robust *p-value*	0.001	0.006	0.001

Notes: results obtained employing local polynomial RD estimators with automated bandwidth selection developed by Calonico et al. (2014b). Models are estimated with `rdrobust` (Calonico et al., 2014a). Specifications employ a mean-squared error (MSE) optimal bandwidth that varies for each outcome. All models include a linear adjustment of the running variable on both sides of the threshold, competition fixed effects and use a triangular kernel. Standard errors are clustered at the competition level.

Table A8: Staggered DID estimates

	(1) Patent (d)	(2) Patents (log)	(3) Citw patents (log)
ATT	-0.025**	-0.033**	-0.032**
	(0.012)	(0.013)	(0.014)
N	3744	3744	3744
	VC (d)	VC deals (log)	VC amount (log)
ATT	0.006	0.004	0.023
	(0.006)	(0.005)	(0.038)
N	3744	3744	3744
	Assets	Tangibles	Revenues
ATT	-0.026	-0.021	0.174
	(0.106)	(0.314)	(0.303)
N	1682	1682	1798
	Employees	Failure	Sudsidy
ATT	0.074	-0.044***	0.193***
	(0.057)	(0.016)	(0.018)
N	1020	3680	3744

Notes: results obtained using the Callaway and Sant'Anna (2021) staggered DID estimator using the entire sample of applicants. Standard errors in parentheses are robust and clustered at the firm-level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Staggered DID estimates around the threshold

	(1) Patent (d)	(2) Patents (log)	(3) Citw patents (log)
ATT	0.021 (0.018)	0.029 (0.028)	0.028 (0.032)
N	530	530	530
	VC (d)	VC deals (log)	VC amount (log)
ATT	0.093 (0.091)	0.071 (0.065)	0.418 (0.399)
N	530	530	530
	Assets	Tangibles	Revenues
ATT	-0.026 (0.302)	0.383 (0.879)	-0.012 (0.717)
N	208	208	230
	Employees	Failure	Sudsidy
ATT	0.019 (0.110)	-0.075 (0.059)	0.168*** (0.058)
N	122	516	530

Notes: results obtained using the Callaway and Sant'Anna (2021) staggered DID estimator and restricting the bandwidth around the threshold to [-1;1] centered ranks. Standard errors in parentheses are robust and clustered at the firm-level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Treatment effect derivatives

	Patent (d)	Patents (log)	Citw patents (log)
Incentive	-0.023	-0.031	-0.048
	(0.027)	(0.041)	(0.042)
Rank	-0.000	0.003	0.007
	(0.005)	(0.006)	(0.007)
TED	0.008	0.008	0.006
	(0.009)	(0.009)	(0.011)
N	893	893	893
	VC (d)	VC deals (log)	VC amount (log)
Incentive	0.008	0.007	0.045
	(0.015)	(0.013)	(0.096)
Rank	0.000	0.000	0.002
	(0.001)	(0.001)	(0.008)
TED	0.001	0.001	0.003
	(0.005)	(0.004)	(0.033)
N	893	893	893
	Investment	Tangibles	Intangibles
Incentive	-234.879	-187.196	-47.683
	(214.171)	(194.508)	(37.193)
Rank	42.159	32.325	9.834
	(36.551)	(30.675)	(9.324)
TED	-23.044	-21.123	-1.921
	(30.604)	(22.236)	(15.784)
N	498	498	498
	Revenues$_{t+1}(log)$	Assets$_{t+1}(log)$	Employees$_{t+1}(log)$
Incentive	-0.421	-0.065	-0.163
	(0.642)	(0.184)	(0.120)
Rank	0.124	0.069	0.043
	(0.102)	(0.056)	(0.028)
TED	-0.040	-0.055	-0.007
	(0.113)	(0.058)	(0.030)
N	483	440	249
	Failure$_{t+1}(d)$	Failure$_{t+2}(d)$	Subsidy$_{post}(d)$
Incentive	-0.050*	-0.048	0.190***
	(0.025)	(0.035)	(0.050)
Rank	0.008	0.009	0.011
	(0.006)	(0.010)	(0.013)
TED	-0.003	-0.003	-0.021
	(0.008)	(0.010)	(0.023)
N	873	876	892

Notes: results obtained estimating the following RDD equation by means of OLS: $Y_{ic}^{Post} = \alpha + \beta Grant_{ic} + \gamma Rank_{ic} + \eta(Rank_{ic} \times Grant_{ic}) + \eta Y_{ic}^{Pre} + \delta_c + \varepsilon_{ic}$. The coefficient on interaction ($Rank_{ic} \times Grant_{ic}$) is the treatment effect derivative (TED) (Dong and Lewbel, 2015). The bandwidth around the threshold is [-5;5] centered ranks. Standard errors clustered at the competition level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Effects on applying and winning H2020 grants

	(1) $\text{Apply}_{Post}(d)$	(2) $\text{Win}_{Post}(d)$
Diff-in-Means	0.001	0.000
p-value	0.980	0.884
Window	1	1
N_{left}	120	120
N_{right}	145	145
N	265	265

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Fisherian *p-values* are obtained using 1,000 permutations. Outcome variables: (1) dummy equal to one if firm has applied to H2020 in the three years following the competition, and 0 otherwise; (2) dummy equal to one if firm has won a H2020 grant in the three years following the competition, and 0 otherwise. Dependent variables are demeaned to account for competition fixed effects.

Table A12: Additional tests for follow-on public subsidies

	(1) $\text{Subsidy}_{Post}(d)$	(2) $\text{Subsidy}_{Post}(d)$
Diff-in-Means	0.139	0.192
p-value	0.004	0.000
Window	1	1
N_{left}	102	94
N_{right}	117	103
N	219	197

Notes: results obtained employing the regression-discontinuity local randomization approach (Cattaneo et al., 2015) restricting the window around the threshold to [-1,1]. Models are estimated with `rdrandinf` (Cattaneo et al., 2016). Column (1) reports estimates from a sample that excludes firms that have failed after the competition. Column (2) reports estimates from a sample excludes firms for which the focal subsidy cannot be identified. Outcome variable: dummy equal to one if firm has has received a public subsidy in the three years following the competition, and 0 otherwise. Dependent variables are demeaned to account for competition fixed effects.

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